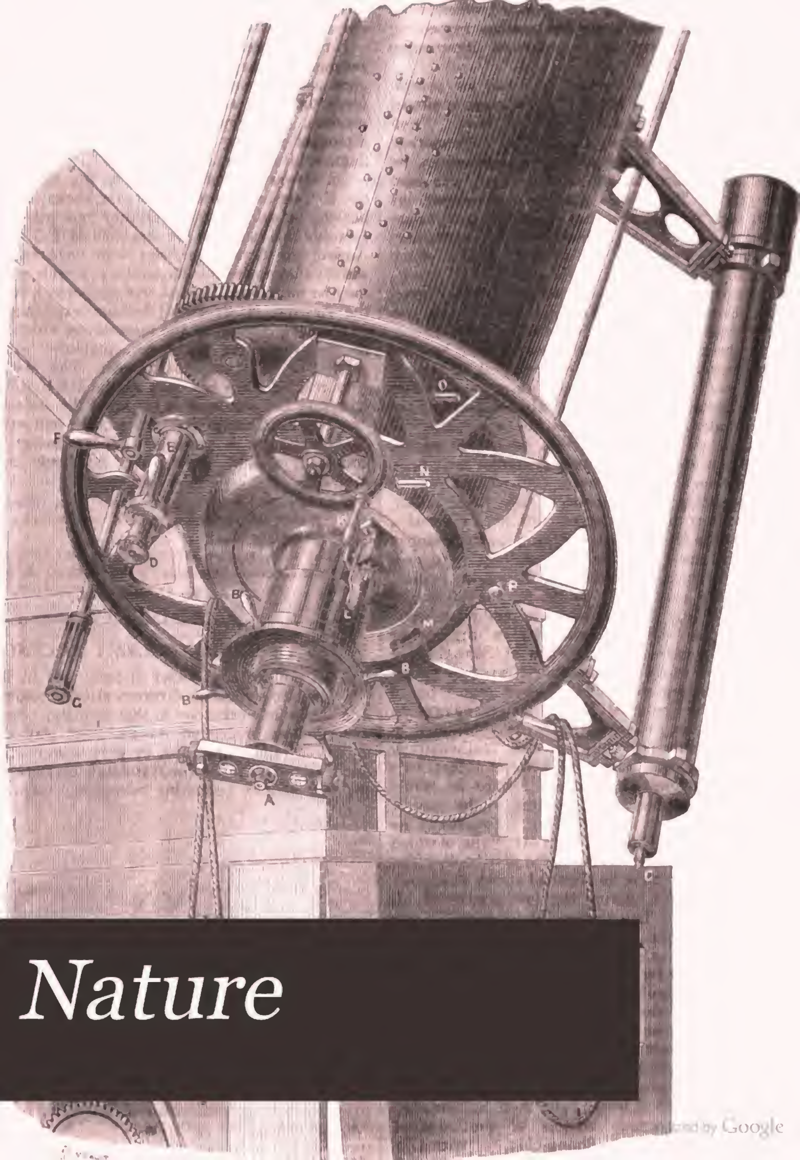
Nature



NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

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"*To the solid ground
Of Nature trusts the mind which builds for aye.*"—WORDSWORTH

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INDEX

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A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, MAY 5, 1881

EVOLUTION

Evolution, Expression, and Sensation. By John Cleland, M.D., F.R.S., Professor of Anatomy in the University of Glasgow. (Glasgow: James Maclehose, 1881.)

PROF. CLELAND is so well known as a skilled anatomist who holds some views of his own on the subject of evolution, that we are glad to welcome in this book a definite statement of what these views are. The work, moreover, is throughout very interesting. It is a collection of six essays, of which the first is on Evolution, the second on Expression, the third on Vision, the fourth on a Theory concerning the relations of Body and Mind, the fifth on Theories of the Cell, and the sixth a reprint of an address to medical students on Truth, Pathology, and the Public. These essays display a good deal of original and suggestive thinking, though not always of a kind with which we are disposed to agree.

The first essay, on the "Evolution of Organisations," expresses the view that gradual development has been the law of organic nature, but that this law has always been subservient to, or expressive of, supernatural design. After some introductory paragraphs the author clearly enough strikes his key-note thus. The fact of evolution being granted, it "may be conceived of variously, both in respect of character and cause. In its character it may be conceived of as a growth without aim, forming altogether an indefinite aggregation like the sum of the branches of a tree; or the view may be held that it is an orderly arrangement, like some vast temple in which every minaret and most fantastic ornament has got its own appointed place and harmonies, while the central tower ascends to its pre-ordained completeness." He then goes on to complain that "the name of Evolutionist has, with curious obliviousness, been assumed as a distinctive title by those who believe that the evolution is merely indefinite. . . . Had they called themselves Demolitionists, on account of their disbelief in morphological design, the name might possibly have been more expressive."

From this quotation the tone of the whole essay may be inferred. The essay, however, is written in the most temperate style, and by a man who certainly has a good right to be heard on all matters pertaining to morphology. We shall therefore offer a few remarks upon his general position as indicated by the above extract.

As regards the mere name appropriated by evolutionists of the naturalistic school, we cannot see that there is much ground for complaint. It is intended to signify belief in gradual development by natural causes as distinguished from sudden changes due to supernatural intervention. The name therefore has really no direct reference to any ulterior belief or opinion as to whether behind the natural causes producing evolution there is any supernatural design—provided only that this design is not supposed to display itself by breaking out into miracle, or interference with these natural causes. Therefore Prof. Cleland has quite as much right as Prof. Häckel, whom he rightly enough regards as a representative of the thorough-going "Demolitionists," to call himself an evolutionist, and we do not see that Häckel could properly deny him this right; they both believe in evolution as a process, much as they may differ in their views on all that lies behind that process. And the only reason why the term evolutionist has in many minds become identified with extra-theistic opinion, is simply because the theory of evolution has been for the most part developed by minds unfettered by any preconceived ideas on "the Method of Divine Government." If we had had to wait for the natural theologians to teach us the theory in question, Prof. Cleland's essay would not yet have been conceived. It nevertheless remains perfectly true that now when it has been conceived, written, and printed, he is as much an evolutionist as anybody else.

But when we pass from this question of mere terminology to the more important matter with which the essay is concerned, we are brought face to face with a question which it is useless in these columns to discuss. This question is whether the new light which science has shed on biology by the theory of descent is compatible with the older theory of design, and if so, to what extent. It is useless in these columns to discuss this question, because it is one upon which opinions differ, and may legitimately differ, through all points of the intellectual compass; there being here no general medium of knowledge to direct opinion, every man's judgment rests in whatever

position it may be brought to rest by the particular circumstances of his temperament and education. Thus it is that to Prof. Cleland it appears most reasonable to conclude that "in the evolutions of organisation there is a non-material impulse," to which virtually we may refer all difficulties that we cannot solve in our contemplation of the natural process; while to Mr. Darwin this method of relegating any special question in science to an ultimate theory of things amounts simply to "a re-statement of the question." As a question of science no explanation is furnished by this method, and whether or not in particular cases any such explanation is possible, men of science, *as such*, must never assume, as by following the method in question they would assume, that such explanation is impossible.

Therefore, so long as men of science are watchfully careful never, as Prof. Cleland phrases it, "to make final do the duty of efficient causes," so long it matters not to science what views her individual cultivators may hold on super-scientific questions. Only it is certain that a man strongly imbued with belief in final causes is apt to incur the danger of confusing them with efficient causes, and a striking instance of this fact is displayed by Prof. Cleland himself, where he says, "If evolutions are definite, or, in other words, if morphological designs exist, the necessity for explaining *all* affinity by genetic relationship disappears." This we understand to mean that wherever affinity cannot at once be seen due to "efficient causes," we are at liberty forthwith to ascribe it to "final causes." And this presents in as bad a form as ever the doctrine which for centuries has paralysed the movements of science. If a scientific man of to-day wishes to retain his belief in "morphological design," it must be as a belief in some wholly transcendental principle with which science has nothing whatever to do. Otherwise he fails in scientific method, as for instance Prof. Cleland fails where he points to "sex and symmetry" as due to design, only because he does not see how they can be due to natural selection. It makes not an atom of difference to the logical position of science whether or not the fact of sex or symmetry, or any other fact to which any writer can point, is inexplicable by natural selection. The logical position of science is that if such facts are not due to natural selection, they must be due to some other natural causes which we may reasonably hope some day to *ascertain*. And if it is asked what is the justification of this logical position to which science has been raised, the answer is supplied by the history of science itself. Let any one go through the writings of Paley, Bell, or Chalmers, and strike out all the instances of "morphological design" which he plainly sees can now be explained by natural selection, and he must be a very dull man if he continues to repose any confidence in the residue as evidence of causes other than strictly natural. In the face of so immense an analogy the burden of proof lies with the teleologists to show that any special cases to which they may point as still requiring explanation are to be regarded as inexplicable; and this burden most assuredly is not discharged by Prof. Cleland when he seeks to strike at the heart of natural selection as a natural cause by saying of heredity that in it he "can only recognise a phenomenon the origin of which demands an explanation." No doubt this explanation is demanded but is demanded at the hands of observation and experiment—not from the cloudlands of "spirit which pervades the whole."

We have thought it well to devote the main part of this review to the essay on Evolution, because the occasion seems a suitable one to raise our voice against the pernicious habit of flirting with final causes which still lingers among a certain section of scientific workers. Let any one who so may wish continue to believe in final causes; but if he does not also wish to clog the wheels of science, let him cease to throw his final causes into any gap which the roads of inquiry may present. Science, as such, requires no *deus ex machinâ*, and those of her votaries who feel that *they* require him will best consult her interests by laying the strongest possible emphasis upon the *ex*.

The most interesting of the other essays is that on Expression. In a section devoted to "Permanent Expression," it is observed that while in many respects the physical peculiarities of permanent expression admit of being explained by obvious causes, in other respects this is not so. Thus, for instance, "a massive chin is so distinctly a physiognomic representation of firmness, that an artist would in vain attempt to exhibit the resolution of a Cromwell in a face with a small and narrow jaw, or with one of those pretty chins like a bagatelle ball, not uncommon in certain localities. . . . Yet the chin has no physical function whatever, so far as I am aware."

In a section on the "Expression of the Emotions," Dr. Cleland argues in favour of an active principle which may be defined as unconscious symbolism, and certainly in the course of a few interesting and suggestive pages he makes out a strong case. It is first shown that language serves, as it were, to stereotype a number of symbolic ideas, so that, for instance, words signifying elevation come also to signify greatness, goodness, &c., while we likewise "associate impressions derived through the organs of sense with impressions from the moral world similarly pleasant or otherwise, as in the case of sweetness, bitterness, brightness, and gloom." Such associations having become firmly established, the way is prepared for their expression by gesture; so that at last "the workings of the mind are expressed by attitudes, gestures, and movements of body of a nature correlative with them." Thus "slight movements of the arms express the hugging of an idea to the bosom when nothing but what is thoroughly impersonal is thought of, and the fingers bend as if to keep a something in the hand when nothing but delightful sentiment is conceived." And similarly the gesture of sweeping away, backwards, and downwards a repulsive object from before the eyes, "is a gesture applied to the intangible and invisible; by it the cleric puts away false doctrine, and the fastidious sublimely brands a notion as vulgar."

This theory, of which many other illustrations are given, is to some extent the same as that which Mr. Darwin calls "serviceable associated habits," seeing that the principle of association is concerned in both; but as in Dr. Cleland's theory the association need not be "serviceable," and as it is concerned with an unconsciously symbolic representation of ideas, we think with him that it deserves to be considered as a distinct theory, and we can scarcely doubt that the principle with which it is

concerned has played an important part in the development of emotional expression in man.

The remaining essays, which we have no further space to consider, are likewise entertaining, and we therefore recommend the book to all who are interested in the sundry biological theories of which it treats.

GEORGE J. ROMANES

LEGGE'S "BIRDS OF CEYLON"

A History of the Birds of Ceylon. By Capt. Vincent Legge, R.A. Part III., concluding the Work. 4to. (London: Published by the Author, 1880.)

THE ornithologists are certainly active at the present time. We have just recorded the commencement of an important work on the Birds of New Guinea, we have now to notice the conclusion of an excellent volume on the Birds of Ceylon. Capt. Legge's book will, we are sure, be much valued by the numerous European residents in the coffee-districts of the island, who cannot fail to have their attention attracted by the beautiful forms of ornithic life which surround them, and as yet have had no ready means of becoming acquainted with what is known of them. Jerdon, singularly enough, did not include Ceylon in the area of his "Birds of India," and although Blyth, Layard, and Holdsworth have worked long and laboriously on the Ceylonese avifauna, their memoirs on the subject are dispersed about in various serials and other publications, which it is not possible for a coffee-planter to have at his command. A general *résumé* of the Ceylonese ornithology, with full descriptions of all the species, with excellent illustrations of the peculiar forms, and with every necessary detail required for the instruction of the local student and collector, is therefore likely to be a most acceptable piece of work to the resident in the island. At the same time Capt. Legge has produced an elaborate and scientifically exact Monograph of a local Avifauna, which will be received with welcome by naturalists of every class, and is well worthy to take its place on their shelves alongside such works as Dresser's "Birds of Europe" and Buller's "Birds of New Zealand," with which it corresponds in size and character.

As regards the general features of the Ceylonese Ornis Capt. Legge observes that the island, "although it contains none of those remarkable forms which characterise the birds of some of the Malay islands, undoubtedly possesses a rich avifauna; and, considering its geographical area (about five-sixths of Ireland), the number of species is very large. The tropical position of Ceylon, coupled with its location in the path of the monsoon winds and rains, fosters the growth of luxuriant vegetation and verdant forests, which, as a matter of course, teem with all that wonderful insect-life necessary for the sustenance of birds. Hence the large number of resident species inhabiting it; whilst the fact of its being situated at the extreme south of an immense peninsula makes it the finishing point of the stream of waders and water-birds which annually pass down the coast of India. Lastly, the prevalence of a northerly wind at the time of migration of weak-flying warblers brings these little birds in numbers to its shores."

The total number of species of birds included in Capt. Legge's work is 371, of which two have been introduced by man's agency, and about eighteen others are somewhat doubtful. The authentically determined birds of Ceylon may therefore be stated at about 350, of which forty-seven are peculiar to the island; this indicates a very large amount of individuality. The relationship of the Ceylonese Ornis, Capt. Legge tells us, is, as might have been expected from the geographical position of the island and its separation from the mainland merely by a shallow strait, "closer to that of South India than to the avifauna of any other part of the peninsula. Wallace, in his great work on the 'Distribution of Animals,' considers the island of Ceylon and the entire south of India as far north as the Deccan as forming a subdivision of the great 'Oriental Region.' It is however in the hills of the two districts, which possess the important element of a similar rainfall, where we find the nearest affinities both as regards birds and mammals; and this is exemplified by the fact of some of the members of the Brachypodidæ and Turdidæ (families well represented in both districts) being the same in the Nilghiris and in the mountains of Ceylon, while many of the Timaliidæ and Turdidæ in one region have near allies in the other.

"But though this strong similarity in the avifauna of the mountains in question, as well as their geographical characters, indicate a contemporaneous upheaval and enrichment with animal life of their surfaces, a similar connection is found between the northern parts of the island and the low country of the Carnatic.

"Here, again, we have in the fossiliferous limestones of the two regions an undoubted connection, and also an affinity in their avifaunas, which differ totally from that of the mountain-districts on either side of the straits."

In concluding our notice of this admirable volume we must not fail to call special attention to the plates which have been drawn by Mr. Keulemans, and are excellently coloured. They are devoted to the illustration of the species peculiar to Ceylon. Nor must we forget the map, which forms the frontispiece and shows the five zoo-geographical regions into which the author divides the island, besides the various localities referred to in the course of the work.

OUR BOOK SHELF

A Manual of Ancient Geography. Authorised Translation from the German of H. Kiepert, Ph.D. (London: Macmillan and Co., 1881.)

THE name of Kiepert is in itself a sufficient guarantee of the thoroughness and accuracy of a book on geography. That writer, in his "Lehrbuch der Alten Geographie"—from which the present work is abridged, though he himself describes the "Lehrbuch" as a "Werkchen"—has brought together a vast amount of well-digested information respecting ancient geography, so that the book excites the student's admiration from the grasp it displays of the many sides—geological, ethnographical, philological, historical, climatological, &c.—of that wide-reaching subject, and the discernment and critical spirit which characterise it. To the English reader the smaller work, which has been excellently translated by G. A. M., supplies a want that has long been felt. We possess no satisfactory book on the subject intermediate between primers and elaborate treatises, and the present one has all the advantage of being the condensation of a larger book, so that in reading it we feel all through that the

author draws on a wide field of knowledge. What is most needed in such a manual is that it should be clear, interesting, suggestive, and not overladen with such details as clog the memory while they make no permanent impression on the mind. In the present work the statistical form is avoided, and the dry bones of geography are clothed with such information as gives them life and colour. The vegetation and products of the different countries, their inhabitants and history, are all noticed in connection with the configuration of the ground. Thus of Italy we are told that, "besides the oak forests which covered the lower slopes, and the beeches and firs which covered the higher parts of the mountains up to 5000 feet, and far more densely in ancient times than now, a large part of the mountain region, especially of the Apennines, owing to the steep and rocky character of the ground, remains unsuited for any other purpose but cattle-feeding, and this in the higher regions is confined, as in the Alps, to the summer months. A regular interchange of cattle and sheep according to the seasons, such as is now usual, already went on in antiquity, as, for instance, between Samnium and Apulia." So too, when the volcanic system of Italy is mentioned, we are told that "the lava flowing from these volcanoes afforded in old Roman times, as it does still, the hardest material for binding together the great military roads (*silex*), while the conglomerate of tufa, consisting of the lighter masses thrown down, which was spread over the plain, was the commonest material for building, and its weather-worn surface made the most fertile soil for tillage." In a similar geological sketch of Greece, the various centres of volcanic action are noticed, and the metals which are found in different parts of the country. In respect of the history and politics of the several districts a large amount of useful information is brought together. For instance, we find a clear statement of the names by which the ancient Greeks were known at different times and by different peoples, and the history of the name Italia is discussed in the same manner. Nor are outlying nations neglected. Under the headings "The Scythians" and "The Sarmatae" we get the results of a large amount of research and discussion, and the Carthaginian province in Sicily is duly noticed. Philology again, which is now no unimportant handmaid of geography, is made to add its contribution of information; as when we are told that the names Asia and Europe "are derived from the *açu* and *irib* of the lately-deciphered Assyrian monuments, meaning east and west, and answering to the Homeric expression πρὸς ἠῶ ἠέλιόντε and πρὸς ζόφον, to the later Greek names of countries, 'Ανατολή and 'Εσπερία, to the modern *Orient* and *Occident* (borrowed from the Latin), or to the Italian *Levante* and *Ponente*." Sometimes a name is connected with the features of the ground, as where the Jordan, with its steep descent down a deep valley, is explained to mean "the down-flowing"; or where Zancle is said to be called "the Sickle" from "the form of the tongue of land which incloses the natural harbour." So, too, the student is incited to further research when he learns that Mount Atabyrion in Rhodes bears in reality the same name as Mount Tabor, and that other Greek names have undoubtedly Semitic appellations; and his appetite for history is whetted by discovering that Cappadocia is the Old Persian *Katpatuka*, and that Marsala is the mediæval Arabian name for Lilybæum. Numerous points like these are illustrated by geography, and M. Kiepert gives his readers the full benefit of them. But this is not to the neglect of the more substantial part of the subject, which is amply and clearly expounded.

H. F. TOZER

Lehrbuch der organischen qualitativen Analyse. Von Dr. Chr. Th. Barfoed. Dritte Lieferung. (Kopenhagen: Andr. Ferd. Höst und Sohn, 1881.)

DR. BARFOED'S work on organic qualitative analysis is completed by the issue of the present part. The author is to be congratulated on producing so valuable a book of reference for the laboratory worker. The present part contains a full account of the tests for the commoner alkaloids, and for a few of the more fully examined vegetable colouring matters. A general method for the examination of organic substances, whether free from, or mixed with, inorganic compounds, is also given. This general plan is not however arranged in cut-and-dry tabular form, but is rather a guide which in the hands of the experienced student will prove of much value. We can but repeat what we said in noticing the first part of this book, that every student who is desirous of obtaining a real knowledge of qualitative organic analysis, ought to possess Dr. Barfoed's work.

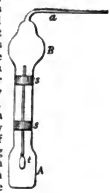
LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Hot Ice

THAT the experiments of Dr. Carnelley on hot ice have excited much interest is not to be wondered at. His statement, however, that ice could be raised to a temperature of 180° without melting was so amazing that many a one could not accept it without repeating the experiments. Soon after the first short notice of Dr. Carnelley appeared in NATURE we took up the matter, but as the method used by Dr. Carnelley seemed to us to be somewhat troublesome, we made use of quite a different method. In the axis of a glass tube A B, 16 mm. in diameter, and 56 c.m. in length was fixed a thermometer *t* by means of two strips *ss* of elastic brass sheet. One of the ends of the tube was 22 mm. in diameter, while the other end had the shape of a bulb, and was drawn out in a narrow tube, *a b*, about 50 c.m. in length. The tube was placed in an inclined position with the end *b* in a glass filled with water that was kept boiling. The bottom of the glass was covered with a layer of mercury. Next the tube was heated by a Bunsen burner; a part of the air was driven out, and, after retiring the burner, the tube was partly filled with the boiling water of the glass. The water in the tube was then boiled, a still greater part of the air escaped, and by removing the burner the tube filled itself nearly entirely. The heating and cooling were repeated three or four times, and in this manner the tube could be filled with the boiling water, not a single air-bubble being left. The end *b* of the narrow tube



was now dipped in the mercury, and by heating the tube so much of the water was driven out that the remainder filled the enlarged part A for three-fourths. The tube being now slowly cooled, the mercury rose in the tube *a b*, and it was very easy to seal the tube at *a* with the blowpipe. The water in A was now frozen, and by gently warming with the hand the ice-cylinder was loosened from the tube; by inverting the tube the molten ice was brought into the bulb B, where it was fixed by freezing. This part of the process presents some difficulty. When the heating by the hand was not stopped in time, too much of the ice was converted into water. However, by placing the bulb B in a freezing mixture of snow and salt the melting can be almost instantly arrested. The bulb of the thermometer being in this way surrounded with an ice cylinder 12 mm. in diameter, the bulb B had only to be placed in the freezing mixture to have the apparatus ready for the appliance of heat. The results of our experiments confirm those of Dr. Carnelley, inasmuch as the ice did not melt, notwithstanding the heating of the tube at A was in one instance so strong that the glass was softened and gave way to the external pressure of the air. They differ, however, as regards the temperature of the ice, which remained generally at −7°. By very strong heating

the thermometer rose to 0°, but *never exceeded that point*; when not, the bulb of the thermometer, by the volatilising of the ice, was partly laid bare. As it appears by the detailed description of Dr. Carnelley's experiments in NATURE, vol. xxiii. p. 341, that the success depends for a great part on the size of the condenser, we have made another apparatus with a condenser of half a litre; the results we may obtain therewith will be related shortly.

We have also made some experiments on naphthalene. The pressure of the naphthalene vapour at the melting-point, nearly 80°, being ± 9 mm., as was found by a preliminary proof, it was expected that it would not be very difficult to obtain and to maintain a vacuum sufficient to observe the demeanour of naphthalene under similar circumstances as ice. The apparatus we used resembles in its principal features that we made use of in experimenting on ice; alone, the condensing surface was much greater. The thermometer bulb being imbedded in a cylinder of pure naphthalene 13 mm. in diameter, the thermometer was fixed in the axis of the glass tube, and this latter drawn out. A small quantity of water being brought in the tube, the pressure was reduced by means of an ordinary air-pump to 5 mm., and the drawn-out end of the tube melted through. In another instance the tube was several times filled with carefully-dried carbonic acid and exhausted, and lastly, when the pressure had been reduced to 7 mm., sealed. To remove the remaining carbonic acid and aqueous vapour a certain quantity of caustic potash and some pieces of oxide of calcium were inclosed in the tube. In what manner the tube had been prepared, the results when heat was applied, the upper part of the tube being cooled in a freezing-mixture or simply in snow, were always the same. The thermometer rose very rapidly to about 79°, and *stayed at that point* as long as no part of the thermometer bulb was denuded of naphthalene. At the same time the naphthalene sublimed very regularly, covering the sides of the tube next to the heated part with a beautiful layer of naphthalene crystals.

C. J. E. BRUTEL DE LA RIVIÈRE
A. VAN HASSELT

Assen (Netherlands), April 14

Sound of the Aurora

THE interesting communications which have lately appeared in your periodical regarding the supposed connection between "sound" and the "aurora" (NATURE, vol. xxiii. pp. 484, 529, 556), lead me to suppose that the following notes may be considered by you and your readers worthy of record. They were copied last autumn by myself from the Strangers' or Visitors' Book at the Hotel on the Æggischorn, and bore the date July 10, 1863:—

"Visit to the Col de la Jungfrau described: On descent surrounded by thunderclouds evidently charged with electricity. At 12.15 a sound similar to that made by a boiling kettle was heard to issue from one of the alpenstocks, and very soon a similar sound issued from all the bâtons. On shaking the hands similar sounds issued from the fingers. Observing that the veil of one of the party stood upright on his hat, one of the gentlemen and one of the guides, who had experienced prickly sensations on the crown of the head, removed their hats, when their hair stood up as if under a powerful electrical machine. Whenever there was a peal of thunder all of the phenomena ceased, to be speedily renewed when the peal was over. At such times all the members of the party felt severe shocks in the parts of the body which were most affected; and one gentleman had his right arm paralysed and rendered useless for several minutes. The clouds passed away and the phenomena finally ceased at 12.30. The guides with us were Joseph Marie Claret of Chamouni, and Smith of this house, and they were as much affected by the electricity as we were. At the top of the Col the aneroid barometer stood at 18·83."

I believe the above statement, clear and pointed as it is, was signed by the names of Watson, Sowerby, and Adams.

It will be seen that other phenomena are mentioned, in addition to the sounds heard in connection with the electrical ones, which are worthy of regard. I would, among other points, draw attention to the effect produced on the arm of one of the travellers, and should be glad to know from any of your correspondents whether they have met with other like results of electrical interference with the actions of muscles in mountain travelling.

I may mention in passing that in the same Visitors' Book at the Æggischorn Inn I found notes to the effect that the Jungfrau and the Aletschorn were first ascended by a lady in August of the year 1863.

JOHN W. OGLE

30, Cavendish Square, W.

A PRESUMPTION as to the true character of the sound of the aurora is perhaps offered by the fact that to many persons a flash of lightning is accompanied by a distinct *whishing* sound. As this is simultaneous with the flash, and therefore evidently subjective, it seems to offer evidence merely of the close connection existing between the senses of seeing and hearing.

April 30 E. HUBBARD

Symbolical Logic

IN my recent letter on Symbolical Logic (see NATURE, vol. xxiii. p. 578) I said that Prof. Peirce's symbol of inclusion, as defined by him in his "Logic of Relatives," was equivalent to the words "is not greater than." This however is not quite correct; for though Prof. Peirce speaks of this symbol as equivalent to the words "is as small as," he also speaks of it as denoting "inclusion," and his illustration $f \prec m$ may be read, *The class f is included in the class m.* In my notation the analogous composite symbol $f : m$ may be read, *The statement f implies the statement m.* If for f in my notation we read *He belongs to the class f*, and for m we read *He belongs to the class m*, then my $f : m$ will coincide in meaning with Prof. Peirce's $f \prec m$; but this does not alter the fact that my f differs in meaning from his f, that my : differs from his $\prec$, and my m from his m. Mr. Venn, in his recent paper in the *Proceedings* of the Cambridge Philosophical Society, speaks of these distinguishing features of my method as unimportant, and he regards my definitions of my elementary symbols as "arbitrary restrictions of the full generality of our symbolic language." But Mr. Venn overlooks the fact that all accurate definitions are more or less arbitrary restrictions of language, and he also seems to me, in this particular case, to mistake *vagueness* for *generality*. Philosophical investigations that begin with *Let* x = *anything* commonly end with x = *anything*, a result which, whatever may be thought of its generality, does not add much to our knowledge.

HUGH MCCOLL

73, Rue Siblequin, Boulogne-sur-Mer, April 26

The Formation of Cumuli

THIS afternoon the air to a great distance above the surface of the earth has been filled with fluttering dry leaves. For some weeks no rain has fallen in this vicinity, and a cold northerly wind has prevailed. To day, for the first time during the continuance of this cold and rather clear weather, the hill-sides having a southern exposure have begun to be sufficiently warmed to cause upward currents of air along their surface. The effect has been curious: piles of cumuli have formed persistently in certain quarters of the sky, and eddying masses of leaves caught up along the hill-sides have been falling apparently from the under surface of the dense masses of cloud. My attention was first caught by the fall of chestnut and other varieties of leaves, which must have traversed a long distance, as there are no trees of the sort near at hand in the direction from which the wind was blowing at the time. Whilst walking near an elevated ridge of ground an hour later it was my fortune to catch sight of a thick mass of leaves rushing directly up its side and pouring apparently into the bosom of a dark cloud which overhung the hill. This cloud remained almost stationary, although there appeared to be a lively breeze along its under surface, the leaves darting forward very swiftly. The entire phenomenon was quite interesting as affording an illustration of the method of formation of clouds of the variety named.

M. A. VEEDER

Lyons, New York, March 20

"The Oldest Ocean Post Office"

IN NATURE, vol. xxiii. p. 254, just received here, it is stated that in Magellan Straits there has been for some years past, chained to a rock there, a barrel from which passing ships take letters for the direction they are going in, leaving others for the opposite quarter; it is added that up to the present no abuse of the privileges of this primitive post-office has been reported.

In Victor Hugo's romance, published in English, some fifteen years ago, as "The Toilers of the Sea," a tale of the first quarter of this century, he makes one of his sailors tell another about such an "Ocean Post Office" at Cape Famine in Magellan Straits. Cape Famine was the scene of an early settlement which—in that bleak place—was entirely dependent on the outside world for means of subsistence, and when these, on one occasion, failed, through delay in the arrival of a ship, the colonists died; the circumstance giving the place its present name. The existing colony in Magellan Straits, about twenty miles from Cape Famine, is a Chilian settlement called Punta Arenas, or Sandy Point. Its trade is in guanaco and emu skins, brought to it by the Patagonian Indians. The colonists do a little also in agriculture, coal-mining, and gold washing.

Sailing-ships invariably now go round Cape Horn; the narrowness of the Straits at some points, their strong currents, and the alternating fogs and wild winds that prevail making the passage a very risky one for such vessels as are now employed in ocean navigation. There are however two steamship companies, a Liverpool and a Hamburg one, whose vessels pass through the Straits, and, touching at Sandy Point, insure nearly weekly communication between that place and the east and west. But the oldest commanders of these steamers know of no such institution as an ocean post office in Magellan Straits. Indeed at Sandy Point there have long been the usual facilities for postage, and the plan of the barrel is therefore not needed in the Straits.

That plan is adopted in "Post Office Bay," in one of the uninhabited Galapagos Islands, and possibly that fact, or the adoption of some similar device in the Straits at a time, long ago, when there was no settlement there, and of which a tradition may still remain, may have suggested Victor Hugo's narrative, which again may have been the origin of the paragraph quoted in your columns. ARCH. ROXBURGH

Valparaiso, March 23

JOHN DUNCAN, THE ALFORD WEAVER-BOTANIST

THE subscriptions spontaneously sent for the purpose of forming a fund to raise this deserving old botanist above the need of accepting parochial relief have now reached the handsome sum of 322*l.* 19*s.* 10*d.* Of this sum there were sent through NATURE 73*l.* 6*s.* 9*d.*; to Mr. Jolly, Inverness, 223*l.* 10*s.* 10*d.*; to Alford direct, 90*l.* 19*s.* 7*d.*; through the *Free Press*, Aberdeen, 12*l.* 9*s.*; and through the *People's Journal*, Aberdeen, 1*l.* The names of the subscribers to the NATURE Fund have already been published here. The list of the others is much too long to be inserted, the very length showing the wide-spread interest excited by the case. It includes above 300 separate subscriptions, ranging from 30*l.* from Mrs. Alfred Morrison of London, to 1*s.* from a working man and from a child botanist, and the names of many of our most eminent scientists. Some interesting details might be given of the warm sympathy expressed in the case; the wording of special subscriptions, some large sums from nameless donors, and the plans adopted by different persons in different parts of the country interested in the old botanist, to gain the help of the generous: but want of space prevents these being entered on. One item deserves mention, namely, the interest manifested in the case by several scientific societies already mentioned in NATURE; though it must be added that the absence of the names of some societies especially devoted to Botany, and of the University of Aberdeen, which received the gift of John's herbarium, is not a little strange. The action of these generous societies is no doubt a not unimportant means of assisting scientific inquiry and "endowing research." As already stated, Her Majesty was graciously pleased to present a gift of 10*l.*

A trust-deed has now been formally executed and signed by John Duncan, disposing of the money thus subscribed, and his books and other possessions, during his life and after death, vesting all powers, with certain discretionary liberties, in seven trustees, provision being made for their permanent continuance. These trustees consist of Mr. William Jolly, H.M. Inspector of Schools, Inverness, whose sketch of Duncan in *Good Words* in 1878 first drew attention to the old man, and whose recent appeal in his behalf has resulted in the present ample provision for his comfort, Mr. Farquharson of Haughton, near Alford, chairman of the School Board of Alford, the Rev. Mr. Gillan, and the Rev. Mr. Brander, the Established and Free Church clergymen of Alford, and three other gentlemen personally interested in Duncan; the permanent trustees of his property, if any remain, to be the ministers of the Established and Free Churches of Alford and the Chairman of the School Board of Alford, with power to add to their number, so that the full number shall never be under five. It is provided, that he agrees to the provision made for him during his life, which is ample, and that whatever sum remains at his death shall be vested in safe securities and the interest arising therefrom devoted to the foundation of scholarships or prizes for the promotion of science, especially Botany, in schools in certain parishes named, in the Vale of Alford; his books, which are numerous and good, especially those on Botany, being gifted to the parish of Alford, for the same object. Meantime the greater part of the money in hand will be invested at good interest.

It must be gratifying to the subscribers to know that not only will the comfort of the old botanist be secured for the remainder of his life, but that any surplus, which is almost certain to be considerable, will promote for all time the pursuit of those studies that have made Duncan famous, among the children, male and female, of the district in which he has achieved his own scientific work. The worthy man, now in his eighty-seventh year, is frail, and the past severe winter has been hard upon him, as upon all aged people; but he may, and probably will, survive for some years to come. His gratification and gratitude at the kindness recently shown him are expressed with childlike depth and sincerity. Any remaining subscriptions offered should be sent without delay to Mr. Jolly, Inverness, in order that all requisite arrangements may be completed.

ELECTRIC LIGHTING

I.

ONE of the greatest experiments ever made in street illumination by means of the electric light was commenced on March 31 in the City of London. The enterprise of the City authorities in this direction is the more commendable, inasmuch as they had previously tried electric lighting on the Holborn Viaduct and in Billingsgate Market with very poor success. In fact, in these two instances the experiment was a decided failure.

Now however, gaining experience from the advances that have been made in other directions, and especially from the great success attending the illumination of several railway stations, the City authorities determined to divide the City into three districts for the purpose of experiment.

1. The London Bridge district: embracing London Bridge, Adelaide Place, King William Street, the front of the Royal Exchange and Mansion House, the Poultry, a part of Cheapside as far as King Street, the upper part of Queen Street, King Street, and the Guildhall Yard.

2. The Blackfriars Bridge district: embracing Blackfriars Bridge, New Bridge Street, Ludgate Circus, Ludgate Hill, St. Paul's Churchyard (north side), and the remaining portion of Cheapside beyond King Street.

3. The third district has not been lit up, and therefore we need not refer to it.

The first district is being lit up by Messrs. Siemens Brothers at a total cost of 3725*l.* for the twelve months, replacing 138 gas-lamps. The other district has been lit up by the Anglo-American Electric Light Company, on the Brush system, for a total cost of 1410*l.*, replacing 150 gas-lamps.

Before drawing any comparison between these two systems it will be just as well to describe how each has been carried out.

The Messrs. Siemens have applied to their (No. 1) district six powerful lights, fixed at a height of 80 feet from the street level upon tall latticed iron masts similar to those which are used on many railways for signalling purposes. They have also twenty-eight smaller lights, carried upon special iron posts, considerably higher than the ordinary lamp-posts, being 20 feet from the pavement. The powerful lights, fixed high up in air, are used more for the illumination of open spaces, and this system—a special feature of Dr. Siemens'—has been very extensively adopted at the Albert Docks, Blackpool, and other places. The central station of the Siemens system is in Old Swan Lane. Here three engines, supplied by Messrs. Marshall of Gainsborough, of their semi-portable type, and of 10-horse power each, fitted with an admirable automatic expansion gear (Hartnell's) specially applicable to engines used for electric lighting purposes, are fixed. Two of these engines are always at work during the time of lighting, but one is kept in reserve, ready at a moment's notice to replace either that may fail. These engines, by means of beltings and a counter-shaft, apply their power to various dynamo-machines of the well-known Siemens type. Each of the high lights is worked by a separate and distinct dynamo-machine, with which it is connected by separate conducting-wires. The wires throughout the whole of the City are of thick copper, very well insulated, and laid under ground after the customary manner of laying down wires for telegraphic purposes. The smaller lights are worked by alternating currents on the system with which we are familiar on the Thames Embankment applied to the Jablochkoff candle. In Swan Lane there are two alternating current machines each working two circuits of seven lights each, the lamps being arranged so that consecutive lamps are not in the same circuit, and by that means, if any accident should occur to one set of lamps it would only extinguish one out of two lights in a street, and not throw the whole district into darkness, which would be the case if all the lights were worked on one circuit. The field-magnets in all the large dynamo-machines are excited by a similar dynamo-machine, while the magnets of the alternating current machines are fed by currents from a smaller continuous dynamo-machine. Each large machine absorbs between 4- and 5-horse power, but the alternating machines require much less. The furthest light from the generating centre —Old Swan Lane—is that in front of the Guildhall, which is nearly three quarters of a mile distant, involving a length of wire of 2500 yards (a return wire being used), whose total resistance does not exceed one Siemens' unit. The illuminating power of the high lights is estimated to be 6000 candles, but it is well known that this estimate of the illuminating power of an electric light is a very wild one. There is no doubt that the lights are very powerful, and a stream of brilliant illumination is thrown all over such an area as that in front of the Royal Exchange. The Siemens' lamps burn for eighteen consecutive hours, owing to the size and length of the carbons used. They are provided with reflectors which throw a bright cone of light down in a very peculiar way, giving to this experiment a very marked feature.

There is no doubt whatever that where it is required to illuminate a large area this is very much more efficiently and economically done by using one single powerful light high up in air, than by distributing several smaller lights over the surface. In the former case the light is more evenly, uniformly, and perfectly diffused, in fact it acquires the character of bright moonlight, while in the latter case the light is distributed in patches of intensity and darkness over the whole space.

When streets are dealt with the conditions are different, and it is here quite easy to show that economy and efficiency are provided for by properly distributing smaller lights along the street. The Messrs. Siemens have set to work to solve this problem in a scientific way, and Mr. Alexander Siemens, under whose control and management the system has been carried out, can show mathematically that to distribute light uniformly and properly a certain definite proportion should exist between the height of the posts and the distance at which they are apart. That this has been carried out is abundantly evident by the very even way in which light is distributed along Queen Street, King William Street, and Cheapside. Indeed it is difficult to see any break in the intensity of the light along the route—a proof that the practical application of the law very nearly approaches its theoretical limit. The theoretical point to be aimed at is that the height of the poles should be to half the distance between them as 1 is to the square root of 2. This has not been absolutely obtained, but a very close approach to it. The small lights only give an illuminating power, according to Messrs. Siemens, of 300 candles, and this probably is well within the mark. Comparisons between lights of low intensity are very easily and accurately measured; it is only when a power equal to thousands of candles is arrived at that the failure of comparison with a standard candle becomes evident.

The high lights have not been burning uniformly with that steadiness that success demands. Instances of failure are not numerous, though they have been frequent. The smaller lights, on the other hand, have worked more uniformly, and have given considerable satisfaction. The strong shadows thrown by the high lights have a very weird-like effect in certain positions, and the vibration of the lamp gives to the shadow of the pole that supports it an unsteadiness that has led the unwary to imagine in many instances that the pole itself was shaking. Could the shadows be removed from the effects of these high lights the effect would be very fine; as it is they detract enormously from the beauty of the lamps. The effect of the high lights to those standing below is excessively pleasing, and doubtless in warmer weather will be more highly appreciated than it has been during the past week. It is when crossing streets, and especially when crossing such a busy thoroughfare as that in front of the Mansion House that these lights show their efficiency to advantage. It is quite amusing to see how the *gamins* of London have taken advantage of the combination of electric lighting and asphalte road to convert the whole City into a gigantic skating-rink. Hundreds of boys are to be seen every night disporting themselves on their roller-skates.

(*To be continued.*)

WEATHER WARNINGS

IN a lecture on Solar Physics delivered at South Kensington on Friday last Prof. Balfour Stewart stated that he believed one great cause of weather changes to be solar variability in which we have periods of short length, as well as others extending over many years.

These weather changes, it is sufficiently well known, are propagated from west to east after they have once appeared.

Again there are variations in the diurnal declination range which may be said to constitute magnetic weather.

These are also caused by solar variability, and it is suspected that they are likewise propagated from west to east, although more quickly than the well-understood changes of meteorological weather.

It would thus appear to be at least possible that British magnetical weather of to-day may be followed by corresponding meteorological weather five or six days hence.

Prof. Stewart has made a preliminary trial, which induces him to think that this is the case, and that it

may ultimately be possible to forecast British meteorological weather by means of magnetic weather some five or six days previous to it.

BAROMETRIC GRADIENT AND WIND

I HAVE often felt surprised that the superiority in force of northerly and easterly as compared with southerly and westerly winds accompanying any given amount of barometric gradient has, at least until recently,[1] excited but little attention, seeing that the superiority in question is almost sufficient to suggest itself to any student of daily weather-charts. The comparison of anemometric records for the elucidation of this subject can only be imperfectly made, owing to the fact that there are very few situations at which an instrument can be erected which shall have a really equal exposure to winds from all points of the compass; neither is it possible, as I think, in comparing anemographic records from stations at our different coasts to eliminate the various effects of local inequalities of the earth's surface upon the force of the winds. There are two methods which can be employed in the investigation of this question, which seem to yield reliable, though necessarily somewhat rough and imperfect, results. One of these is to examine separately the anemographs of our imperfectly, but moderately well-exposed, inland stations, in relation to various values of barometric gradient in different directions. The other method is to discuss the means of estimated wind forces in relation to amount and direction of gradient for a large number of years and at a large number of stations. I have hitherto but partially and tentatively employed these two methods, but the results arrived at may possibly be of interest to some readers of NATURE. The mean wind velocities at Stonyhurst Observatory, obtained by me from the hourly readings published by the Meteorological Committee for the years 1874 to 1876 inclusive, for different moderate amounts of atmospheric gradient are as follows:—

Gradient per fifteen nautical miles.	Mean velocity in miles per hour of winds from points between S.S.E. and N.W. (inclusive).	Mean velocity in miles per hour of winds from points between N.N.W. and S.E. (inclusive).
·006	4·31	5·53
·009	5·90	6·82
·012	7·79	9·63
·015	11·09	13·97
·018	13·03	15·29

The mean wind velocities at Kew Observatory for the same period for similar gradients are as follows:—

Gradient per fifteen nautical miles.	Mean velocity in miles per hour of winds from points between S.S.E. and N.W. (inclusive).	Mean velocity in miles per hour of winds from points between N.N.W. and S.E. (inclusive).
·006	4·14	6·88
·009	6·41	8·63
·012	8·37	10·93
·015	11·21	14·27
·018	13·56	16·98

This shows that for any given (moderate) gradient winds from north and east points are stronger than those from south and west points at these stations. The second method, in which the estimated wind-forces have been employed, has been tried by me in the cases of twelve of our English stations for periods varying from ten to three years. The stations examined have been Shields, York, Nottingham, Liverpool, Hurst, Scilly, Dover, London, Oxford, Cambridge, Yarmouth, and Jersey. At all these stations, excepting Liverpool and Jersey, with very low gradients (viz. from ·001 to ·005 inch for fifteen miles), mean estimated wind forces from points between north-north-west and south-east, inclusive, have been higher than those from points between south-south-east and north-west inclusive. With the higher gradients we necessarily find results opposed to this in the cases of stations having a good exposure on the west or south and a bad exposure on the north or east, just as, on the other hand, we find the result above mentioned unduly heightened at stations which have only a good east or north exposure. If however we take stations whose exposure, though not unexceptionable, seems tolerably fair, we find that with somewhat steep as well as with low gradients, north and east winds accompanying any given amount of gradient have a higher estimation than south and west winds accompanying the same. The following table shows results at which I have arrived from an examination of the reports from three stations, viz. the two inland stations of Nottingham and London and the one sea station of St. Mary's, Scilly, which last, while very well exposed to all winds, is perhaps most perfectly so to those from the Atlantic.

	Gradient in inches per fifteen nautical miles.	Mean estimated force of equatorial winds (Beaufort scale).	Corresponding approximate velocity (miles per hour).	Mean estimated force of polar winds (Beaufort scale).	Corresponding approximate velocity (miles per hour).
Nottingham	·001 to ·005	0·43	5·2	1·06	8·3
	·005 to ·010	1·88	12·4	2·26	14·3
	·010 to ·015	2·99	17·9	4·06	23·3
	·015 to ·020	3·64	21·2	4·61	26·0
	·020 to ·025	4·41	25·0	5·35	29·9
London	·001 to ·005	0·91	7·5	1·31	9·5
	·005 to ·010	1·45	10·2	2·00	13·0
	·010 to ·015	2·24	14·2	2·93	17·6
	·015 to ·020	3·01	18·0	4·18	23·9
	·020 to ·025	3·64	21·2	4·85	27·2
Scilly	·001 to ·005	2·42	15·1	2·49	15·4
	·005 to ·010	4·24	24·2	4·86	27·3
	·010 to ·015	5·45	30·5	5·68	31·7
	·015 to ·020	6·40	36·4	6·49	36·9

The suggestion which I offer in explanation of this difference of force in the two classes of winds is made with some diffidence, since it involves a hydrodynamical question, the solution of which is somewhat difficult. Since the atmosphere is of greatest density near the poles, while barometric pressure is less near the poles than over the tropics, the pole-ward, and, under the effects of the earth's rotation, eastward movements of the atmosphere, at any given considerable altitude above the earth's surface, must necessarily greatly exceed the corresponding movements at the surface of the earth. "The planes of equal pressure receive," in short, "an ellipsoidal form, the major axis of which is perpendicular to the axis of the earth."[1] Thus the polar areas of low pressure must be far more permanent and far better marked in the upper than in the lower regions of the atmosphere; consequently gradients for westerly winds when occurring at the earth's surface must commonly extend into the higher regions of the atmosphere; while gradients for easterly winds must, on the contrary, be usually accompanied by gradients for westerly winds at no great distance above them. Observations of the movements of the upper clouds, and also of the winds experienced at the summits

[1] Sprung, "Studien über den Wind und seine Beziehungen zum Luftdruck," ii. p. 6.

[1] Hann, "Zeitschrift der österreich. Ges. für Meteorologie," vol. xiv. p. 35.

of high mountains, fully establish this fact. Observations of the upper clouds further indicate that when a cyclonic disturbance travels eastward in our latitudes, the passage of its centre is usually accompanied (or, more strictly, followed) only by a temporary backing and subsequent veering of the westerly upper-currents, showing that where we have circular isobars at the earth's surface, we should find in the region of the cirrus merely a loop or bend in the isobars for that altitude, could such isobars be drawn. Could we in short have a weather-chart confined to the region of cirrus, we should see in it, in lieu of a deep cyclone, a shallow "secondary" travelling round a portion of the great polar area of depression.

It is true that north-easterly winds may thus be subject to more retardation due to friction at their upper surface than south-westerly winds. But in a fluid like the atmosphere the whole effect of this retardation must be conceived as almost insignificant.

The question, then, that I would ask is this:—May not the fact that any given gradient for an east wind is wholly contributed by the strata of atmosphere near the earth's surface, while a similar gradient for a west wind is contributed by the whole mass of atmosphere overhead, be imagined, consistently with what we know of the mechanics of the atmospheric currents, to give a greater force of wind in the former than in the latter case, at the surface of the earth?

There is one other point to which I may be here permitted to call attention, though it relates to language alone. I have employed above, consistently with common usage, the expression "gradient for" a particular wind; but this expression appears liable to the objection that it involves a hypothesis, and one which is moreover not in accordance with fact. "A gradient for a south-west wind" signifies a distribution of pressure in which isobars lie south-west and north-east, and in which the lowest pressure is in the north-west and the highest in the south-east. But it is only in the higher latitudes, and on a level surface such as the sea, that this distribution is actually accompanied by a south-west wind. In inland localities, even as far north as the latitudes of the British Isles, it is accompanied by a wind between south-south-west and south; in lower latitudes by a wind still more from the higher to the lower pressures, and finally at the equator such a distribution of pressure would be accompanied by a south-east wind. Further, the expression leads to the needless ignoring of the more local deflections of the winds produced by irregularities of the earth's surface. Would not the expression "north-west*ward*" gradient, simply indicating that barometric pressure decreases most in a north-westward direction, be more correct and equally intelligible? Such a gradient would be one for winds between south-west and south in our northern latitudes, for winds between north-east and east in corresponding southern latitudes, and for winds from the intermediate points over intermediate portions of the globe. "North-west*ward*," "north*ward*," and "north-east*ward*," &c., gradients, are moreover slightly shorter expressions than gradients "for south-west," "for west," "for north-west winds," &c.

W. CLEMENT LEY

SCIENCE IN CHINA[1]

I.

THE Department for the Translation of Foreign Books at the Kiangnan Arsenal, Shanghai, which has for its object the translation and publication of books relating to the arts and sciences of the West, was established towards the close of the year 1869, mainly through the instrumentality of Messrs. Hsü and Hwa, natives of Wuseih, and who at that time were on the staff of officials at the Kiangnan Arsenal. The causes which led to the commencement of this important undertaking are, however, traceable to a much earlier date. In fact, to find a suitable starting-point for its history, it is necessary to go back to the earlier portion of the lives of these two Chinese gentlemen.

Wuseih is an important city on the borders of the Ta Hu, or Great Lake, in the province of Kiangnan, and has long been noted for its industrial pursuits, as well as the energy and enterprise of its inhabitants, many of whom have emigrated to Japan at various times. It was in this busy place that a little *coterie* of intelligent scholars was formed, all deploring the hollow and unsatisfying nature of the ordinary routine of Chinese studies. They determined to push their investigations in a more useful and promising field by endeavouring to become acquainted with the great laws of nature, and to gather as much information as they possibly could respecting the various branches of science and art.

Without organising themselves into a society, these aspirants for intellectual light used to have occasional meetings of an informal kind for mutual improvement, each person explaining any new facts or ideas he had acquired. The works of the early Jesuit fathers on mathematics, astronomy, and kindred subjects were carefully read, as well as original native works. But at last, during a visit to Shanghai, they found a valuable prize in Dr. Hobson's translation of a treatise on Natural Philosophy, published at the London Mission Hospital in Canton in the year 1855. This book, though of a very elementary character, was like the dawn of a new era upon their minds, enabling them to leap at one bound across the two centuries that had elapsed since the Jesuit fathers commenced the task of the intellectual enlightenment of China, and bringing them face to face with the results of some of the great modern discoveries. Apparatus was extemporised at their homes to perform the various experiments described in its pages, and every new theory or law was put to the test as far as their limited means would permit. Frequent papers were written and circulated from one to another, while queries were continually started by individuals asking for more information on difficult subjects. A pile of such manuscripts accumulated in the house of Mr. Hsü, who, with his son, formed a sort of centre for this little oasis in the midst of a vast desert of ignorance and superstition. Unfortunately, however, these manuscripts were all destroyed when the Taiping rebels captured the city, and the little company were glad to escape with their lives to the neighbouring hills, among which they found a temporary refuge. Even in these trying circumstances they were able to turn their knowledge to good account in different ways so as to alleviate their own hardships as well as those of their fellow-sufferers.

In the third moon of the first year of Tung-che, or 1862, an Imperial edict called upon the Governor-General of the "Two Kiang" provinces to search throughout his jurisdiction for men of talent and ingenuity, and versed in the arts and sciences, who should assist in improving the condition of the Empire. H. E. Tsêng Kwo-fan accordingly selected six men, whose names were duly forwarded to Peking. Among the number were Messrs. Hsü and Hwa, whose reputation as scientists had by this time extended far beyond their native town. They were afterwards invited to an interview with the Governor-General at Anching, and were at once retained on his staff, with the view of their being able to study and perfect themselves in the more useful branches of the foreign arts, sciences, and manufactures.

At that time the rebels were in possession of Nanking, and the surrounding country was in a most unsettled state, so that little could be done in the direction of improvement or study. Mr. Hwa, however, was engaged with others in collecting and preparing such scientific books as China then possessed. This work was after-

[1] By Mr. John Fryer, Chief Translator to the Chinese Arsenal.

wards continued at Nanking, where, under the auspices of the Viceroy, an establishment was commenced for the publication of useful books. Many valuable works, such as Mr. Wylie's translation of Euclid, the Differential and Integral Calculus, Dr. Edkin's translation of Mechanics, and similar treatises, have already been republished there, and the establishment is still in existence.

While Mr. Hwa was engaged in this kind of labour, Mr. Hsü was called upon to perform a task of a very different kind. The Viceroy required him to build a steamboat, and reluctantly he consented to make the attempt. He first made a model of an engine from the somewhat rough illustrations in Dr. Hobson's work before referred to. This proving to be a success, he was encouraged to proceed with the more difficult task assigned him. By means of Chinese tools and materials, and such ideas as he contrived to get through looking carefully over a small steamer at Anching, he managed to prepare his designs, and commenced his work with no foreign assistance whatever. He met with a most determined opposition from local officials, but, assisted by his son and encouraged by the Viceroy, who took a lively interest in the proceedings, the work was at length completed; not, however, without at least one entire failure. The steamer, which was of twenty-five tons measurement, was able to make 255 *li*, or about 85 miles, in fourteen hours, and to do the return journey in less than eight hours at her trial trip on the Yang-tse in 1865. The Marquis Tsêng, now ambassador to England, also took great interest in this little craft, giving her the highly classical name of *Wang-kao* or *Yellow Swan*, and making several trips in her on the Yang-tsze.

It will be readily granted that the experience gained under so many difficulties ought to have given Mr. Hsü and his son somewhat of an insight into foreign arts and sciences, and to have raised them far above the level of the best of their fellow-countrymen. Not contented, however, with the small stock of knowledge they felt they possessed, they made several visits to Shanghai, in some of which they were joined by Mr. Hwa, with a view to making new mental acquisitions. During these visits they made the acquaintance of Mr. Li Shan-lan, the celebrated native mathematician, who was then translating with the Rev. J. Edkins and Mr. Wylie such works as Whewell's "Mechanics," Herschel's "Astronomy," Euclid, the Calculus, &c., at the London Mission. On these occasions they added largely to their intellectual attainments. They also gained many new ideas from other well-known Sinologues, such as the Revs. Messrs. Muirhead and John, and Dr. Williamson, for whom they often express much respect.

At length deciding to settle in Shanghai for the convenience of carrying on their investigations and studies in the vicinity of foreigners, they obtained from H. E. Tsêng Kwo-fan a mandate attaching them to the staff of officials at the Kiangnan Arsenal, which had recently been commenced. Here they arrived in the beginning of the year 1867, and soon endeavoured, in connection with the Arsenal directors, Fung and Shen, to organise methods by which their long-cherished hopes might be realised and their thirst after knowledge satisfied. Their aspirations finally resolved themselves into a definite form, and led them to devise a plan for the translation and publication of a series of treatises on the various branches of Western learning that should bear some resemblance to the *Encyclopædia Britannica*, of which they had ordered a copy from England. In this manner they hoped not only to instruct themselves, but to diffuse the knowledge they had acquired with so much pains among their fellow-countrymen, and leave behind them a lasting name throughout the Empire. It was also imagined by them that such a series of treatises would prove especially useful as text-books in various educational establishments of a high order, which it was then hoped would soon be instituted in the different provinces.

This scheme was warmly taken up by the directors of the Arsenal, who easily obtained the permission of the Viceroy to begin to carry it out on a small scale by way of experiment. Various foreign gentlemen were applied to for their services, but without success, till at last a commencement was made by Mr. Fryer, who at that time was editing the Chinese newspaper published at the *North China Herald* Office in Shanghai. He was asked to purchase a collection of suitable European books, and to begin at once to translate a work on Practical Geometry with Mr. Hsü, jun. Subsequently Mr. A. Wylie's services were secured for a treatise on the Steam Engine, with Mr. Hsü, sen., while Dr. Macgowan undertook the translation of a work on Geology with Mr. Hwa. These three books, which formed the beginning of this large undertaking, were translated at the residences of the Europeans above named. It soon became manifest, however, that it would be impossible to carry on the work successfully except at the Kiangnan Arsenal, where the books were to be printed and published, and which is distant about four miles from the foreign settlement. Here Mr. Fryer was pressed to give his whole time and attention to translation, and in June of 1868 commenced his labours in a building which was set apart for that purpose. The earliest publications gave such satisfaction to the Viceroy at Nanking that he ordered the operations of the translation department to be extended; the immediate result of which was the addition of Mr. (now Dr.) Kreyer to the regular staff. Subsequently when the Government school for interpreters had been removed from inside the Chinese city to the arsenal, Mr. (now Dr.) Allen's services were re-engaged to conduct it, and he was further asked to give a portion of his time to the work of translation. Dr. Kreyer, after rendering effective service as a translator for some time, left his post for that of interpreter to the Taotai of Shanghai, much to the loss of the department. The vacancy was afterwards filled by Dr. Suvoong, a Chinese graduate of the United States, who has begun to enrich the collection of books by translations of medical and other works, for which task his long residence and studies in America have well qualified him.

The number of the native members of the staff has been subject to frequent changes. At present there are five Chinese gentlemen who are engaged either in writing the translations or in preparing the various books for publication. Among this number Mr. Hsü, sen., is the only one who has remained constantly at his post from the commencement, and whose desire for knowledge does not yet appear to abate, although he is now well advanced in years. Others have worked for longer or shorter periods, and then have either grown tired of such monotonous labour, or have accepted official appointments that were offered to them. This continual changing has not been without injurious effects in some cases. Either important books have been left half finished, no one liking to take up another's work, or if finished the manuscripts have been taken away or passed from one to another, so that after the lapse of a year or two they cannot be found.

Among the officials who have left the work for higher appointments may be mentioned H. E. Li Fung-pao, the present Minister to Berlin; Mr. Hsü, jun., who has just started to join him as secretary, and who was lately Director of the Shantung Arsenal; Mr. Hwa, who has been Director of the Tientsin Powder Works, and now is Resident Curator of the Chinese Polytechnic Institution; Mr. Wang, who is a director of the Tientsin Arsenal; and Mr. Hwang, an *attaché* of the Chinese Legation in London. The names of several other gentlemen in important positions might also be added, all of whom were at one time or another on the staff, and manifestly derived

benefit from carrying on work which brought them into daily contact with Europeans. Viewed, therefore, merely in the light of an educational establishment, this department has been of much benefit to the Government by supplying so many intelligent and well-informed officials, all more or less imbued with favourable notions respecting foreigners and a desire to see foreign intercourse extended.

The history of Mr. Ka Pu-wei, who has for several years worked in connection with this department, is almost as remarkable as that of Messrs. Hsü and Hwa. From his childhood he had a strong leaning to mathematical studies; but not being in independent circumstances, he was obliged to support himself by keeping a rice-shop inside the city of Shanghai. Here he prosecuted his studies with such success that he was able to calculate eclipses and to prepare an almanac giving particulars respecting the movements of the heavenly bodies, which he ventured to publish. The Government alone having the authority to publish almanacs, and the country being at the time unsettled by the Taiping rebellion, he was charged with having designs against the Imperial throne, and cast into prison. He narrowly escaped with his life, but suffered imprisonment for above a year, till his friends could procure his release. He is now chiefly engaged in compiling and publishing a nautical almanac, calculated for the longitude of Shanghai instead of Peking or Greenwich, and in preparing various books of mathematical tables, for all of which his past studies have been an excellent means of preparation.

Equally interesting is the history of Mr. Li Shan-lan, who was for a short time connected with the Translation Department before his removal to Peking, as Professor of Mathematics in the University of that city. He is a native of the Province of Chekiang, and from his earliest years manifested a remarkable genius for the science of numbers. In the year 1845 he began to publish original treatises embracing different problems in the higher mathematics. On one occasion when at Shanghai he went to a chapel where Dr. Medhurst was preaching to a Chinese congregation, and showed him one of these works. This resulted in his being engaged in the London Mission, where Mr. Wylie took him in hand and translated with him several mathematical works of the highest order, as well as Herschel's "Outlines of Astronomy." With Dr. Edkins he translated Whewell's "Mechanics." Nothing in the way of science seemed to come amiss to him. Eventually he commenced Newton's "Principia" with Mr. Wylie, of which he only translated a small portion of the first book. The remainder of the first book he finished at the Kiangnan Arsenal with Mr. Fryer during the few months of his connection with the Translation Department. He seemed to enter into the most intricate of its problems with the greatest zest and enthusiasm, and often expressed his intense admiration for Newton's genius. His skill in solving the most difficult mathematical questions that could be given him was truly remarkable. Of course there are not many men of his calibre to be found in China; but still no doubt others will be brought to light through the impulse which foreign intercourse is bringing to bear upon the stagnant minds of this long-isolated nation. Now and then a lesser light than Li Shan-lan appears among the various visitors at the Arsenal, and it is reported that Ku Shang-chih, a native of Chiu-shan, is in advance of him; but this needs confirmation.

The establishment where the books are printed in the old-fashioned way from wooden blocks was first merely a small room, but has now grown into a separate range of buildings, and employs upwards of thirty hands as block-cutters, printers, bookbinders, &c., and is superintended by an under-official. Another under-official has charge of the books when printed, and is responsible for the money derived from their sale. About half-a-dozen copyists complete the *personnel* of the department.

The library of foreign books consists now of several hundred volumes, and forms probably the best collection of the kind in China. It is contemplated to make extensive additions shortly of recent important publications.

It may be mentioned that, as a mark of Imperial favour, various honorary degrees of rank have been conferred upon the native and foreign members of the Translation Department, in acknowledgment of the value of their services. Mr. Fryer, Dr. Kreyer, and Dr. Allen received diplomas entitling them to the third, fourth, and fifth degrees of civil rank respectively.

On various occasions some of the highest officials in the Empire have sent requests for books to be translated, bearing on subjects in which they took particular interest. Notably this has been the case with H. E. Li Hung-chang. Among the high dignitaries who have expressed their satisfaction at the results attained by this department, it may be mentioned that on one occasion, when staying at the Arsenal, H. E. Ting Jih-chang expressed himself in strong language as to the importance which he attached to the translation of books, compared with the work carried on in other departments. The Marquis Tsêng, who resided for a few days at the Arsenal in 1877, and has from the first been in favour of the undertaking, gave Mr. Fryer a Chinese fan, on which he had written by way of compliment a verse of Chinese poetry of his own composition, and which may be freely translated as follows:—

"Nine years have elapsed since our last conversation;
But your translations have been forwarded to me from time to time.
May your fame surpass that of Verbiest and Schaal,
As the electric light exceeds the spark of the glowworm."

(*To be continued.*)

THE GREAT VIENNA TELESCOPE

THE political and social disturbances in Ireland have of late somewhat diverted attention from the literary and scientific work done in that country. Such work has nevertheless proceeded on its quiet way despite land agitation, failure of crops, or even commercial distress; and Ireland is to be congratulated on the completion of the fine 27-inch refracting telescope, designed and constructed by Mr. Howard Grubb of Dublin for the Imperial and Royal Observatory of Vienna.

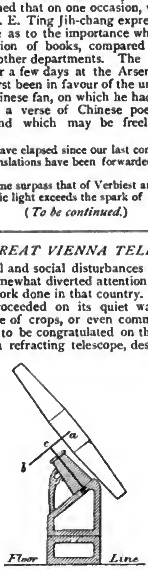


FIG. 1.

This telescope is the largest equatorial refractor at present in existence. In the year 1873 Director Littrow, of the National Observatory of Vienna, induced the then Austrian Minister of Public Construction (R. von Stromayer) to consent to the removal of the Observatory from the old site in the Vienna University grounds to a more favourable site, consisting of a level piece of ground

of some fifteen acres in extent, about 200 feet over the general level of the city, and nearly three miles to the north of the cathedral of St. Stephen's. On this plateau a magnificent edifice has been erected, which measures from north to south 330 feet, and from east to west 240 feet. The general plan of the observatory is that of a

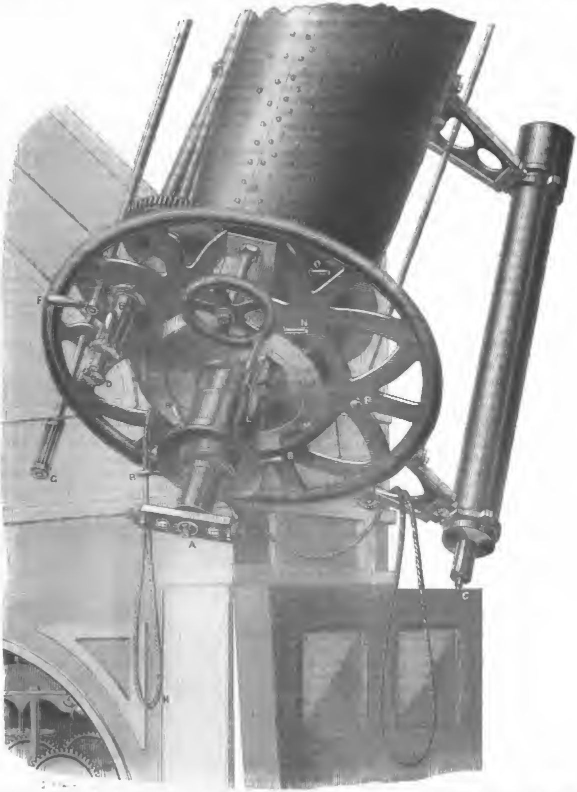


FIG. 2.—Eye-End and Breech-Piece of Telescope. A is the micrometer eye-piece; B B, handles for working the focussing screw; C, eye-piece of finder telescope, 4 in. aperture; D, eye-piece of reader for reading right ascension and declination circles; E, handle for revolving same to point to various verniers; F, clamping handle in declination; G, slow motion handle in declination; H, clamping cords for right ascension; I, slow motion cord for right ascension; K, quick motion handle for right ascension and declination movements; L, flute key for position circle clamping; M, window through which glass position circle is viewed while illuminated from behind by beam of light from gas-lamp at end of declination axis; N, screw for slow motion in position angle; O, key for throwing into position a set of illuminators for "dark field" illumination of micrometer; P, handle for throwing into position an arm carrying one central mirror for "bright field" illumination of micrometer.

Latin cross. One great dome forms the centre, three smaller domes terminate the extremities of three of the short arms, while the fourth arm, looking south, contains the library and lecture-rooms, &c. The south *façade* is very imposing, and in it are the rooms for the director of the Observatory. In one of the smaller domes (each 27

feet in diameter) is placed a 12-inch refractor by Alvan Clark, and it is intended to have in the other two an equatorial for photographic work, and an altazimuth or comet-seeker. It seems a pity that the use of so magnificent a building should in some measure be sacrificed to architectural display, for the splendid south *façade* being devoted to domestic purposes, an enormous proportion of the observations will of necessity be made over the chimneys of the dwelling-house. The great central tower is 45 feet in diameter, and its dome and the revolving machinery to work it have been supplied by Mr. Howard Grubb, who has also put up all the domes of the smaller towers. The great dome is of a very peculiar construction. It is formed of two thin shells of steel plate varying in thickness; these are riveted on the inside and outside of a very light set of steel plate girders 18 inches deep at base and 9 inches at crown, the whole forming a cellular construction like the top and bottom of the Britannia Tubular Bridge. This form gives enormous stiffness for the amount of material used, besides possessing several points of peculiar usefulness for astronomical work, such, the more specially, as keeping the temperature of the dome wonderfully constant, even under most trying circumstances. The total weight of this steel dome, with its ribs and girders, the cast-iron sole plate, and fitting, is about 15 tons, and as the result of a series of very ingenious mechanical contrivances thought out by Mr. Grubb, the tractive force necessary to pull round this huge drum, even when resting, as at Dublin, on a temporary support and insufficiently levelled, was only 70 lbs. All these domes were constructed, so far as the framework went, in Dublin, and they have been placed for some time past *in situ* at Vienna, under the superintendence of Mr. W. K. Davis, Mr. Grubb's engineer.

The new Observatory having been well advanced in 1874 Director Littrow sent his first assistant (now the General Director), Dr. Ed. Weiss, on a tour of inspection to all the great observatories and astronomical workshops of Europe and America, with the result that Dr. Weiss recommended to his Government that an instrument of at least 26-inch aperture should be ordered from the establishment of Mr. Howard Grubb, and in the year 1875 the contract between the Austrian Government and Mr. Grubb was signed for a 27-inch refractor. The mechanical parts were nearly finished in the year 1878, but the greatest difficulties were experienced by the Messrs. Feil of Paris in obtaining perfect disks of glass for the objective, and it was not until after several trials and towards the close of 1879 that this firm succeeded in sending to Dublin disks that ultimately proved perfect. These had to be worked into the objective at Mr. Grubb's establishment, and on several occasions serious flaws were only discovered at a time when but for them the object-glass would have been complete.

The general form of the equatorial may be described as a modification of the German form. In designing it Mr. Grubb kept in view the fact that while circumpolar motion was very desirable—indeed almost necessary—for objects from the horizon to, or approaching to, the zenith, it was by no means so essential for objects beyond that to the pole. This will be evident on consideration, for nineteen-twentieths of the objects usually under observation in these latitudes are between the zenith and south horizon, and if one be observed north of the zenith its apparent rate of progression is so slow that a very little motion of the telescope takes place for any given duration of observation. Keeping this in view, Mr. Grubb has adopted the form shown (Fig. 1), in which the intersection of the axes *c* is placed, not over the centre of the pier, but over the north end nearly, and this allows the telescope circumpolar motion for all objects up to the zenith. It should also be observed that this circumpolar motion gives another advantage besides that of non-reversal, viz. that it allows choice of two positions of the telescope in observing almost any object. In observing near the meridian the telescope may be used *either* to the east or west of the pier, and in observing towards the east or west the telescope can be used either over the polar axis or under. This is sometimes found to be a great convenience. The above considerations were those which influenced Mr. Grubb in deciding on the particular form for the Vienna instrument, and the result is that almost all the advantages of circumpolar motion are obtained without any serious counterbalancing disadvantages.

To enter into full details of all the various parts of this magnificent telescope would far exceed the space at our command. Up to this it has been thought impossible to apply to large equatorials those many important contrivances to save time and labour which all first-rate

Fig. 3.

instrument-makers would adapt to small instruments, but in this one all such contrivances and many novel adaptations of such have been utilised. To enable our readers to form some idea of the resources within the ready reach of the observer, we, through the kindness of the proprietors of our contemporary, *Engineering*, give an illustration (Fig. 2) of the eye-end of the tube.

In the largest telescope until now in existence, the great 26-inch refractor at Washington, the oft-recurring operation of reading the circle involved the sending of an assistant with a hand-lamp to climb up some twenty feet of the instrument, and the vernier not being in a fixed place, but moving about with the telescope, the labour and difficulty of this operation are great. In the Vienna instrument Mr. Grubb has so contrived it that the observer sitting in his chair can read the right ascension and declination circles through one single reader, all being illuminated by one gas-lamp hung on the end of the declination axis.

Another wonderful convenience is enabling an assistant

to easily turn and set the instrument to right ascension. This can be understood from Fig. 3, which is from a photograph. In the case of an instrument of the size of the Vienna equatorial, the observer requires an elaborate stage or platform of variable height and position in order to reach the eye-end, and if he has to move to another object he must descend from his chair or platform and move *it* before he can move the instrument, and then he has not the facility of sighting objects, but must adopt a sort of tentative process, climbing up and down his stage and moving it and the telescope alternately. To avoid this labour and save time an arrangement is supplied by which the assistant from the ground floor can set the instrument in right ascension, and once set in one direction, the other (declination setting) can be readily managed by the observer. The assistant is supposed to stand at the south end of the pier. He has there before him a desk on which his catalogue and paper can rest, a sidereal clock-face let into the south end of the pier, a reader telescope by which he can read the lower right ascension circle, and a hand-wheel, which by means of shafts and gearing communicate motion to the instrument in right ascension, and finally a handle which is keyed on to a screw forming the toe-bearing of the quick motion driving-shaft. By giving this handle one turn, the driving-shaft is allowed to drop down out of gear with the rest of the wheelwork, so that the clock when driving the instrument may not have the additional work to do of driving this shaft, which is necessarily a quick one as compared to the motion of polar axis itself. A lamp is attached to pier at west side, which serves to throw light on sidereal clock-face, catalogue, &c., on desk, and also to send a beam of light up through a long tube directed to the vernier of the right ascension circle which is visible through the reader. The assistant has therefore full power of setting the instrument roughly, or if desired with any degree of accuracy, in right ascension, or reading off the right ascension of an object which requires to be identified.

The tube of the telescope is made of steel plate about one-eighth of an inch thick in the centre, and tapering to about one-twelfth of an inch at the ends, the points being all lapped and riveted; it is 33½ feet in length, and lessens from 36½ inches in diameter in the centre to 27 inches at the one and 12 inches at the other end. The weight of the moving parts is between six and seven tons, and still the whole is under easy control of the muscular power of one arm.

The Commissioners appointed by the Austro-Hungarian Government to report on this telescope consisted of Prof. Ball, Astronomer-Royal for Ireland, Earl of Crawfurd and Balcarres, Mr. Huggins, Prof. J. Emerson Reynolds, Earl of Rosse, Prof. Stokes, Dr. G. J. Stoney, and Mr. Walsh, the Austro-Hungarian Consul at Dublin. On March 16 this Commission forwarded to the Austro-Hungarian Embassy in London their full approval of the performance of the instrument, thus marking the successful completion of the largest refracting telescope in existence.

It will be remembered that Mr. Grubb has built among other fine instruments the great Melbourne Reflector, the largest equatorially-mounted telescope known. He has not rested on his laurels, but is now to be cordially congratulated on a still greater accomplishment.

WHEWELL ON COLOURING GEOLOGICAL MAPS

ONE of the subjects to be brought forward for discussion at the International Geological Congress to be held this year at Bologna is the *unification des figurés;* that is to say, an attempt will be made to come to some agreement as to the signs and colours to be used on geological maps. Two things have to be indicated: *succession, i.e.* the stratigraphical position of the rock; and *formation*, by which, adopting a terminology now in much favour on the Continent, we mean the lithological character and origin of the rock, and not, as is generally understood by the word in this country, a subdivision of the stratigraphical series. I have found among Prof. Sedgwick's papers a scheme drawn up by Dr. Whewell nearly half a century ago, which I have thought might be of use to those who are considering this question, and of interest to many besides. It is dated from Dublin, 1835, where he was attending the meeting of the British Association, and it was probably suggested by the publication of Griffiths' map.

William Smith shaded up to the lowest beds of the various groups into which he divided the strata, in order by contrast to mark more strongly the coming in of a new series, but he does not seem to have adopted any system beyond sometimes taking such tints as were suggested by the predominant colours of the rocks represented. Salter, I remember, proposed a scale of colours founded on the spectrum, but the scheme proposed by the late Master of Trinity I do not remember to have seen put forward before. THOS. MCK. HUGHES

Trin. Coll., Cambridge, April 26

Proposal for a Systematic Scale of Geological Colours

THE objects which it would be desirable to secure in fixing the scale of colours for a geological map appear to be the following:—

1. That the different members of the geological series should be coloured in a manner somehow depending on their *place in that series*, so that successive rocks are distinguished.

2. That *this* distinction (of succession) should be governed by some *general principle*, and should not be merely arbitrary; so that without referring to the index list the colour itself should show the difference of older and younger in the rock.

3. That the colours should also show the great leading differences of the material of the strata (limestone, sandstone, clay), so that without referring to the scale these differences should be known from the colour.

4. That *igneous* rocks should by some general circumstance in the colour be distinguished from sedimentary.

5. That the colours which are brought near each other by proximity of succession should be strongly *contrasted.*

The following method would, I think, secure these objects. It proceeds on the supposition that there are three primary colours—Red, Yellow, Blue—and that any two of these mixed in considerably different proportions make several shades of intermediate colour; thus between red and yellow we have many shades of orange, proceeding from pure red, through reddish orange, to orange, yellowish orange, and pure yellow; in like manner we have many shades of green between yellow and blue, and many shades of purple between red and blue. Our scale can be subdivided as far as these shades are distinguishable. We have also black, which can be combined in various proportions with each of the simple colours; thus black with successive doses of red makes brown more and more red.

The general principles which I propose are these:—

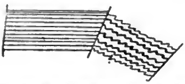
1. Let one of the above four simple colours represent the material and another the order of succession, and let successive mixtures represent successive strata of the same material. Thus let the oldest *limestone* be pure *blue*, let doses of yellow be added to mark newer and newer limestones, the chalk being a very yellow green; thus all the oolite series will be green of different shades.

2. Let the simple colour which represents succession be different for different materials, thus let *blue* represent succession for the *clays*, and let all the clays be *purple*, the oldest therefore being the reddest, and the newest the bluest purple.

In the same way let black represent *sand* strata, and let *red* represent *succession* in such strata; then the sandstones will be represented by *browns*, the oldest being the blackest, and the newest the reddest brown.

3. Let igneous rocks be represented by a general colour, as *red*, and let any order which obtains among them (succession, for example, if succession can be traced) be marked by doses of another colour, as yellow; then the igneous rocks will be all red or orange, the newest being the yellowest.

4. Let other differences (as mineralogical differences) be represented by other means, as by *hatching*. Thus granite and quartz rock, if contemporaneous, may be marked by red with the addition of lines—



According to these principles the English strata would be represented by the following colours, which may thus be denoted by letters; let Red, Yellow, Blue, *Black*, be marked by R, Y, B, *A*, and let mixtures be represented by combinations of the letters. Then we have—

Orange, R R R Y, R R Y, R Y, R Y Y, R Y Y Y.
Green, Y Y Y B, Y Y B, Y B, Y B B, Y B B B.
Purple, B B B R, B B R, B R, B R R, B R R R.
Brown, A R R R, A R R, A R, R A A, R A A A.
Unstratified rocks (primary), R.
" " (trap), R R Y, R Y, R Y Y, &c.
Clay slate, B R.
Oldest limestone, B, B B B Y, B B Y.
Oldest sandstone, A, A A A R, A A R.
Secondary limestone (Mountain), B Y.
New red sandstone " A R.
Oolites " B Y Y, B Y Y Y.
Green sand " A R R.
Chalk " B Y Y Y Y.

Observations:—1. The method here proposed would answer the objects above stated, for the material and order of succession are marked by their proper colours; and the sands, clays, and limestones which occur near each other would be very distinct. Thus the green sand is reddish brown (A R R) and the chalk is yellow green (B Y Y Y Y).

2. Perhaps it may appear an inconvenience that contiguous members of a formation of the same material are proximate shades of the same colour: thus the oolite beds would be successive shades of green (B Y Y, B Y Y Y), and might be difficult to distinguish. I answer, that the beds themselves are often difficult to distinguish, so that our language is most indistinct when our knowledge is most indistinct; again, that the inconvenience, when it is one, may be remedied by marking or hatching those strata; again, that no systematic method can be devised which will not be open to this objection.

3. The above principles being adopted, the whole range of the colours, as modified by the *succession-colour*, might be different according to the different object of the geologist. Thus if he had to exhibit the whole geology of England, *all* limestones whatever must come in between B and Y (Blue and Yellow). But if he take the secondary period only, he may use all the possible shades of green for members of that part of the series alone, and may thus make his terms more numerous.

4. If the *whole* range of the *succession-colour* be employed and exhausted on a *part* only of the geological series of strata, the strata which occur beyond this part will, in the scale used on such occasions, be without representative colours. This is an inevitable evil. We cannot combine the extremes of detail and generality. If we use all our means in expressing a part, we must for the time omit to express the remainder. We must do this when our purpose requires and justifies it.

5. When we use all our colours for part of the geological series, we still preserve the principles above proposed and the advantages which they secure, namely, that the material and the succession are both exhibited in an intelligible way without reference to the index.

6. If we thus make a part of the geological succession to occupy the whole power of our successive colour, we have different colours from those which we have when we represent the whole succession. The partial map has a different index from the general one. This is a serious evil, and must not be incurred without strong necessity.

7. It may be mentioned as an advantage of the proposed notation that many of the colours which are used in it agree very nearly with colours commonly used: as red for granite, blue for older limestones, yellow, or yellow-green for chalk, brown for some sandstones, purple for clays. The main novelties are that the oolites are green, and the coal-fields not black; but as to the latter point, query, whether a *coal* stratum be a proper geological distinction? If the coal-measures be sand or clay beds, they should be brown or purple, according to the material which is taken as characteristic.

In a given case we may have to determine the question above suggested, whether we should employ the whole range of our succession colours on a limited geological period, as, for example, the transition-period. In order to decide this consider what you want. How many limestones have you? How many sandstones, how many clay-rocks, which are to be distinguished? If the oldest limestone be pure blue, and mountain limestone pure green (B Y), we can easily interpose three or four limestones, as (B B B B Y, B B B Y, B B Y); is this a sufficient number of terms for you? and so of the rest.

Summary.—Let there be in all cases a *material colour* and a *succession colour*, namely—

	Material.	Succession.	
Limestone	B ...	Y ...	(Green)
Sandstone (black) ...	A ...	R ...	(Brown)
Clay	R ...	B ...	(Purple)
Igneous Rocks	R ...	Y ...	(Orange)

The latter two lines lead to no confusion, for though R in the clays indicates the material, it is never to be used without B, and R in igneous rocks is never used with B.

It may be observed that in the preceding scheme I have not exhausted the power of colour, for I have not used either the combinations of *black with blue* or of *black with yellow*, or the combination of *three* simple colours. W. W.

Dublin, August 17, 1835

NOTES

We are glad to learn that the Italian Government has decided on having a deep-sea expedition in the course of this summer to explore the Mediterranean. The necessary arrangements are now being made by Prof. Giglioli, the eminent zoologist, at Florence, who will take charge of the biological part of the work. Capt. Magnaghi will be intrusted with the physical part of the work, as well as with the command of the vessel. The scientific results may be expected to be of especial interest, because nothing has been done to explore the depths of the Mediterranean beyond the short cruise in H.M.S. *Porcupine* in 1870.

Prof. Tyndall has written to the *Times* of yesterday a letter of great interest on the attitude of the late Mr. Carlyle towards modern science, which it has been taken for granted was purely hostile. But according to Prof. Tyndall, not only was Mr. Carlyle deeply interested in some of the latest researches of science, but he took great and successful pains to understand

them, and was even open to accept some of the latest developments of scientific thought. At first, for example, his attitude to Darwinism was decidedly hostile, but later on, Prof. Tyndall states, "he approved cordially of certain writings in which Mr. Darwin's views were vigorously advocated, while a personal interview with the great naturalist caused him to say afterwards that Charles Darwin was the most charming of men."

WE learn from the *American Naturalist* that a proposal will be made at the next meeting of the American Association to invite the British Association to hold its meeting in 1883 in America in conjunction with its American sister. The proposal deserves consideration.

ACCORDING to the *Frankfürter Zeitung*, at Nakkoo, in the Island of Lapland, an eagle was shot on the 15th ult., which measured 6½ feet between the tips of the wings. Round its neck it had a brass chain to which a little tin box was fastened. The box contained a slip of paper on which was written in Danish, "Caught and set free again in 1792 by N. and C. Andersen.—Boetod in Falster, Denmark."

WE regret to learn that the printing of the "International Bulletin" issued by the U.S. Signal Office will hereafter be twelve months after date, instead of six months as at present. This seems to us a step backwards from the energetic and liberal policy of the late General Myer.

PROF. GEGENBAUR, the well-known Heidelberg comparative anatomist, is said to be dangerously ill with blood-poisoning, contracted while dissecting.

THE Davis Lectures for 1881 will be given in the lecture-room in the Zoological Society's Gardens in the Regent's Park, on Thursdays at 5 p.m., commencing June 16. The following are the subjects and lectures:—June 16, Whales, Prof. Flower, F.R.S.; June 23, Dolphins, Prof. Flower, F.R.S.; June 30, Extinct British Quadrupeds, Mr. J. E. Harting; July 7, The Limbs of Birds, Prof. W. K. Parker, F.R.S.; July 14, Birds Ancient and Modern, Mr. W. A. Forbes; July 21, Zoological Gardens, Dr. P. L. Sclater, F.R.S.; July 28, Chameleons, Prof. Mivart, F.R.S. These lectures will be free to Fellows of the Society and their friends, and to other visitors to the Gardens.

AT the fifty-second anniversary of the Zoological Society the report of the Council on the proceedings of the Society during the past year was read by Mr. Sclater, F.R.S., the Secretary. It stated that the number of Fellows on December 31, 1880, was 3309 against 3364 at the same date of the previous year, 153 new Fellows having been elected, and 208 removed by death or other causes during the year. The total receipts for 1880 had amounted to 27,388*l*. against 26,463 for 1879. The ordinary expenditure for 1880 had been 24,753*l*., and the extraordinary expenditure 1825*l*., besides which the sum of 1000*l*. had been devoted to the repayment of part of the mortgage-debt due on the Society's freehold premises, which had thus been reduced to 7000*l*. This had left a balance of 879*l*. to be carried forward for the benefit of the present year. The total assets of the Society on December 31 last were estimated at 27,852*l*., and the liabilities at 9078*l*. Amongst the works carried out in the Society's Gardens in 1880 were specially noticed the completion of the insectorium (which had just been opened to the public, and contained a collection of living insects), and the thorough repair and reconstruction of the parrot-house. The number of visitors to the Society's Gardens in 1880 had been 675,979, against 643,000 in 1879. The zoological lectures having been well attended during the past year, would be continued during the present season. The number of animals in the Society's collection on December 31 last was 2372, of which 703 were mammals, 1438 birds, and 231 reptiles. Special attention was called to the increasing number of presents to the menagerie received by the Society of late years, the number thus acquired having now so increased as to usually exceed the number of those obtained by purchase. Col. J. A. Grant, C.B., F.R.S., Dr. Günther, F.R.S., Prof. Newton, F.R.S., Osbert Salvin, F.R.S., and the Right Hon. George Sclater Booth, M.P., were elected new Members of Council. Prof. W. H. Flower, LL.D., F.R.S., was re-elected President, Mr. Robert Drummond, Treasurer, and Mr. Philip Lutley Sclater, M.A., Ph.D., F.R.S., Secretary to the Society.

A NEWLY issued part of the Annals of the "Museo Civico" of Genoa is devoted to a memoir by Dr. Peters and Marquis G. Doria on the Mammals of New Guinea and the adjoining Papuan Islands, procured during the recent researches of Beccari, D'Albertis and Bruijn. In the collection amassed by these ardent explorers fifty-seven species are represented, amongst which are twenty-two Marsupials, nineteen Bats, and thirteen Rodents; *Sus papuensis*—an introduced species—was the only Ungulate met with. It will be seen, therefore, that, as in Australia, the Mammal-fauna of the Papuan sub-region may be said to consist nearly entirely of Marsupials, Bats, and Rodents. Its affinity to Australia is further shown by the presence of a Monotreme (*Tachyglossus bruijnii*), and by the occurrence of such genera as *Macropus*, *Dasyurus*, and *Dromicia*. The memoir is illustrated by eighteen excellent plates.

MR. THISELTON DYER writes to the *Daily News* in reference to a suggestion "that the labels of ferns, flowering and other plants in Kew Gardens should bear not only scientific but popular names." Mr. Dyer states that as far as such popular names can be ascertained they are carefully indicated on the Kew labels. "There is some misapprehension," Mr. Dyer states, "about the popular names of plants. Your correspondent seems to have proceeded on the assumption that there is a popular botanical nomenclature co-extensive with the scientific. This is very far indeed from being true even of a vegetation so thoroughly investigated as that of the British Islands. Of the plants of foreign (especially tropical) countries it is obviously, with the exception of some useful or medicinal plants, not true at all. But, as you will observe from the accompanying copy of the popular guide to the Royal Gardens, where anything like a genuine popular name exists, great prominence is given to it at Kew. . . . The popular tongue is by no means ready in finding acceptable names for the foreign plants of our gardens, and is quite content to accept from botanists Dahlia, Petunia, Phlox, Pelargonium, Gladiolus, Calceolaria, and the like."

WE take the following from the May number of the *American Naturalist*:—The Kansas Academy of Science, at their November meeting, appointed a Commission to memorialise the Legislature in reference to a State Survey. Two preliminary surveys under Professors Mudge and Swallow have already been made. A more extended and thorough scientific survey is now needed. The most active geologist now in the field in this State is Prof. O. W. John, who for two years past has studied the stratigraphical geology of Eastern Kansas. Last summer Prof. F. H. Snow, with several assistants, spent over a month in Santa Fé Cañon, New Mexico, as well as in Colorado, and made important entomological collections; among them were twelve new species of coleoptera and an interesting collection of geometrid moths, comprising a number new to the Colorado plateau region. Prof. A. Hyatt, the curator of the Boston (U.S.) Society of Natural History, announces that a sea-side laboratory will be opened this year under his direction at Annisquam, Mass., three miles from Gloucester, from June 5 to September 15.

WE have received copies of handy and cheap guides to the New Natural History Museum; penny guides are furnished for

each of the departments, and a guide to the whole place costs only threepence. This is as it should be.

FROM Gustav Wolf, the Leipzig publisher, we have received a copy of a most useful "Naturwissenschaftlich-mathematisches Vademecum." The catalogue is really an index, both of subjects and authors, to all recent publications of importance in physical and natural science, and is likely to prove of real service to all scientific workers.

AT the meeting on April 26 of the Institution of Civil Engineers, Mr. Walter R. Browne, M.A., M.Inst. C.E., read a paper on "The Relative Value of Tidal and Upland Waters in maintaining Rivers, Estuaries, and Harbours." The author, while declining to lay down any universal rule, held as a general principle that the main scouring agent was not the tidal but the low-water flow. This principle was supported by the following line of argument:—1. The silt, which tended to choke up tidal channels, was almost wholly due to the tidal water, and not to the fresh water. 2. The tidal water brought up more silt on the flow than it took down on the ebb; *i.e.*, on the whole it tended to choke the channel, not to scour it. 3. The low-water flow, if left to itself, scoured away the deposit and kept the channel open 4. Hence it was concluded that where the two acted together, the scour must be due mainly, if not entirely, to the low-water flow, and not to the tidal flow. It was added that low-water scour was in its nature self-regulating, whilst tidal scour, if it once began, would tend to increase indefinitely. But the essential point was to discover the ratio of the bottom to the surface velocity under all possible circumstances, since it was obvious that the former alone had any scouring effect. Tables were given showing that the ratio of bottom to surface velocity diminished rapidly with an increase of depth; but their application to tidal channels was doubtful, because then the river, instead of being (in a theoretical point of view) indefinitely long, fell at a short distance into an estuary whose waters might be considered at rest. The author had conducted two series of experiments on the surface and bottom velocities of the River Avon, in the course of an ebb-tide. Both series of experiments showed that during the greater part of the ebb the bottom velocity was actually *nil*. When about two thirds of the ebb was over, the bottom layers of water appeared to start into activity, and to assume a velocity about two-thirds of that at the surface. This is shown by the following extract from the tables:—

Position of meter.	Time after ebb began. h. m.	Depth of water. ft. in.	Velocity, feet per sec.
Surface	1 0	22 8	3·57
Bottom	1 10	21 0	0·00
Surface	1 53	14 10	4·60
Bottom	2 4	13 6	0·00
Surface	3 45	5 8	3·07
Bottom	3 54	5 7	1·91

The following conclusions were drawn from these and other experiments:—In the largest rivers the bottom velocity is for practical purposes the same as the surface velocity. In ordinary rivers the bottom velocity bears to the surface velocity a ratio which is about three-fourths at 5 feet depth, one-half at 15 feet, and one-third at 25 feet. In tidal channels, such as the Avon, during two-thirds of the ebb the slope of the surface is exceedingly small; and while the surface velocity is large the bottom velocity is *nil*. During this period no scour, but rather deposit, is going on. For the remainder of the ebb the conditions approximate to those of an ordinary river; scour does go on, but its amount is insufficient to sweep away the silt which has been deposited, not only at the top of the tide, but also during two-thirds of the ebb. Embankments had frequently proved beneficial rather than the reverse; a fact explained by the author's experiments, since the level of the ebb tide would in consequence fall more rapidly, and the point at which the water at the bottom began to move would be reached at an earlier period. Again, the process called "dockising," or damming a river at its mouth, had frequently been condemned on account of supposed injury to the river itself, or even to the estuary in which it flowed, but, as would appear from this paper, without foundation. The results would exercise an important influence on other cases, both of theory and practice.

A SHOCK of earthquake is reported on the night of April 28 from Sicily and the province of Reggio di Calabria, and as far as Catanzaro and Monteleone.

WE learn that M. Alphaud, the Director of Public Works in Paris, has in his hands the tender of Siemens and Co. for constructing an electrical railway from the Place de la Concorde to the interior of the Electrical Exhibition. M. Alphaud has given his adhesion to the request, which will be sent with his recommendation to the Commission of Sewers appointed by the Municipal Council, that when the Exhibition shall be closed, the railway will be kept running in the Champs Elysées.

MR. PREECE has been spending a few days in Paris in order to report on the electric establishments and experiments which are being made in that city. He inspected the electric conductors of several large monuments, visited the telephonic exchanges, the Méritens factory, where are being built the magneto-electric engines ordered by the French Lighthouses Administration and the Trinity House, &c., &c.

THOSE interested in sanitary matters should see the Preliminary Report to the U.S. National Board of Health on the Relation of Soils to Health, in the supplement to the *Bulletin* of the Board for April 16. The special point reported on is the Filtering Capacity of Soils, by Messrs. R. Pumpelly and G. A. Smith.

A MEETING of the Yorkshire Geological and Polytechnic Society was held on April 27 at the Royal Institution, Hull, under the presidency of Mr. A. K. Rollit, LL.D., F.R.A.S., &c., ex-president of the Hull Literary and Philosophical Society. There was a fair attendance, including representatives from several parts of Yorkshire. A brief introductory address on recent advances in physical science was made by Dr. Rollit, after which Mr. G. W. Lamplugh, F.G.S., read a paper on "The Peculiar Intermingling of Gravel and Boulder Clay in some Sections near Bridlington." Mr. J. W. Davis, F.G.S., hon. sec., then read and remarked upon papers by Mr. A. G. Cameron of H.M. Geological Survey, on "The Subsidences above the Permian Limestone between Hartlepool and Ripon," and Mr. J. E. Clark, B.A., on "A Deep Glacial Section at the Friends' Retreat at York." Dr. James Geikie, F.R.S., was present at the meeting, and made some observations on the subject of geology generally. In the afternoon the Society made a geological excursion to the east coast at Withernsea and to the gravel-pits at Kelsey Hill near Burstwick, at the former of which places Dr. Geikie delivered a geological address.

ABOUT twenty minutes to eleven on Monday night, owing to some accident at present unexplained, the electric lights on the Brush system, one of the three with which experiments are at present being made in the City, were suddenly extinguished, leaving a large portion of the City in total darkness. The area over which the Brush light has been placed extends from Blackfriars Bridge, up Ludgate Hill, to St. Paul's Churchyard, and down Cheapside as far as Queen Street and King Street. Fortunately the old gas-lamps remain in their places while the electric light experiments are being made, and orders were quickly given for these to be lighted. Every attempt was made by those in charge of the Brush light to restore the connection, and for a

few minutes it seemed as though they had succeeded; but this only lasted for a very short time, and it was soon seen that something had gone hopelessly wrong.

THE following excursions have been arranged] for by the Geologists' Association:—To Croydon, Shirley and the Addington Hills, May 7; to Grays, Essex, May 14; Sheppey, May 23.

MR. LANT CARPENTER asks us to state that in his article on Niagara in NATURE, vol. xxiii. p. 511, he attributed the article on the "Music of Niagara," in *Scribner's Magazine* for February, 1881, to Mr. Eugene Schuyler, whereas the author was Mr. Eugene Thayer, of Tremont Street, Boston, Mass.

THE additions to the Zoological Society's Gardens during the past week include a Silver Fox (*Canis fulvus*, var. *argentata*) from North America, presented by Mr. Robert Hunt L. B. Lydston Newman; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mrs. J. S. Henderson; a Goldfinch (*Carduelis elegans*), British, a Snow Bunting (*Plectrophanes nivalis*), European, presented by Mr. John Fletcher; an Eyed Lizard (*Lacerta ocellata*), South European, presented by Mr. James Wellford; an Indian Cobra (*Naia tripudians*) from India, presented by Mr. A. H. Jamrach; a Ludio Monkey (*Cercopithecus ludio*) from West Africa, on approval; two Humboldt's Lagothrix (*Lagothrix humboldti*), two Matamata Terrapins (*Chelys matamata*) from Upper Amazons, a Green-billed Toucan (*Ramphastos dicolorus*) from Guiana, three Saddle-billed Storks (*Xenorhynchus senegalensis*) from West Africa, three Roseate Spoonbills (*Platalea ajaja*) from South America, a Japanese Teal (*Querquedula formosa*) from North-East Asia, three Magellanic Geese (*Bernicla magellanica*) from the Falkland Islands, purchased; a Reeves' Muntjac (*Cervulus reevesi* ♂) born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET QUESTION.—It may be remembered that when the late Prof. Watson claimed to have seen an unknown object near the star θ Cancri during the totality of the eclipse of July 29, 1878, it was objected with respect to his supposition as to its being an intra-Mercurial planet, that he had not anywhere mentioned his having seen the object at the same time as the star, or as well as the star, consequently that his circle-reading may have really applied to the latter. From Prof. Watson's official report on his observations, just published with many others, by the Superintendent of the Naval Observatory at Washington, it appears that this objection is no longer valid. Prof. Watson writes: "Between the sun and θ Cancri, and a little to the south, I saw a ruddy star whose magnitude I estimated to be $4\frac{1}{2}$. It was fully a magnitude brighter than θ Cancri, which I saw at the same time, and it did not exhibit any elongation, such as might be expected if it were a comet in that position. The magnifying power was 45 and the definition excellent. My plan did not provide for any comparison differentially with a neighbouring star by micrometric measurement, and hence I only noticed the relation of the star to the sun and θ Cancri." It is difficult to understand how the observation can be explained, except by admitting the existence of an unknown body in the vicinity of the star, or by imputing to the deceased astronomer a want of *bona fides*, for which we do not believe there is the slightest excuse; he was too well known and respected to allow of such an imputation.

The solar eclipse of May 17, 1882, will afford the next opportunity of repeating observations of the kind made by Prof. Watson in 1878, but the duration of totality will nowhere exceed 1m. 48s., and in the most accessible portion of the central line will amount to 1m. 15s. only.

THE TRANSIT OF MERCURY, NOVEMBER 7, 1881.—With the positions of the Sun and Mercury given in the *Nautical Almanac* from Leverrier's Tables, and the diameters of those bodies obtained by the same astronomer from the discussion of former transits, the following will be the geocentric Greenwich times and the reduction-formulæ for the internal contacts during the transit of Mercury on November 7 in the present year:—

First internal contact, Nov. 7, 10h. 18m. 15s.·8 + [1·4205] r sin l − [1·5404] r cos l cos (L + 55° 34′·2).

Last internal contact, Nov. 7, 15h. 35m. 28s.·2 + [0·9136] r sin l + [1·6302] r cos l cos (L − 35° 23′·2).

Where r is the radius of the earth at the place, l its *geocentric* latitude, and L the longitude from Greenwich, reckoned towards the east. The quantities in square brackets are logarithms of seconds of time.

It will be seen that the transit will be invisible in this country, and will be best observed from the Australian observatories. At the Cape of Good Hope the sun will not rise till about four minutes after the second internal contact has taken place. At Madras he will be above the horizon before the middle of the transit, which ends there about 20h. 59m.

As an example of the use of the above formulæ we may compute the local mean time of first internal contact for the Observatory at Melbourne. The longitude of this observatory is 9h. 39m. 54s. 8 E., or in arc 144° 58′·7, and the geographical latitude is − 37° 49′·9. From Bessel's Table in the *Berliner Jahrbuch* for 1852, we find log. r = 9·9999, and the reduction of latitude, 11′·1, so that l = − 37° 38′·8.

Constant	+1·4205	Constant +	55° 34′·2	Constant	−1·5404
r	9·9999	Long. ...	144° 58′·7	r	9·9999
sin l ...	−9·7859			cos l ...	+9·8986
		A... ...	200° 32′·9	cos A ...	−9·9715
	−1·2063				+1·4104
	−16s.·08				+25s.·73
					−16s.·08
					+ 9s.·65

	h	m	s
Geocentric time ...	10	18	15·8
G.M.T.	10	18	25·5
Longitude E.	9	39	54·8
Melbourne mean time	19	58	20·3

A NEW COMET.—The Smithsonian Institution telegraphs the discovery of a comet by Mr. Lewis Swift on the morning of the 2nd inst. in the constellation Andromeda; motion slow, southwards.

GEOGRAPHICAL NOTES

WE understand that the Council of the Geographical Society have recently voted a contribution of 100*l*. towards the expenses of the Palestine Exploration Fund's Expedition to Eastern Palestine.

THE May number of the Geographical Society's *Proceedings* contains Mr. James Stewart's paper on Lake Nyassa and the water-route to the lake-region of East Africa, with a map of the north end of Nyassa. A note afterwards given embodies recent information from Livingstonia as to a serious depression in the level of the lake, which threatens to make the south end, as well as the Upper Shiré, unnavigable, and by consequence detract very seriously from the value of this route. Col. Tanner's paper on Kafiristan is also given, with a map of that and the adjacent region. Some account is furnished of Dr. Junker's journey in the Nyam Nyam country from the traveller's letters to Dr. Schweinfurth and Signor Gessi. Reference is again made in the Geographical Notes to the late Capt. Wybrants' expedition to South-East Africa, but complete details of its diastrous ending are still wanting, which seems the more remarkable as the lamented leader died as far back as November 29, 1880. An interesting note deals with Dr. Kirk's recent visit to the Dar-es-Salaam district of East Africa, and it is also stated that the Rev. T. J. Comber is about to make another attempt to reach Stanley Pool by the Makuta route, while one of his companions will follow the line of the Congo. The remaining notes refer to Major J. Biddulph's work on the tribes of the Hindu Kush, and Père Desgodins' labours in the cause of geography on the eastern and southern frontier of Tibet.

AT the evening meeting of the Geographical Society on Monday next Mr. E. Whymper will read a paper describing the geographical results of his journey among the Andes of Ecuador.

THE new volume of the *Geographisches Jahrbuch* is of great value to scientific geography in its most comprehensive acceptation. Dr. Behm has been compelled to retire from the editorship, and is succeeded by Prof. H. Wagner of Göttingen, who, we have no doubt, will maintain the *Jahrbuch* at its previous high standard. The first part is devoted to the various geographical sciences. The first paper is by Prof. Zöppritz, "On the Present Standpoint of Physical Geography." This is followed by an account of recent researches in geographical meteorology by Dr. Haan; and papers on the Geographical Distribution of Animals and of Plants by Dr. Schwarda and Dr. Oscar Drade respectively. Prof. Bruhns summarises recent work in Europe in the measurement of degrees, and Herr Anwers gives the latitudes and longitudes of 144 astronomical observatories. Prof. F. v. Fritsch brings together recent investigations on the geographical distribution of geological formations all the world over; while Dr. v. Scherzer has his usual account of the world's commerce, and Dr. Gerland summarises recent advances in ethnological research. In the second part, which deals with general matters, Dr. Wagner has a thoughtful and useful article on the development of *Methodik* in geography; while, along with Herr Wichmann, he brings together a good deal of information on geographical societies, congresses, and journals. Thus it will be seen the new volume contains much matter of permanent interest.

WE have received Nos. 6, 7, and 8 (in one thick volume) of the *Bulletin* of the Union Géographique of the North of France. M. Leon Lacroix has a long paper describing a plan for the exploration of Central Africa, by the Wellé, a project we should much like to see carried out. M. Alf. Renouard, in a paper on the Geography of Flax, brings together much curious and useful information. Dr. Harmand's paper on the Races of Indo-China ought to interest ethnologists. Among the other contents are papers on the French in Indo-China, by M. Suerus; Syria in 1860, by M. Huberdeaux; and a note on the Isthmus of Panama, by M. V. Duburcq.

THE principal paper in the January number of the *Bulletin* of the Paris Geographical Society (just received) is one of much research, by M. Dutreuil de Rhins, on the routes between China and India.

THE *Mittheilungen* of the Vienna Geographical Society contain an account of a botanical excursion in the north of the Caucasus, by M. P. Muromtzoff, and a paper on the Floods of the Winter of 1880–81, by Baron Stefanovic von Vilovo.

UNDER the title of "Istruzioni Scientifiche dei Viaggiatori," the Italian Ministry of Agriculture, Industry, and Commerce have issued a very full and carefully compiled manual of information and instruction for travellers, edited by Signor Arturo Issel, with the collaboration of several specialists. The manual seems to us to combine the best features of all its predecessors in other languages, and ought to be of real service to all travellers who know Italian. It includes astronomy, meteorology, geography, and topography, deep-sea exploration, geology and palæontology, anthropology and ethnology, zoology, botany, and mineralogy. It is published under the auspices of the Italian Geographical Society.

HEFT 4 of Band ii. of the *Mittheilungen* of the German African Society contains communications from Dr. Büchner and Herr Flegel. The former has been doing a considerable amount of exploration between the capital of Muato Janvo's kingdom and the Congo, though his progress has been hindered by the usual African difficulties. His collections have been very numerous; unfortunately several boxes of them have been lost in the vessel in which they were being brought home, which was wrecked during the recent gales in the Channel. Herr Flegel has been doing some successful work on the lower and middle Niger.

PROF. GIUSEPPE DALLA VEDOVA has published the address he gave on the inauguration of the Chair of Geography at the University of Rome in November last. The subject is the Popular and the Scientific Conception of Geography. He shows that while the popular idea has its uses, the scientific conception is the only basis on which the subject can be studied with profit. He insists on the fact which has been frequently expounded in these pages, that geography has really become a sort of meeting-place for all the sciences, and that while topography may form the groundwork, it requires a knowledge of most of the physical and biological sciences to understand how the surface of the earth has reached its present condition.

DR. OSCAR LENZ has arrived in Berlin, where he has been lecturing on his journey across the Sahara to Timbuctoo.

AFTER all there seems to be little doubt that the news of the massacre of Col. Flatters and the other members of the Trans-Saharan Expedition is too true. Of course the project of a railway across the Sahara must be abandoned, in the meantime at least.

FROM *Les Missions Catholiques* we learn that news has at length been received respecting Père Law's expedition from Gubuluwayo, in Matabele Land, to Umzila's country, which was known to have met with some misfortune on the road. After passing the Insimbi Mountains the party reached the Great Sabi River, on the lower course of which we presume that Capt. Phipson-Wybrants died. The expedition journeyed, with their heavy waggon, along the left bank of the Sabi, meeting with country so difficult to traverse that in some parts they had to hew out of the rock a road for their waggon. Progress in this manner was terribly slow, and when that part of the Mashona country which owns some sort of allegiance to Umzila was reached, difficulties increased, as the natives did all they could to hinder their passage. Eventually on August 7, in a rugged pass where, surrounded by Mashonas, the missionaries were all doing their utmost to cut a road for the waggon, Père Wehl, by an accident not very clearly explained, got separated from his companions and was never seen again, though later news seems to have reached Gubuluwayo of his safety among a friendly tribe. Père Law and the rest of the party not unnaturally took fright at this, and leaving their waggon, escaped from their savage tormentors in the night. After about a fortnight's march they contrived to reach Umzila's kraal in a state of great exhaustion from fever and fatigue. They of course had to abandon almost all their property with the waggon, but further supplies have since been sent to them from Gubuluwayo.

PÈRE DEPELCHIN, the head of the mission station at Gubuluwayo, has been for some time absent on an expedition beyond the Zambesi, and from his long silence it was thought that he too must have met with some serious accident. He appears, however, to have reached in safety an out-station at Tati in Matabele Land, but no account of his adventures has yet been received.

MR. MCCALL, of the Livingstone (Congo) Inland Mission, is said to have formed a station at Manyanga, some 200 miles up the Congo, above the Yellala Falls, and he has no doubt about being able to reach Stanley Pool this year. The comparatively rapid progress thus made by following the right bank of the Congo will probably induce the Baptist Missionary Society's party at San Salvador to alter their tactics and follow the line of the river, instead of wasting their energies in fruitless attempts to make their way by land through the hostile Makuta towns to Makwekwe, on the left bank of the Congo, above Manyanga, and so on to Stanley Pool.

THE French Geographical Society held its annual meeting on Friday, April 29, when Admiral La Roncière le Noury was elected president. On the occasion of a proposal to erect a monument to Col. Flatters and his companions it was resolved to inscribe on tablets the names of all the martyrs of geography who have lost their lives in any exploration in which the French Geographical Society has been interested.

DR. O. F. VON MÖLLENDORFF has just published separately at Berlin (Reimer) two maps which have been drawn from his surveys by Dr. Kiepert for the Berlin Geographical Society's *Zeitschrift*. One is an original map of the hill-country north and west of Peking, while the other embodies routes in the Chinese province of "Dshy-li," and environs of Tientsin. Dr. Möllendorff, as we have before mentioned, claims to be an authority on the subject of the transliteration of Chinese sounds, but we doubt if many people in this country will recognise in "Dshy-li" the name of the metropolitan province (Chihli), and yet the maps are issued with English titles.

THE FUTURE DEVELOPMENT OF ELECTRICAL APPLIANCES[1]

THE lecturer began by referring to our obligations to laboratory workers and the necessity for a larger endowment of original research. The applied science of the future lies in-

[1] Abstract of a lecture delivered by Prof. John Perry at a meeting of the Society of Arts, March 24.

visible and small in the operations of men who work at pure chemistry and physics, and it is especially true of laboratory work in electricity that every day a man sees new lines of research opening up before him which his resources do not allow him to follow up.

In the applied science of electricity certain fixed laws tell us much about the future which is not generally known; and it is first necessary to become acquainted with these laws if we would speak of this future. By numerous experiments the lecturer showed that electricians are dealing with measurable things, and he gave in wall-sheets such information as seemed sufficient to give exact ideas in this matter to a popular audience. These wall-sheets had also been put in a printed form and circulated among the audience. The following is an example:—

WALL-SHEET II.—ELECTRICAL MAGNITUDES (SOME RATHER APPROXIMATE)

Resistance of

One yard of copper wire, one-eighth of an inch diameter	0·002 ohm.
One mile ordinary iron telegraph wire	10 to 20 ohms.
Some of our selenium cells	40 to 1,000,000
A good telegraph insulator	4,000,000,000,000

Electromotive force of

		Volts.
A pair of copper-iron junctions at a difference of temperature of 1° Fahr.	=	0·000,01
Contact of zinc and copper	=	0·75
One Daniell's cell	=	1·1
Mr. Latimer Clark's standard cell ...	=	1·45
One of Dr. De La Rue's batteries ...	=	11,000

Lightning flashes probably many millions of volts.

Current measured by us in some experiments:—

Using electrometer	= almost infinitely small currents.
	Weber.
Using delicate galvanometer	= 0·000,000,000,040
Current received from Atlantic cable, when twenty-five words per minute are being sent	= 0·000,001
Current in ordinary land telegraph lines	= 0·003
Current from dynamo machine ...	= 5 to 100 Webers

In any circuit, *current* in webers ... = *electromotive force* in volts ÷ *resistance* in ohms.

WALL-SHEET III.—RATE OF PRODUCTION OF HEAT CALCULATED IN THE SHAPE OF HORSE-POWER

In the whole of a circuit = *current* in webers × *electromotive force* in volts ÷ 746.

In any part of circuit = *current* in webers × *difference of potential* at the two ends of the part of the circuit in question ÷ 746.

Or, = square of current in webers × resistance of the part in ohms ÷ 746.

The distinction which must be made between *electricity* and *electrical energy* was dwelt upon. A miller does not merely speak of the quantity of water in his mill-dam; he has also to consider the height through which it can fall. A weight of one thousand pounds falling through a distance of one inch represents the same energy, that is, gives out the same amount of work in falling as one pound through one thousand inches. A mere statement then of the quantity of electricity given out by a machine is insufficient; it is also necessary to state what is the height or difference of potential through which it is falling. The quantity of electricity in a thunder-cloud is comparatively small, but the difference of potential through which this quantity passes when discharge occurs is exceedingly great. So it is with the two factors of the electrical energy developed by this glass machine. The quantity of electricity obtainable from this machine is comparatively small, but it is like a small quantity of water at an exceedingly great height, whereas in all these other machines we have, in the analogy of the miller, a very great quantity of water and a very small difference of level. I put this water analogy before you because you have all more or less exact notions about water, and because, within certain limits, the analogy is a very true one. I have traced it more fully in the wall-sheet. Of this and the other wall-sheets each of you possesses printed copies.

WALL-SHEET I.

We Want to Use Water.	*We Want to Use Electricity.*
1. Steam-pump burns coal and lifts water to a higher level.	1. Generator burns zinc, or uses mechanical power, and lifts electricity to a higher level or potential.
2. Energy available is, amount of water lifted × difference of level.	2. Energy available is, amount of electricity × difference of potential.
3. If we let all the water flow away through channel to lower level without doing work, its energy is all converted into heat because of frictional resistance of pipe or channel.	3. If we let all the electricity flow through a wire from one screw of our generator to the other without doing work, all the electrical energy is converted into heat because of resistance of wire.
4. If we let water work a hoist as well as flow through channels, less water flows than before, less power is wasted in friction.	4. If we let our electricity work a machine as well as flow through wires, less flows than before, less power is wasted through the resistance of the wire.
5. However long and narrow may be the channels, water may be brought from any distance, however great, to give out almost all its original energy to a hoist. This requires a great head and small quantity of water.	5. However long and thin the wires may be, electricity may be brought from any distance, however great, to give out almost all its original energy to a machine. This requires a great difference of potentials and a small current.

After showing, by passing currents from two large Gramme machines through certain resistances and lamps, that electrical energy may be sent to a distant place and there converted into heat and light, the methods taken at the City and Guilds of London Institute for simultaneously measuring mechanical work, currents of electricity, resistance, the candle-power of electric lamps, &c., were described, the dynamometers, photometers, &c., being exhibited, as well as diagrams showing their construction. Actual measurements were made of the strengths of currents and the candle-power of an electric light. Many of the contrivances in use were invented by the lecturer and his friend Prof. Ayrton.

The transmission of mechanical power to a distance through the agency of electricity was illustrated by a number of experiments; the driving of a lathe and other machines, and proof that the motor which gives out power at the distant place produces a back electromotive force opposed to that of the generator.

"Now what do these examples show you? They show that if I have a steam-engine in my back yard I can transmit power to various machines in my house, and if you were to measure the power given to these machines you would find it to be less than half of what the engine driving the outside electrical machine gives to it. Further, when we wanted to think of the heating of buildings and the boiling of water, it was all very well to speak of the conversion of electrical energy into heat, but now we find that not only do the two electrical machines get heated and give out heat, but heat is given out by our connecting-wires. We have then to consider our most important question. Electrical energy can be transmitted to a distance, and even to many thousands of miles, but can it be transformed at the distant place into mechanical or any other required form of energy, nearly equal in amount to what was supplied? Unfortunately I must say that hitherto the practical answer made to us by existing machines is 'No'; there is always a great waste due to the heat spoken of above. But fortunately we have faith in the measurements, of which I have already spoken, in the facts given us by Joule's experiments and formulated in ways we can understand. And these facts tell us that in electric machines of the future, and in their connecting-wires, there will be little heating, and therefore little loss. We shall, I believe, at no distant date, have great central stations, possibly situated at the bottom of coal-pits, where enormous steam-engines will drive enormous electric machines. We shall have wires laid along every street, tapped into every house, as gas-pipes are at present; we shall have the quantity of electricity used in each house registered, as gas is at present, and it will be passed through little electric machines to drive machinery, to produce ventilation, to replace stoves and fires, to work apple-parers, and mangles, and barbers' brushes.

among other things, as well as to give everybody an electric light.

"Probably you think it very strange that I should show you the inefficiency of electric transmission of energy, and then make this very bold assertion. Well, the fact is that the ordinary electrical machines in use have not been constructed with a view to economy. They have been constructed to show that brilliant lights and considerable power may be produced from small machines. They have, at a comparatively small cost, attracted attention to the fact that electricity is an important agency. In so far they have done well; but on the other hand they gave rise to the well-known assumption that 50 per cent. of the mechanical power given to the generator was the maximum amount which could be taken from the motor. The true solution of the problem of transmission of power was, I believe, first given by Prof. Ayrton in his Sheffield British Association lecture. It had been supposed that to transmit the power of Niagara Falls to New York a copper cable of enormous thickness would be needed. Mr. Ayrton showed that the whole power might be transmitted by a fine copper wire, if it could only be sufficiently well insulated. He also showed that instead of a limiting efficiency of 50 per cent., the one thing preventing our receiving the whole of our power, is the mechanical friction which occurs in the machines. He showed, in fact, how to get rid of electrical friction. I will briefly give you our reasons. A machine at Niagara receives mechanical power, and generates electricity. Call this the generator, and remember that Wall-sheet III. teaches us that the mechanical power is proportional to the electromotive force produced in the generator, multiplied into the current which is actually allowed to flow. Let there be wires to another electric machine in New York, which will receive electricity, and give out mechanical work, as this machine does here. Now I showed you a little while ago that this machine, which may be called the motor, produces a back electromotive force, and the mechanical power given out is proportional to the back electromotive force multiplied into the current. The current, which is of course the same at Niagara as at New York, is proportional to the difference of the two electromotive forces, and the heat wasted is proportional to the square of the current. You see then, from Wall-sheet III., that we have the simple proportion—power utilised is to power wasted, as the back electromotive force of the motor is to the difference between electromotive forces of generator and motor. This reason is very shortly and yet very exactly given in Wall-sheet IV., a printed copy of which you all hold in your hands."

WALL-SHEET IV.

Let electromotive force of generator be E; of motor F. Let total resistance of circuit be R. Then if we call P the horse-power received by the generator at Niagara. Q the horse-power given out by motor at New York, that is, utilised. H the horse-power wasted as heat in machines and circuit. C the current flowing through the circuit.

$$C = \frac{E - F}{R}$$

$$P = \frac{E(E - F)}{746 R}$$

$$Q = \frac{F(E - F)}{746 R}$$

$$H = \frac{(E - F)^2}{746 R}$$

$$Q : H :: F : E - F.$$

"To put it more shortly still, the power wasted is proportional to the square of the current flowing, whereas the power utilised is proportional to the current, and also to the electromotive force of the motor. The greater, then, we make the electromotive forces, the less is the loss of power in the whole operation. Perhaps you will see this better from the water analogy. A small quantity of water flowing through a water-main may convey a large amount of energy, if it only has sufficient head. The frictional loss of power is independent of the head, but depends very much on the quantity of water. In the model before you is the water analogy. Here is a reservoir, which I shall call A, kept filled with water by a steam pump, which draws the water from the sea-level, which I shall call K. Water flows from reservoir A to distant reservoir B, where it drives a turbine giving out work due to its head B K. The current from A to B, through the communicating pipe, is the same always, so long as A and B are at the same difference of level, and therefore the frictional loss of energy is always the same, whereas the work utilised from B, by driving the turbine, increases proportionally to the height of B above sea-level. The result, then, to which the above laws led us was that for the future development of the transmission and distribution of electric energy it will be necessary to use electric machines of great electromotive force. Indeed so important must this principle become that we believe there is a future in this direction for the employment of even plate electrical machines, such as that of Holtz."

Then followed a discussion of methods of obtaining great electromotive force. Mr. Perry's own ways of carrying out these ideas are shown in his own dynamo-machine, which is large, has great speed, has no iron in its movable part, and has a commutator of small frictional resistance. Electric lighting and heating, telephones, and electric railways of the future were all spoken of as illustrations of the transmission of energy by electrical means, and as such they must be governed by the above principle.

It was then shown experimentally that electrical energy may be stored up in considerable quantities in an available form for future use, and the bearing of this fact on the future utilisation of great but variable natural sources of power, such as the wind and tide, was dwelt upon.

The remainder of the lecture was devoted to the importance of the principle of recurrent effects; one illustration was given as follows:—"If I very much alter the magnetic field in this telephone, by bringing a powerful magnet near it, with great care in listening I hear the faintest sigh, due to the diaphragm settling itself into a new position, its vibrations dying away as it does so; and if I brought a small magnet near, I should hear nothing. And yet the change of magnetism which produces the loud telephonic effects which we listen to is almost infinitely smaller. Why is this? It is due to the rapid recurrence of the effects. Now you are all aware of the importance of the telephone as a method of communication; I believe that a much greater importance is in store for it as a laboratory appliance."

The photophone and the method by means of which Messrs. Ayrton and Perry determined the index of refraction of ebonite, finding its square to be roughly the same as the mean value of its measured specific inductive capacities: the use of a powerful sub marine source of musical sound as a coast-warning, which might be heard in a ship well above all other sounds, and the experiments which have been made by the lecturer and his colleague in this direction: these and other matters were discussed as examples of the use of the principle of recurrent effects. The lecture concluded by an account illustrated by experiments of Mr. Edward Bright's method of de-electrifying woollen yarn, and of Messrs. Ayrton and Perry's plan for seeing by electricity what is occurring at a distant place. A selenium cell moving over an image at, say, York, gave corresponding light and shade to corresponding parts of a screen at, say, London. Mr. Perry's York image was very simple, being a series of black, grey, and white squares, which were faithfully reproduced on the distant screen.

MECHANICAL RESEARCH

IT will be remembered that some time ago the Institution of Mechanical Engineers appointed a Committee to examine into three selected questions of research in matters pertaining to their profession. These researches are still in progress, but preliminary reports have been issued by the Committee, of which we propose to give a brief account.

The Hardening and Tempering of Steel.—One or two letters on this subject have lately appeared in our columns, and allusion has been made to the report by Mr. Wm. Anderson, presented to the Committee who were appointed specially to investigate this difficult question. Mr. Anderson's report, which contains much useful information in a comparatively small compass, is itself too long for our pages. We therefore give the following *résumé* of the question, taking Mr. Anderson's report as our basis:—

Whilst the theory of this subject is in a very vague and uncertain condition, the facts are exceedingly well known, and are daily applied in almost every department of arts and manufactures. Wherever steel tools are used it is necessary that they should be hardened and tempered; since the ordinary tool-steel, as supplied chiefly from Sheffield, is too soft for cutting and

abrading purposes. It is known however that if a piece of this steel be heated, and then suddenly cooled (generally in a bath of water or oil) it becomes much harder, not only on the surface, but throughout, provided its thickness be not excessive. The greater the range of this cooling (in other words, the difference between the temperatures of the steel and the bath at the first moment of "quenching") the more intense is the hardening, but at the same time the greater the brittleness of the piece. Hence it is always desirable that the range of cooling should be as small as is consistent with the steel acquiring that degree of hardness which is essential for the work it has to do. This condition is secured by the further operation of tempering. In this process the steel is first hardened to excess by rapid cooling, then re-heated with great care to a certain temperature corresponding to the purpose for which it is intended, and then quenched again from that temperature. The particular point at which to stop the re-heating is recognised by one particular hue in what are called "the colours of tempering," *i.e.* a fixed range of colours, commencing with pale yellow and ending with dark blue, which the steel is always seen to assume in succession as its temperature gradually rises. Thus, if the article in question be a sword it is heated to a bright blue; if it be a cold chisel it is stopped at a brownish orange.

The various attempts to explain these singular facts (at least on the part of French and English metallurgists) are set forth in the Committee's Report. In the first place it seems now to be generally held that pure steel is a compound of iron and carbon only, and that these two elements exist, not in a state of chemical combination (forming some definite carburet of iron), but of intimate mechanical mixture, such as chemists call by the name of "solution." The question next arises, What is the exact condition of each of these independent elements. In the case of very soft, or "grey," cast-iron, it is known that the carbon is not wholly in solution, but occurs partly in molecules of pure graphite. Following this hint M. Jullien has advanced the theory that molten cast-iron, or molten steel, is a solution of liquid carbon in liquid iron; that under slow cooling part of the carbon separates as graphite, while the remainder continues in solution; but that with rapid cooling this separation does not take place, and the whole of the carbon crystallises, forming, when cool, a "solution" of crystallised carbon in amorphous iron. This view of the difference between hard and soft cast-iron, or hard and soft steel, is accepted by Caron, Åkerman, and others, partly on the ground that hardened steel dissolves completely in hydrochloric acid, while the same steel, after annealing, will leave a residue of insoluble carbon. Jullien, however, goes beyond this, and would explain the whole phenomena of hardening on the same principle. He holds that carbon liquefies in presence of red-hot iron, and is absorbed by it; that if the mixture is cooled slowly the carbon remains amorphous, but if cooled quickly the carbon crystallises in the diamond form; and thus hard steel is iron set in a matrix of diamond. This theory, though ingeniously supported, labours under the difficulty that the liquefaction of carbon has never been otherwise achieved; and also that it gives no explanation whatever of the phenomena of tempering, especially the characteristic colours. On the other hand, Barba and Åkerman hold that the hardening of steel is due to the severe compression produced in the outer layers by the contraction in rapid cooling; this compression at once retaining a greater proportion of carbon in solution, and rendering the whole mass more physically dense and compact. But the Report points out that the outside layers, which are the hardest, are brought into a state of tension, not compression, owing to their inability to contract over the hotter mass inside.

For these reasons the Committee have rejected both these theories, and propose one of their own, due apparently to Mr. William Anderson, but suggested by the experiments of Edison on platinum wire, an account of which appeared in NATURE, vol. xx. p. 545. They refer to the generally-accepted fact that ordinary steel contains a certain proportion of occluded gases (consisting, according to Müller, of hydrogen, nitrogen, and carbonic oxide). They suggest that the application of heat causes these gases to be expelled through minute fissures which open in the steel, as they opened, according to Edison, in the platinum wire observed by him; and that sudden cooling prevents the re-absorption of what has been expelled, perhaps actually tends to expel the remainder. By the loss of these gases the metal becomes denser and harder than before. If the metal be now expanded by gentle heating, the fissures open, re-absorption begins; and the various changes which the surface undergoes during this process are marked by the succession of colours which are characteristic of tempering.[1] The Committee propose to make a series of experiments to test the truth of this theory, which is certainly ingenious, and if confirmed would go far to remove the difficulties which beset this important subject.

Form of Riveted Joints.—The second Report is written by Prof. W. C. Unwin, of Cooper's Hill, and is on "the best form of riveted joints to resist strain in iron or steel, or in combination." It may be well to explain that by a "riveted joint" is meant the mode of fastening together the strips, or plates, out of which boilers, tanks, girders, and other structures in wrought iron or steel are built up. This mode in its simplest form is as follows:—A row of holes is punched or drilled along the edge of the two plates to be united, and these edges are then made to lap over each other so that each hole in one plate comes fair with a hole in the other. A red-hot rivet (that is a pin with a rounded head) is then passed through both holes, and the end is flattened down by hammering or pressure, so as to form a second head. The two plates are thus pinned together by the rivets, and so long as these remain entire they cannot be separated without tearing across. Such a joint is called a "single-riveted lap-joint." It is obvious that the plate must be greatly weakened by the piercing of the holes; and as a matter of fact it appears that such joints cannot be arranged to give more than about half the strength of the solid plate. To increase this "proportion of strength," as it is termed, the rivets are sometimes arranged in two or three rows, and of course more widely spaced in each row; or the edges of the two plates are simply brought up against each other and secured by either one or two "cover-strips" fitting over and riveted to both.

Each of these forms demands a separate investigation in order to fix its design. Thus a single-riveted lap-joint under a tensile stress may fail in any one of the following ways: (1) the rivet may cut into the plate, enlarging and injuring the hole; (2) the plate may cut into the rivet, and finally shear it off; (3) the part of the plate between the rivet and the edge may break through, allowing the rivet to come away from the plate; (4) the plate may simply tear across along the line of rivets. It is clear that in a perfect joint the dimensions must be such that the resistance to each of these modes of fracture should be the same, and should have its greatest possible value. This could be easily arranged if the absolute resistance of the material to these various forms of stress were accurately known. But the values of these resistances are of course very different for steel and for iron; they also vary considerably, whether in steel or iron, according to the quality, and to some extent according to the thickness. Hence experiments on these values become absolutely necessary before any correct design can be made out.

The course which such experiments should take is fairly sketched out by the author of the Report now before us. A good and uniform quality of iron or steel, as the case may be, should first be selected, both for plates and rivets. The resistance of this material to the various forms of stress should then be carefully ascertained by experiments made both with simple bars or plates, and also with actual riveted joints, so designed that they shall be certain to fail in one particular way. These constants once settled, it is easy to calculate for any description of joint the dimensions which will give the highest proportion of strength. A joint should then, and not till then, be prepared, having exactly these dimensions, and a few others having dimensions varying slightly from these in each direction. If, on testing, the first joint proves to gives the highest breaking strain of the set, the correctness of the whole investigation will be established.

Unfortunately the method thus sketched out has not hitherto been adopted. The immense practical importance of the subject (for the money expended yearly on riveted structures may be counted by millions) has indeed brought forward a host of experimenters; and the mere classification and abstracting of their results occupies no less than sixty octavo pages of this Report. But almost without exception they seem to have begun at the wrong end, *i.e.* they started with making a riveted joint of what they chose to consider to be the best design, and then pulled it asunder. In addition, scarcely any of their experiments have been made with the care and accuracy, or on the scale, which the subject demanded. In fact it is not going too far to say that 90 per cent. of these experiments are only injurious, as

[1] The Report supposes that the colours of tempering are due to diffraction, not to interference: this does not seem to be in accordance with the facts, but it also does not seem to be absolutely required by the explanation.

cumbering the ground, and that the remaining 10 per cent., if useful, are very imperfect. One instance of the evil done by such means will suffice. Sir Wm. Fairbairn, to whom is due the credit of being the earliest labourer in this field, experimented on certain single- and double-riveted joints, and found that the "proportion of strength" in the case of the former was 56 per cent., and in the latter 70 per cent. of the solid plate. These figures, which of course applied only to the particular designs tested, have been repeated in almost all manuals of engineering as if they were universally true; disregarding the obvious fact that a double-riveted joint could be made just as weak as a single-riveted one, by simply spacing the holes in the outside row at the same distance.

The Committee wisely determined to throw aside the voluminous labours of their predecessors, and begin *de novo* a connected series of experiments, based on the true and scientific method described above. We cannot find space to consider the many collateral points with which these experiments will have to deal, much less to give any account of the results which they are to supersede. These, as embodied in this Report, will remain a singular instance of the lamentable waste of money so continually incurred in engineering experiments. There can be little doubt that less than a tenth of this money, if applied on the scientific and proper method, would have set the whole question long ago at rest, and would now be saving the world, through increased economy of construction, many hundreds per annum for every pound so expended.

Friction.—The last of the three subjects under consideration is that of Friction at High Velocities, the Report on which has been prepared by Prof. Kennedy, of University College, London. This subject offers a curious instance of the influence exercised by a distinguished experimenter, and how his conclusions are pushed, by those who blindly follow his guidance, much further than he himself would attempt to go. About fifty years ago the late General Morin made an important series of experiments, from which the well-known "Laws of Friction" were deduced. One of these laws is that the friction between solid bodies in motion, or dynamical friction, is independent of the velocity. It was overlooked, by those who announced this law, that the experiments were only conducted with certain substances under small pressures and at moderate speeds. General Morin himself, in an interesting letter published in the present Report, expressly states that he had himself always regarded his results, "not as mathematical laws, but as close approximations to the truth *within the limits of the data of the experiments themselves.*" Unfortunately others did not imitate this caution: they asserted everywhere that the law was universal, and by many it is asserted to be so still.

That it is not universal has however been sufficiently proved. At the time of the launch of the *Great Eastern* the late Mr. Froude showed, by experiments on a large scale, that the friction of a vessel on the launching-ways decreased rapidly as the velocity increased. In 1851 Poirée and Bochet showed that the coefficient of friction of railway wheels sliding on rails diminished very rapidly with increase of speed (between limits of 900 and 3600 feet per minute). Recently Capt. Douglas Galton and Mr. Westinghouse made a long series of experiments on the friction of railway-brakes (cast-iron blocks on steel tyres), and their results showed a marked decrease of friction, with increase of speed, within the very large range of 400 to 5300 feet per minute. Prof. Kimball has made experiments at much lower speeds (about 1 to 100 feet per minute), both with pieces of wood and with wrought-iron spindles in cast-iron bearings; and he also finds a rapid decrease of friction with increase of speed. At the lowest possible speeds (0·012 to 0·6 feet per minute) Prof. Fleeming Jenkin finds a similar decrease, pointing to the supposition that the change from statical to dynamical friction is not sudden, but continuous. Lastly, Prof R. H. Thurston has made an elaborate set of experiments on the frictional resistance of lubricated bearings. He arrives at the conclusion that for cool and well-lubricated bearings the coefficient of friction decreases up to a speed of about 100 feet per minute, and afterwards increases with the speed approximately as its fifth root. The details of these experiments do not seem to have been published, so that it is not certain how far this curious result may be taken to hold.

It will be seen that none of these various experiments confirm the universal law deduced from Morin's results, viz. that dynamical friction is independent of velocity. On the contrary, it may be taken as proved for *unlubricated* surfaces (such as railway brakes) that the coefficient of friction diminishes rapidly with increase of velocity; although the exact law of variation and its relation to the pressure on the surfaces is not fully determined. With *lubricated* surfaces the same fact may be assumed to be true at speeds up to 100 feet per minute; but above this, if we accept Prof. Thurston's results, the result is the opposite. It seems clear that the question is ripe for further investigation, which might take the form, first of repeating and extending Thurston's experiments with lubricants, and secondly of ascertaining the law of variation with unlubricated surfaces more exactly than could be done by the aid of the experiments hitherto carried out.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the Downing College Examination in June, 1881, one Foundation Scholarship of the annual value of 80*l.* will be thrown open to all members of the University who have not kept more than six terms. The subjects of this Examination will be Chemistry (Theoretical and Practical), Physics (Heat, Electricity, and Magnetism), Comparative Anatomy, Physiology, and Botany. The Examinations for Minor Scholarships, which are open to all persons who have not entered at any college in the University or who have not resided one entire term in any such college, will be held in Downing College on Tuesday, May 31, and three following days. Further information will be given by Mr. J. Perkins or by the Rev. J. C. Saunders, tutors of the College.

AT a special meeting of the Fellows of Gonville and Caius College, held on the 30th ult., Dr. Paget, F.R.S., Regius Professor of Physic in the University, and Mr. Pattison Muir, Hon. M.A. (Cantab.), were elected Fellows of the Society. Dr. Paget was formerly a Fellow of Caius College.

OXFORD.—In addition to the courses of lectures in Natural Science enumerated last week, the following courses will be held during this term in the University Museum:—Prof. Price will lecture on physical optics, and Prof. Westwood will lecture on the orders of the Arthropoda. In the absence of Prof. Rolleston, who is abroad on account of ill-health, Mr. Jackson will form classes for general catechetical instruction, while classes will be formed by Mr. Robertson for practical microscopy, and by Mr. Thomas for the study of the developing chick.

At the Botanical Gardens Prof. Lawson will lecture on elementary botany (development), and will continue his course on the dissection of plants.

In the Geological Department under Prof. Prestwich, lectures will be given on some of the secondary and quaternary strata. The Professor will have excursions to inspect the sections of the several formations around Oxford, commencing on Saturday, April 30, and to be continued through May. On each preceding Friday he will lecture on the subject of the following Saturday's excursion, or on some other subject of which notice will be previously given. Notice will also be given in the Gazette of the preceding week, and in the Museum, of the places to be visited, hours of meeting, &c.

In a congregation holden on Tuesday, May 3, the proposal to allow selected candidates for the Indian Civil Service to obtain the B.A. degree after two years' residence, was thrown out. An amendment to excuse selected candidates from responsions only was carried by 63 votes to 49.

THE scheme for the establishment of a University College in Liverpool is now almost matured, and it is expected that the College will open for its first session in October next. The donations have reached the sum of 100,000*l.*, and the task of drafting a constitution for the College is now being performed by a special committee. The Earl of Derby has accepted the office of president, the vice-presidents being Mr. Christopher Bushell and Mr. William Rathbone, M.P.

SCIENTIFIC SERIALS

Journal de Physique, April.—Theory of machines with alternating currents, by M. Joubert.—On radiophony (second memoir), by M. Mercadier.—Application of Talbot's fringes to determination of the refractive indices of liquids, by M. Hurion.—Apparatus for projecting images at any distance with a variable enlargement, by M. Crova.—Strong and constant voltaic pile, furnishing residues capable of regeneration by electrolysis, by M. Reynier.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xiv. fasc. vi.—On Chætognatha, by Dr. Grassi.—On the stratigraphical position of the phyllitic zone of Rotzo, and the marine limestones which comprehend it, by S. Taranelli.—On a Cremonian quadratic correspondence between the elements of two ruled spaces, by S. Archieri.—The last introduction of fishes into our lakes, by Prof. Pavesi.—On a freshwater sponge new to Italy, by the same.

SOCIETIES AND ACADEMIES

London

Photographic Society, April 12.—J. Glaisher, F.R.S., president, in the chair.—The following papers were read:—On a Swiss tour with gelatine plates, by W. Dillworth Howard.—On art and photography, suggestions for bringing them into closer connection, by H. B. Berkeley.—On the natural camera, and on uncorrected lenses in photography, by Capt. Abney, R.E., F.R.S. This paper described the natural camera as being the means of taking a photograph without an optical glass—a pin-hole producing the picture, although at a long focus—also that an uncorrected or non-achromatic lens, say an ordinary spectacle lens, if its aperture be reduced to one-fifth of an inch, would bring the wave-lengths of all colours into one perfect focus, but which, being very long, would necessitate prolonged exposures; at the same time this could be met by the use of the modern rapid gelatine plate.

Victoria (Philosophical) Institute, May 2.—A paper upon philosophy as advocated by Mr. Herbert Spencer was read by the Rev. W. Ground. The aim of the paper was to show that the philosophy in question is hopelessly illogical, the "analysis" in direct contradiction to the "synthesis."

Göttingen

Royal Society of Sciences, January 8.—On a proposition of the maintenance of the algebraic relation between the integrals of various differential equations and their differential quotients, by Herr Königsberger.—Report on the polyclinic for ear diseases, by Dr. Bürkner.—On the motion of an electric particle in a homogeneous magnetic field and the negative electric glow, by Herr Riecke.—On the quantity of electricity furnished by an influence-machine of the second kind and its relation to moisture, by the same.—Measurement of the force exerted by earth-magnetism on a linear current conductor capable of rotation, by the same.

February 5.—Influence of heat on the optical properties of boracite, by Herr Klein.—On electrical shadows (third paper), by Herr Holtz.—New representation of spherical functions and related functions by determinants, by Herr Henn.—Remarks on a memoir, by Herr Warburg, on some actions of magnetic coercive force, by Herr Fromme.—Observations in the magnetic observatory, by Herr Schering.

March 5.—On the irreducibility of differential equations, by Herr Königsberger.—Contributions to a knowledge of the optical properties of analcim, by Herr Ben Saade.

Paris

Academy of Sciences, April 25.—M. Wurtz in the chair.—The following papers were read:—On a question of ancient metrology; origin of the English mile, by M. Faye. He inquires into the error (long current) of supposing the mile equivalent in length to a terrestrial arc of one minute. The mile has been probably deduced from Ptolemy's measure, and the error of one-sixth seems to arise from the English geographers having supposed that Ptolemy used the Greek foot, which Eratosthenes used 400 years before, whereas he used the Phileterian foot, which is about 0·36m., the earlier one being 0·27m. Eratosthenes counted 700 stadia to a degree; Ptolemy only about 500.—Examination of materials from the vitrified forts of Craig Phadrick, near Inverness (Scotland), and Hartmannswillerkoff (Upper Alsace), by M. Daubrée. Like the forts in France, that at Craig Phadrick must have undergone heat intense enough for the mica to entirely disappear and the felspar to be in great part fused. The minerals produced at cost of the mica and felspar present evident similarities. The Alsace fort seems to have been composed of brown porphyry, but the crystalline products of heat are similar to those in the other case. The ingenious method of heating was probably transported, not invented independently in different countries. The phenomena elucidate metamorphism.—Meteorite which fell at Louans (Indre-et-Loire) on January 25, 1845, and the fall of which was not published, by M. Daubrée.—Researches on piperidine, by M. Hofmann.—Nodule of chromite in the interior of the meteoric iron of Cohahuila (Mexico), by Prof. Lawrence Smith. He obtained, on analysis, oxide of chromium 62·61, ferrous oxide 33·82. While chromite has long been known in association with meteoric stones, the form of its occurrence here is new. The meteorite contained distinct nodules of two chromiferous minerals.—Observations on phenomena of absorption in lower vegetable organisms, by M. Syrodot. Studying Batrachospermeæ, he has found the organs of absorption to present parallel phases to those better known in the higher groups.—M. Sire presented an instrument for demonstrating Foucault's law of the apparent deviation of the pendulum's plane of oscillation. The apparatus may be used in any latitude.—General theory of transmissions by metallic cables; practical rules, by M. Leauté. The author determines, *inter alia*, the coefficient of working (*fonctionnement*) in telodynamic transmissions, a coefficient which fixes the manner in which a cable behaves under a variation in the force exerted. The idea of equivalence of two transmissions as to working is thus reached. The limits of transmission of force by cables are investigated.—On the essence of licari kanali, or essence of female rosewood, by M. Morin. The composition of this essence from French Guyana appears to be identical with that of Borneo camphor.—On the winter-egg of phylloxera, by M. Mayet. About Montpellier the hatching of the egg has occurred during the whole month of April, and even in the end of March.—Results obtained in phylloxerised vines by a mixed treatment with sulphide of carbon and sulphocarbonate of potassium, by M. Laugier.—M. Faye, presenting the first volume of *Annales de l'Observatoire de Toulouse*, edited by M. Baillaud, said it marked a new era in the history of the provincial observatories, great activity being indicated. The researches of M. Tisserand (predecessor of M. Baillaud) on Saturn's satellites are given. M. Perrotin works out the theory of Vesta; while the zodiacal light, the eclipses of Jupiter's satellites, Saturn's rings, &c., are also studied.—On a class of linear differential equations with doubly periodic coefficients, by M. Appell.—Normal production of three systems of fringes of rectilineal rays, by M. Croullebois.—Causes of disturbance of telephonic transmissions, by M. Gaiffe. Two rods from the same piece of steel (capable of being strongly polarised without being tempered) were placed in a telephone circuit, one of them being first magnetised as much as possible. Striking them similarly produced strong currents from the magnetised rod, but very little current from the other.—On the renal origin of nefrozymase, by MM. Bechamp and Baltus.—On the absorption of mineral waters by the cutaneous surface, by M. Champouillon. The absorption of iron and manganese from the waters of Luxeuil was proved in examination of the urine. It is only after a period of mineral saturation that the minerals appear in the urine.—Remarks on the anatomy of pyrosoma, by M. Joliet.

CONTENTS

THURSDAY, MAY 12, 1881

THE LEPIDOPTERA OF SWITZERLAND

Die Lepidopteren der Schweiz. Von Prof. Dr. Heinrich Frey. 8vo. Pp. xxvi. and 454. (Leipzig: Engelmann, 1880.)

"IN the evening of a busy life I hand over for publication a book which, for a generation, has been suspended before my mind as an object to be attained. When in the summer of 1849, in the sunny days of youth, I, an indefatigable traveller, and passionate lover of *Lepidoptera*, for the first time traversed our glorious country in an extended sense, the idea of a Lepidopterous Fauna of Switzerland was conceived." In this manner the author (who is Professor of Pathology in the Polytechnic of Zürich) commences his introductory remarks. It may be well to state here that Dr. Frey is a Swiss only by adoption (we believe a native of Frankfort on the Main); and this will explain why it was not until 1849 that any extended journey in Switzerland had been made by him. His writings on Swiss *Lepidoptera* are familiar to all European students of the order. Many of these are monographic on special genera or groups, chiefly of the *Micro-Lepidoptera*, and in 1856 a volume of 430 pages—"Die Tineen und Pterophoriden der Schweiz" (a descriptive work)—appeared from his pen. To English readers his short, but very suggestive, paper on the "Tineæ of the Higher Alps," published in the *Entomologists' Annual* for 1858, can scarcely be forgotten. Now, "in the evening of life," as he terms it, he comes before the entomological public with his *magnum opus*. We heartily congratulate him thereupon.

It will readily be understood, from the fact of the work consisting only of one volume, and including therein *all* the Swiss species, that it is not descriptive, outside the very few new species or varieties mentioned in it. It is a carefully compiled catalogue, but very different from a bare dry list of names. To each species is appended a certain amount (not complete) of synonymy, indications of the food-plants of the larvæ, the time of appearance of the imago, and, what is most useful, a carefully analysed list of localities, with special regard to altitude, the latter being of great importance and value in treating of a fauna such as that of Switzerland. More than this, there are copious notes on the numerous varieties into which many species run, not only as concerns influences that locality may exercise in the country itself, but also in connection with the forms of the same species occurring in other parts of Europe.

Our author is strongly conservative in some of his views. He hesitates at species-splitting, unless there appear to be the strongest reasons for such a course. He almost snubs the innovations in Staudinger and Wocke's last European Catalogue (adopting however the sequence) by not accepting their changed nomenclature for the most part, but (for the sake of convenience and identification) placing the "restored" names between brackets.

We cannot presume to give an analysis of individual species and their distribution horizontally or vertically. This is everywhere carefully worked out, and more especially in the butterflies. 2508 species are recognised as Swiss, viz., 171 *Rhopalocera* (or butterflies), 61 *Sphingidæ*, 170 *Bombycidæ*, 439 *Noctuidæ*, 375 *Geometridæ*, 191 *Pyralidæ* and *Crambidæ*, 334 *Tortricidæ*, 727 *Tineina*, 35 *Pterophoridæ*, and 5 *Alucitæ*. If varieties (by some considered species) be added, the total is raised to 2829, the butterflies alone claiming 65 of these forms. In the higher groups the author has derived very great assistance from previous writers, or from the somewhat numerous collectors. But when we come to the *Tineina*, near the end of the work, all this is changed; whole pages, including many species on each, giving only one or two localities for each species, and "Frey" as the sole observer, a striking proof of the neglect under which those minute but intensely interesting forms suffer.

Switzerland attracts tourists from all parts of the world; many of these are entomologists who derive vastly increased pleasure and much profit by collecting insects; many of them publish accounts of the results of their excursions. We think the author might have added to his local information by consulting more of the scattered lists published by foreigners, and more especially many such that have of late appeared in English journals.

Very interesting are the copious introductory observations on the physical features of the country. A table gives thermometrical observations for twenty-six stations, showing a mean temperature ranging from + 12·58 Centigrade at Bellinzona (729 feet) to − 0·19 at the Julier Pass (7040 feet); of greatest cold from − 6·8 at Bellinzona to − 24·7 at Davos; of greatest heat from + 33·1 at Lugano to + 17·6 at the St. Bernhard Hospice. A copious analysis of the vertical range of vegetation, especially of certain trees, is given in connection with *Lepidoptera*. Conifers extend to over 7800 feet (it should be remembered that Paris feet are always intended; thus in English the heights would be seemingly greater) in the Southern Alps. At the Albula Pass (7120 feet) 152 species of *Lepidoptera* were noticed by the author, 44 of which were butterflies. About 8500 feet must be considered the limit for *Lepidoptera*, excluding occasional individuals carried higher by the winds. In a lengthened analysis the author does not lose sight of the probable origin of the Swiss fauna. Whilst showing indications of adoption of the glacial theory, he scarcely commits himself to an opinion, and prefers to give the facts, leaving it to others to build theories thereupon. A most instructive chapter is formed by an analysis of the following headings:—(1) species of the high Alps which occur unchanged in the high north and in other European mountain-ranges; (2) species which, living in the high Alps and the north, show but little change; (3) such as undergo greater change in the north and in other mountain-ranges outside Switzerland; (4) those that occur only in the Alps and in the Arctic Zone; (5) darker coloration in Alpine regions; (6) species introduced from the south. The introduction ends with five pages of about 470 Swiss localities, in double columns, with the altitudes.

The whole of this introductory portion cannot but prove fascinating to all who have visited the Alps, or have had occasion to study the insects of high latitudes. The time is fast approaching when multitudes of our countrymen

will be spreading themselves over the length and breadth, or the height and depth, of Switzerland. Many of these will be naturalists more or less acquainted with the Alpine insect fauna. To those who have already made its intimate acquaintance and are competent to distinguish the majority of the species in the field, this work will prove invaluable for consultation on the spot; to those who leave the determination of their materials until arriving home it will add interest to a not otherwise always enjoyable occupation.

In these latter remarks we have endeavoured to make it attractive to the numerous naturalists who are more or less amateurs. But its claims for careful study by those who are working at geographical distribution as a special branch of science, and at the philosophical speculations that such studies give rise to, are indisputable, and cannot be neglected. R. McLachlan

WORKS OF JAMES MACCULLAGH

The Collected Works of James MacCullagh. Edited by Professors Jellett and Haughton. Dublin University Press Series. (Dublin: Hodges, Figgis, and Co.; London: Longmans, Green, and Co., 1880.)

THE admirable practice of building a monument to departed men of science from the original works they have left behind them is steadily gaining ground, and is now indeed almost the rule. The collected works of Green, Rankine, Wheatstone, Cavendish, Graham, and Clifford have been comparatively recently published; those of Maxwell are being edited.

When the papers republished have been written many years the judgment of editors must be severely exercised; the temptation to point out the relation of the work of the original author to subsequent discovery is great, but the difficulty of deciding how much to add is greater. The late Prof. Maxwell in his edition of Cavendish added much of his own, thereby increasing manyfold the value and interest of the book, and at the same time more clearly exhibiting the penetration and genius of Cavendish. In the volume before us little of annotation and nothing of criticism is added, the work is left to speak for itself.

Nearly two-thirds of the book is occupied by the first part, containing the papers on Physical Optics. These are twenty-three in number. The first four deal with the geometrical treatment of Fresnel's theory of biaxal crystals. Unfortunately MacCullagh failed to perceive, or at least to point out clearly, the remarkable experiment of conical refraction, so soon after predicted by Hamilton and verified experimentally by Lloyd.

The method of the sixth paper, on the "Laws of Reflection from Metals," is characteristic, as we find a somewhat similar treatment of the theory of transmission of light in quartz and to some extent in the dynamical theory of double refraction. There is no pretence to a firm foundation on mechanical principles. A formula is assumed, the physical meaning of which is not apparent, and the deductions from that formula are interpreted. Fresnel's formulæ for the intensities of the reflected and refracted rays in the case of an ordinary transparent medium are taken as a starting point. It is further assumed without attempt at physical interpretation that the velocity of propagation of waves in a metal is $m(\cos\chi + \sqrt{-1}\sin\chi)$. The real value of the method may perhaps be best shown by trying to interpret this apparently unmeaning assumption. Passing over the difficulties in Fresnel's method of interpreting $\sqrt{-1}$ as extended by MacCullagh, we have on the assumption of the paper—

$$\zeta = A \sin n\{x - tm(\cos\chi + \sqrt{-1}\sin\chi)\}$$

or which is equivalent—

$$\begin{aligned}\zeta &= A \sin n\{x(\cos\chi - \sqrt{-1}\sin\chi) - mt\} \\ &= A\{\cos \sqrt{-1}\, n\sin\chi x + \sqrt{-1}\sin \sqrt{-1}\, n\sin\chi x\} \\ &\qquad \times \sin n\{x\cos\chi - mt\} \\ &= A\, e^{-n\sin\chi x} \sin n\{x\cos\chi - mt\}.\end{aligned}$$

The physical meaning of this equation is obvious, it clearly means absorption in the metallic medium. MacCullagh's theory of metallic reflection would bear the same relation to a theory resting upon the fact of absorption in the metal that Fresnel's theory of total internal reflection does to Green's.

In the seventh paper, "On the Laws of the Double Refraction of Quartz," we find the same method of treatment. Without any reason of a mechanical nature two equations of motion are assumed, viz.:—

$$\left.\begin{aligned}\frac{d^2\zeta}{dt^2} &= A\frac{d^2\zeta}{dz^2} + C\frac{d^3\eta}{dz^3} \\ \frac{d^2\eta}{dt^2} &= B\frac{d^2\eta}{dz^2} - C\frac{d^3\zeta}{dz^3}\end{aligned}\right\}$$

and the integrals of these are shown to express experimental facts. Subsequently Maxwell has obtained for magnetic rotatory polarisation the equations—

$$\left\{\begin{aligned}\frac{d^2\zeta}{dt^2} &= A\frac{d^2\zeta}{dz^2} + C\frac{d^3\eta}{dz^2 dt} \\ \frac{d^2\eta}{dt^2} &= A\frac{d^2\eta}{dz^2} - C\frac{d^3\zeta}{dz^2 dt}\end{aligned}\right.$$

It is interesting to remark that each system is strictly appropriate to the case to which it is applied. MacCullagh's equations do not apply to magnetic rotation of the plane of polarisation, for there the direction of rotation is reversed if the ray be reversed. On the other hand Maxwell's equations are not to be applied to a solution of sugar or to quartz. The distinction has been overlooked by Verdet, who treats both sets of equations as appropriate empirical formulæ for magnetic rotation; it is perhaps not surprising that Maxwell's fitted those facts the best.

MacCullagh's fame as a physical optician rests mainly upon the well-known paper entitled "An Essay towards a Dynamical Theory of Crystalline Reflexion and Refraction." This is the fourteenth paper of the series. Its position, in relation to the theories of Green and Cauchy, has been very ably examined by Prof. Stokes (British Association Report, 1862). But the appearance of Maxwell's theory of the transmission of light leaves room for a reconsideration of Stokes's criticism. The real point of MacCullagh's position may be shortly stated. In order to obtain his differential equations he must ascertain the form of the function V which expresses the work done in causing a given deformation of the medium which transmits radiation. His reasoning is obscure, but it virtually amounts to this. Let $\xi\,\eta\,\zeta$ be the displacement at the point $x\,y\,z$ of the medium, and suppose a plane

wave propagated through it. If under these *particular* circumstances the quantities—

$$\frac{d\zeta}{dy} - \frac{d\eta}{dz}, \; \frac{d\xi}{dz} - \frac{d\zeta}{dx}, \; \frac{d\eta}{dx} - \frac{d\xi}{dy},$$

are given at each point, these three quantities are in general sufficient to define the direction of the normal to the wave front and the direction of disturbance. Hence—and here is the fallacy—the *general* expression for V must be a function of these three quantities. MacCullagh was probably unaware that these three quantities simply defined the angles through which the element about the point xyz was rotated by the displacement. That his expression for the function V not only rests on wrong reasoning but is actually wrong, for the case in question, where V is supposed to depend upon *the change of form* of the elementary parallelepiped, is easily seen. Suppose the deformation to be irrotational: through a given space on MacCullagh's theory V vanishes: but we know it may be anything we please. Again, suppose the medium within a given space to be simply turned through a given angle without change of shape: MacCullagh gives a finite value to V: but we know that in this case V is zero. It is a pity that MacCullagh did not keep to the method of his papers on metallic reflection and on transmission in quartz, and simply say, without admitting that V depends on the change of form of the elementary parallelepiped, "let us assume that V is a quadratic function of $\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right)$, &c." His theory would then have had a certain amount of analytical similarity with Maxwell's theory of electromagnetic propagation of light, though giving not the slightest adumbration of the physical basis of that theory, the facts which it covers being almost unknown when MacCullagh wrote. MacCullagh's expression for V as a theory regarding the ether as an elastic solid is misleading, but we are certainly not compelled to be so materialistic. To the student of physical optics we should say, first read Green, then read and criticise MacCullagh and Cauchy, and finally read Maxwell, not once or twice only, but until you understand him.

OUR BOOK SHELF

On the Structure and Affinities of the Genus Monticulipora and its Sub-genera. By H. Alleyne Nicholson, M.D., D.Sc., &c., Professor of Natural History in the University of St. Andrews. Pp. 240, and vi. Plates. (Edinburgh and London: Blackwood and Sons, 1881.)

THIS is a most elaborate work, on one of the most puzzling groups of palæozoic fossils, by the accomplished and industrious Professor of Natural History at St. Andrews. He gives the general history and literature of the genus, describes the morphology, dealing carefully with the dimorphism of the corallum, treats of the development of the forms, and compares them with Heteropora amongst the Bryozoa. Then the affinities of Chætetes and Stenopora are considered, and those of the Helioporidæ also. A chapter is devoted to the sub-divisions of the genus and to the consideration of the propriety of separating from it Fistulipora, Coustellaria, and Dekayia. Finally five chapters are occupied by the consideration of as many sub-genera. Yet the author modestly says that it is not a monograph of the Monticuliporidæ! The book is particularly valuable on account of the mass of careful description it contains, and the plates and cuts are excellent, and everybody who has tried to make out these tubular fossils will be grateful to Prof. Nicholson for his work. Like most palæontologists, he has suffered from the fact that his predecessors have described genera and species from very imperfect specimens. This is the curse of modern palæontology, and a clean sweep should be made of every classification which is not clear and definite, and which was founded on bad specimens. The difficulty of the subject taken up by the author may be appreciated by noticing the synonymy of the species; and it is interesting to notice how recent investigations by Busk, Waters, and Moseley are influencing the palæontology of very remote ages.

The author states that in *Monticulipora* there are no septa, and the walls are imperforate, whilst in *Heteropora* the walls are traversed by a very remarkable and exceptionally developed canal system; hence he separates the groups, but states—"In the face of the above distinctions I feel compelled to believe, in the meanwhile, that there is no real relationship at all between *Heteropora* and *Monticulipora*." "On the other hand there are strong resemblances between *Monticulipora* and its allies and various undoubted corals—principally perhaps the *Helioporidæ*" "I am at present disposed to regard the *Monticuliporidæ* as ancient groups of the *Alcyonaria*."

P. M. D.

The Evolutionist at Large. By Grant Allen. (London: Chatto and Windus, 1881.)

WIDER and wider grows the field over which newspapers and magazines exert their distributive influence. Verily, they sow beside all waters, and great is the variety of the seed. Their readers find a royal road to learning the contents of books which they are too hurried to read in full, in short essays which collect the essence, omit the difficulties, and state the conclusions of the writers in the clearest and most unqualified terms. It is satisfactory to find that an effort is made to supply modern science to such readers from competent pens.

Mr. Grant Allen has collected into this engaging little volume a series of well-judged attempts to perform this which have appeared in the *St. James's Gazette*, and no reader who would consult that class of publication for scientific ideas could help being interested and, we should hope, led on to further inquiries by it. Mr. Allen describes himself fairly when he says (p. 109), "I am not a butterfly-hunter myself. I have not the heart to drive pins through the pretty creatures' downy bodies, or to stifle them with reeking chemicals; though I recognise the necessity for a hardened class who will perform that useful office on behalf of science and society, just as I recognise the necessity for slaughtermen and knackers. But I prefer, personally, to lie on the ground at my ease and learn as much about the insect nature as I can discover from simple inspection of the living subject as it flits airily from bunch to bunch of bright-coloured flowers." And any one who sympathises with such feelings will delight in the company of "The Evolutionist at Large."

Nearly all the fresh lights which have been thrown upon the relations of the natural world by the teachings of Darwin and Herbert Spencer are here condensed and exhibited in the most simple gossipping style; while it is hardly necessary to say that the most puzzling questions that remain unanswered will suggest themselves on many a page of such an author's book. His disquisitions on the extent of animal feeling (p. 50), upon the origin of two eyes and the cross-connection of them and other organs with the brain (p. 102), are very interesting. But the most striking question which time after time turns up, as we might expect in such a book by Mr. Grant Allen, is the origin of our æsthetic sense: and since the sense of beauty is little else than a feeling of harmony with and admiration for the forms, colours, and adornments of flowers and animals; and as these are all the result of their selection by animals in choosing their haunts and their mates, we are landed at the rather humiliating con-

clusion that a sense of the beautiful and an admiration for the forms and colours of nature is only a strongly developed instinct inherited from the lower animals! An uneasy feeling is raised not only that the rebuke administered to "man, the most conceited creature," by the flea in Gay's fable is well deserved, but that the description there given of the views taken by other members of creation are far more probable and even reasonable than ever their author thought.

It would very much help the less scientific public to accept the doctrine of development if by it any imaginable explanation of the part an insect takes in its own metamorphosis, or its feelings of personal identity through the states of grub, chrysalis, and butterfly, could be suggested; but an attempt explaining so little as chapter xv. must only make the incredulous close the book more sceptical still.

W. O.

An Elementary Course of Practical Physics. By A. M. Worthington, M.A., F.R.A.S., Assistant Master at Clifton College. 51 pp. (London: Rivingtons, 1881.)

THIS extremely useful and carefully prepared little book is intended to form the basis of the practical teaching of physics for schoolboys. It describes the way of performing fifty-eight experiments in elementary physical measurements. It appears therefore to have exactly struck the right line between the Scylla and the Charybdis of practical physics, in which a middle course between "merely qualitative work only leading to play" and "measurements by costly instruments requiring on an average two hours for each experiment," appears to be difficult to steer. Mr. Worthington, whose experience in teaching of this kind is considerable, has embodied the results of his labours in the present compendious little volume, and were the course he has sketched out adopted in all our public schools the gain to physical science would be great. There can be no doubt that one great drawback to the progress of students in physical laboratories even at the Universities is the want of acquaintance with the common instruments and with the principles of exact measurement. Mr. Worthington's course cannot fail to give this, and to teach moreover something of manipulation, exact observation, and of use of algebra and geometry as applied to real quantities. The acquiring of intelligent and orderly methods of recording observations is facilitated wherever possible by providing a blank schedule or form wherein to enter the various observations and their several corrections, and for comparison between the observed and computed results. The course comprises experiments in elementary mechanical measurements, centre of gravity, specific gravity, elasticity of cords, law of pendulum, &c., and also includes experiments upon the law of Boyle and upon the laws of expansion by heat and of specific heat. We trust it will not be long before Mr. Worthington adds a course of practical experiments in other branches of physics to the present series. He deserves the thanks of all who have to teach physics in the laboratory to beginners in manipulation.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

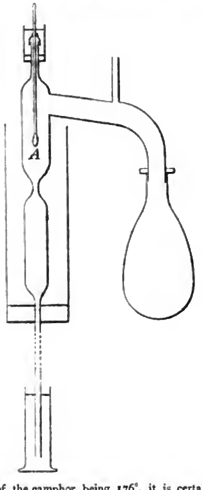
[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Hot Ice

As my name has been mentioned in NATURE in connection with Dr. Carnelley's experiments on hot ice it may possibly be convenient if I describe the experiments in which I have failed to raise the temperature of ice and camphor above their fusing-points when they are heated *in vacuo*.

On December 16, I was present at the meeting of the Chemical Society, when Dr. Carnelley showed his experiments with ice, camphor, and mercuric chloride. At the time they did not appear conclusive to me, for it seemed (although in this I am possibly mistaken) that the thermometer bulbs were too close to the sides of the containing tubes, and that they consequently did not indicate the temperatures of the solids.

A few days afterwards I tried an experiment with camphor in an apparatus so arranged that the thermometer was held rigidly in the axis of a glass tube with the bulb in the middle of a block of camphor which had been previously melted in the tube. The apparatus was exhausted by a water air-pump, and the tube containing the camphor heated. No fusion took place, and the camphor volatilised rapidly; but the thermometer did not indicate a temperature as high as 157° C. The exact temperature could not be ascertained, for a part of the thermometer stem was hidden by the caoutchouc which connected it to the tube. The



fusing point of the camphor being 176°, it is certain that the temperature was far below this, although the glass tube was softened and there were indications of decomposition of the camphor vapour by contact with the hot glass.

Air was afterwards admitted, the camphor fused down, and the air exhausted until the liquid just solidified. Heat was then rapidly applied, but the temperature remained between 170°·2 and 172° until a portion of the thermometer bulb was exposed, when the temperature began to rise. Dr. Carnelley has since informed me that he has obtained precisely similar results with camphor.

On December 30 an experiment was tried with ice in an apparatus bearing a remarkable resemblance to one recently described by Prof. Lothar Meyer (*Ber. Deut. Chem. Ges.* xiv. 718, April 11). The tube A is surmounted by a narrower tube, in which the thermometer stem was fixed by a piece of caoutchouc tube, the joint being surrounded by a tube containing mercury. To the side of A a tube about half an inch in diameter, connected with a copper flask of half a litre capacity, is joined, a branch from this wide tube leading to a Sprengel pump. The

tube A is narrowed in the middle, and its lower extremity terminates in a narrow tube about 33 inches long, and dipping into a cylinder of mercury. Some boiled water was introduced into A before the thermometer was attached, and a small quantity of water was placed in the copper flask. The air was then removed by the Sprengel, the exhaustion being facilitated by heating the copper flask. The water in the glass tube was also heated, in order to expel the dissolved air. When the exhaustion had been completed, the cylinder of mercury was raised until the mercury in the tube stood at the narrow part of A, and the cylinder surrounding the tube was filled with a freezing mixture of ice and salt, the copper flask being also placed in a similar mixture. When the water in the glass tube was solidified, the freezing mixture was removed from the cylinder, the latter lowered, and the column of mercury was depressed by lowering the cylinder of mercury until the column stood at the bottom of A.

A small gas flame was employed to warm the part of the tube containing the ice; some of the ice at the lower part of the solid plug melted and ran down to the surface of the mercury; the upper portion of the ice could not be fused in consequence of the diminished pressure on its surface. When the ice was completely detached from the glass tube a fresh quantity of freezing mixture was placed in the cylinder surrounding the lower part of A.

The air of the room was at 15° C., and the thermometer in the ice indicated − 8°. The tube A was now heated by a Bunsen burner, and the temperature shown by the thermometer was − 6°·5. A jet of steam from a test-tube with cork and narrow tube was directed against the side of A until the ice became very thin on one side of the thermometer bulb; the temperature was now − 1°. The freezing mixture surrounding the copper flask was nearly exhausted; it was therefore replaced by fresh ice and salt, and the steam once more directed against the tube. The thermometer now read − 5°·2. When a small part of the thermometer bulb was free from ice the jet of steam was stopped, and a new freezing mixture placed round the flask, the thermometer indicating − 6°·7. When about one-third of the bulb was exposed the tube was heated by a Bunsen flame and the temperature rose to + 4°, and on allowing the tube to cool it fell to − 5°. Finally, when only a very narrow strip of ice remained attached to one side of the bulb, the tube was strongly heated, and the temperature rose to + 12°, but on cooling it sank to − 2°·2.

The experiment was repeated on January 6, a jacket being placed round the tube so that the heating by steam was more regular than before. On first separating the ice from the containing tube the thermometer indicated − 8°. On heating with a Bunsen it rose to − 6°. When the tube was cold the temperature was − 14°. After passing a current of steam round the tube for half an hour the temperature was − 11°. A fresh freezing mixture was now used, and the steam again turned on; after twenty minutes a small portion of the thermometer bulb became exposed, and two minutes later the temperature was − 9°. A fresh freezing mixture was put round the flask, and when the outside of the tube was cold, the thermometer showed a temperature of − 16°. Steam was again turned on for fifteen minutes, when the temperature was − 12°. When the bulb was half exposed the steam jacket was removed, and the tube heated by a Bunsen; the temperature then rose to − 1°·5. On allowing it to cool it fell again to − 12°. When about three-quarters of the bulb was free from ice the tube was again heated by the gas flame, and the temperature rose to + 29°. The ice then fell off, and although the heating was discontinued the thermometer rose rapidly to 70°.

These experiments, as far as they go, are therefore in accordance with those of Mr. Hannay and Prof. Lothar Meyer, there being no considerable rise of temperature until either the condensation of the aqueous vapour was too slow and the vacuum thus deteriorated, or the thermometer bulb was partly uncovered, and so exposed to direct radiation from the walls of the outside tube.

Two experiments were tried with mercuric chloride, but the results were not satisfactory, in consequence of the high melting point of the solid. The temperature seemed to be above the fusing point, but it was found that the mercury in the thermometer stem had separated.

HERBERT MCLEOD

Cooper's Hill, May 3

Sound of the Aurora

UNDER the above heading Mr. Ogle, in last week's NATURE (vol. xxiv. p. 5), gives an extract from the Visitors' Book at the Æggischorn Hotel describing certain electrical effects which were experienced by Mr. and Mrs. Spence Watson, Mr. Sowerby, and myself on July 10, 1863. I would add one or two facts with regard to our position and experiences. We reached the top of the Jungfrau Joch at 10.5 a.m., and were met by a violent hailstorm, which came rolling up from the northern side of the Col. We at once started to return, and had been walking for two hours down the centre of the Aletsch glacier when the electrical effects began to be felt; we reached the Merjelen See at 3.15, so that at the time of the occurrence we had reached the lower part of the *névé* which is farthest from surrounding mountain tops, where the glacier is widest. We were enveloped in cloud, above which there were no doubt other clouds charged with electricity, and as they approached we were gradually being charged more and more strongly by induction from the lower cloud, and when the discharges or thunder occurred we were suddenly relieved by an electric shock. A kind of *brush discharge* of gradually increasing intensity went on for some minutes, followed by a sudden shock, and this process of bringing us up to the right state of excitement, to be relieved by a sudden shock, was repeated over and over again several times.

The hissing sounds were first heard in the alpenstocks, and gradually increased in loudness up to the sudden discharge. There were clear indications that as condensers of electricity we were not all of the same capacity. We were roped together in threes: in one set of three I was in the middle, with a guide in front and Mr. Sowerby behind. Whilst the charging was going on I felt the pricking sensation at the waist on the side where the cord was knotted, showing that those who were more influenced by electrical induction were charging the others through the rope which acted as a conductor. Judging by his actions, our guide (a young and active man) was strongly influenced by the charge, whilst Mr. Sowerby, the most staid and venerable of the party, was certainly influenced the least. In the other set of three the elderly J. M. Claret of Chamouni was least affected, whereas Mr. Watson, who was not the youngest of the party, was the most powerfully affected. These facts point to a direct relation between the temperament of the individual and his capacity for being excited electrically or his inductive capacity.

I should add that Mr. Packe has had similar experiences, but apparently to a less extent, in his walks in the Pyrenees.

W. GRYLLS ADAMS

Wheatstone Laboratory, King's College, May 9

Palæolithic Man

IN my communication to NATURE, vol. xxiii. p. 604, I chiefly restricted my notes to the higher gravels on the north side of the Thames in and near London. With your permission I will now briefly refer to some of the implementiferous gravels south of London, especially in Kent.

The best known of these are to be seen between the Reculvers and Herne Bay, where a thin stratum of implementiferous gravel caps the cliffs. Similar but deeper gravels, also bearing implements, occurs elsewhere inland, as at and near Chislet. At Canterbury a great number of implements have been found, and to these gravels and implements I would now direct attention. As a rule the Reculver instruments are sharp, unstained, and unabraded (such as have been rolled in the sea of course excepted); the Canterbury examples where the gravel is deep are found at various depths, from 9 feet to 20 feet.

Now there are two distinct classes of implements found in the Canterbury pits, the levels being, according to Dr. Evans, about 80 feet or 100 feet above the river; in one class the specimens are well made and almost as sharp and unstained as when first turned from the maker's hands, in the other the implements are much more rudely made, deeply stained all over of a dark ochreous brown colour, and abraded in a high degree. These latter implements come from distinct strata or deposits of ochreous brown rolled stones that appear to have been brought from a long distance. In my own collection of twenty-nine examples from Canterbury one half are sharp and bright, the other half greatly rolled and deep brown in colour. To my mind these two classes of instruments represent two totally distinct periods in the Palæolithic age immensely removed in time from each other, the abraded examples being the oldest. A point of importance to be observed in the deeply ochreous implements is that many of them were slightly splintered or broken when they were deposited in the Canterbury drift; now these broken and

splintered parts are bright, lustrous, and non-ochreous, exactly resembling in the fractured parts the bright and unabraded implements. This fact appears to me to demonstrate that the abraded ochreous implements acquired their ochreous crust elsewhere, and were objects of great antiquity when the Canterbury gravels were laid down. I have one of these deep-brown greatly-rolled flint implements that was found amongst chert in the famous pit at Broom, near Exeter: the deep ochreous colour was not derived from chert gravel. From whence have these generally massive, abraded, ochreous implements been derived, and how laid down in distinct deposits?

As an instance of very high implementiferous gravels, the same distance south of London as the Ware gravels are north, the ancient gravels on the escarpment of the hills north of Sevenoaks and Ightham may be cited. Some of these heights exceed by 200 feet the heights of the Hertford and Ware positions.

Dr. John Evans, in his admirable book on the "Ancient Stone Implements of Great Britain," pp. 531-532, records the important discovery (on the surface) of ochreous and abraded implements at great heights near Currie Wood, a few miles south of St. Mary's Cray, Kent, at 300 feet above the valley of the Darent, and 500 feet above the sea. Dr. Evans also says (p. 531), "It is, however, necessary that further discoveries should be made in this district before it will be safe to speculate on the origin of these gravels and their relation to the superficial configuration of the neighbourhood." My friend, Mr. Benjamin Harrison of Ightham, has during the last year instituted a rigorous search over the high level gravels south of this district. A tributary of the Medway rises at Ightham, near Sevenoaks; the level of the present stream near the village is 254 feet, and an outlying bed of old river-gravel is found at 330 feet, and another bed up the stream at an altitude of from 380 feet to 400 feet. In these high-level Wealden gravels Mr. Harrison has recently found palæolithic implements *in great numbers*, generally massive, ochreous, and abraded. At 312 feet he has found them *in situ*, and on the surface as high as 335 feet. More recently Mr. Benjamin Harrison has examined the old river gravel at Dunk's Green (two miles and a half south of Ightham), and here at a level of 200 feet has proved the beds to be implementiferous. For these facts and heights I am indebted to Mr. Harrison, who has given me his permission for their publication.

In these two letters I have chiefly confined myself to statements of dry facts, purposely abstaining from any comments on the meaning of the heights, &c., referred to.

WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

Naval Cadet Examinations

I WISH to bring to your notice the injudicious severity to which our competitive examinations have of late attained, regardless, as it appears to me, of the possible injury they may inflict on the health of those who are forced to strain every power, both physical and mental, in the struggle.

The most recent example of the kind is, I believe, the New Standard for Naval Cadetships, which requires boys from between the ages of twelve and thirteen and a half to pass a competitive examination in Latin, French (both translating and speaking), arithmetic up to decimal fractions, algebra, including fractions and simple equations with one unknown quantity, geometry up to first twenty-six propositions of Euclid, English—with Scripture history. They are further tempted, if ambitious, to take algebra up to quadratic equations, and geometry up to the end of the first book of Euclid.

Now when the object to be obtained is no less than a career for life, one can imagine what a force of pressure—from the parent anxious to provide for his son, from the schoolmaster's pride in his pupil, and from the boy's own ambition—is brought to bear to urge nature to the utmost in the trial.

The casualties—for we are entitled to use the expression—that have already occurred under the system have been sufficiently numerous to make any one who will pause to think seriously anxious.

Education is most valuable, but when its attainment is at any time carried out at the expense of health to the pupil it is a failure. "Mens sana in corpore sano" is above everything to be prized, and he who enters upon life's work possessed of that advantage is fittest for its trials.

I will quote an extract from the *Lancet*, which treats the subject from a professional point of view and with an admirable clearness. It says:—

"There can be no room to question the extreme peril of 'over-work' to growing children and youths with *undeveloped* brains. The excessive use of an immature organ arrests its development by diverting the energy which should be appropriated to its growth, and consuming it in work. What happens to horses which are allowed to run races too early happens to boys and girls who are over-worked at school. The competitive system as applied to youths has produced a most ruinous effect on the mental constitution which this generation has to hand down to the next, and particularly the next-but-one ensuing. School-work should be purely and exclusively directed to development. "Cramming" the young for examination purposes is like compelling an infant in arms to sit up before the muscles of its back are strong enough to support it in the upright position, or to sustain the weight of its body on its legs by standing while as yet the limbs are unable to bear the burden imposed on them. A crooked spine or weak or contorted legs is the inevitable penalty of such folly. Another blunder is committed when one of the organs of the body—to wit, the brain—is worked at the expense of other parts of the organism, in face of the fact that the measure of general health is proportioned to the integrity of development and the functional activity of the body as a whole in the harmony of its component systems. No one organ can be developed at the expense of the rest without a corresponding weakening of the whole. These faults of 'training' attain their supreme height of folly and short-sightedness when they are committed in reference to the youths destined for the public services. They are especially illustrated by the 'Regulations respecting Naval Cadets' just issued, and which will take effect in June of the present year. The work of the Civil Service Commissioners in respect to these classes of the possible servants of the State is personally and racially *destructive*. Sooner or later public opinion must recognise this fact, and then perhaps the Government or the Legislature may be moved to interpose—not before, but when it is too late."

We live in an age of reactions, when ideas are hastily adopted, hurriedly brought into practice, and fanatically adhered to. I can only hope that public opinion will recognise the danger that the *Lancet* so clearly points out, and that the Government may interpose before it be "too late." J. D.

Flame-Length of Coal-Gas

I HAVE recently measured the flame-length of a sample of coal-gas burning in air and burning in nitrous oxide (N_2O). The flame-length in air was $\frac{13}{16}$ths of an inch and $\frac{5}{16}$ths in nitrous oxide. The relation of 5 to 13 is very close to what my theory would suggest, and is a confirmation of my law published in your issue of April 7.

I might add that I have recently noticed the flame of a mixture of hydrogen and nitrous oxide burning in air to develop a bright white spot about one-third from the top of the flame, and when the proportion of nitrous oxide is larger, to extend into a cone reaching to the jet. I have not examined this flame with a spectroscope, but am certain, from the whiteness of the flame, that the spectrum would be continuous. LEWIS T. WRIGHT

Water in Australia

A GENTLEMAN recently returned from Australia believes that the arid plain which occupies the centre of that island-continent might be amply supplied with water and converted into rich farm land by a very simple process. He founds his belief upon observed facts in the three sciences of botany, physiography, and geology, thus:—

1. Gum-trees and the mallee scrub flourish there. The gum-trees grow to a great size and withstand the drought of many summers. They must have water; whence do they obtain it?

2. Rivers which flow towards the centre from the mountain ranges along the coasts have no apparent outlet into the sea, but are lost in the desert. What becomes of them?

3. The underlying rock of the central plain is an almost horizontal bed of Tertiary Sandstone.

The conclusion is that the Sandstone is saturated with water and forms an immense reservoir from which existing trees draw their supplies by deep tap-roots, and that by sinking wells in the desert this water could easily be reached.

The author of this theory, wishing only to confer a public benefit, desires to bring it under the notice of scientific men,

that it may be either turned to account or shown to be erroneous. If there is even a remote possibility of its truth it would seem worth while for one or more of the Colonial Governments to have borings made in order to test it. F. T. MOTT

Birstal Hill, Leicester, May 5

The Glacial Blocks of Zinal

MAY I through your columns express a hope that other qualified observers will volunteer to take charge of work such as I propose to do this summer as my share?

This is to mark the position of large blocks of stone on the glacier of Zinal. You will, I hope, receive the report of my friend, Prof. F. A. Forel, upon periodical variations of glaciers. Therein are sketched some of the existing data. I have for years much wished to organise a simultaneous action. With a Galton's pocket altazimuth, a pot of paint, and the superb map on the scale of $\frac{1}{50000}$ of the Swiss Alpine Club (Sheet III. of the Valais du Sud), it will be a pleasant and not a difficult task to lay down a few good triangles, and to paint a letter and indication of bearings of stones along and athwart the great glacier, with which I am well acquainted. The Swiss Alpine Club has erected a hut at Les Mountets, which, at about 9500 feet above sea-level, will form a capital base of operations. The pre-eminently grand scenery would itself reward the short sojourn necessary for our purpose. To secure uniformity of action and registration I propose that we should place ourselves in communication with M. F. A. Forel. I shall be very glad to hear from gentlemen—at this address up to the end of June, and then at the Hôtel d'Anniviers, Vissoie sur Sierre, Canton Valais, the most comfortable quarters in the Val d'Anniviers, about 4000 feet above sea-level, three hours' and a half drive from Sierre railway station.

I would suggest, as good head-quarters and interesting fields of observation: (1) the hotel at the Riffelberg, with the Gorner and Findelen glaciers; (2) the hotel at Saas in Grund, with the Fée and other large glaciers in the Saas-Thal; (3) the hotel at the Maltmark See, with the Allalin and Schwartzberg glaciers; and (4) Macagnaga as a southern station. I myself, also, ask for personal assistance. MARSHALL HALL

Villa Chessex, Veytaux-Chillon, Canton Vaud, Switzerland, May 3

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AT ALGIERS[1]

III.

THE main result of the Algiers Congress has undoubtedly been the acquisition of a considerable amount of matter tending to the development of the great French colony, while at the same time it has been the means of making hundreds of Frenchmen well acquainted with the principal features—physical, geographical, and political—of a country which they knew previously only by name. The general results, as far as universal science is concerned, have been slight, but we cannot regard the Congress as less than a success. It is as if the French had said to the world of science, "Come and see this undeveloped country, and help us to apply each and all of the sciences to its special requirements, to aid us in a more perfect colonisation." The work has been nobly initiated by the French. It is probable that not less than a hundred millions sterling have been expended in the country. The roads and bridges, and telegraph and postal systems are perfect. Everywhere you find evidences of complete organisation. Every small village has its mayor and council; its post-office and diligence service; its water supply and sanitary arrangements; its groves of eucalyptus-trees and trimly-planted streets. Let us take one example—that of Bordj-Menaïel, a village to the east of Algiers, which we visited in the course of an excursion. Twenty-three years ago Bordj-Menaïel was made a centre of colonisation, and 1718 hectares of land were distributed among the first colonists. The total superficies of the commune is 4200 hectares, and it contains a population of 837, of whom 659 are Europeans and 178 indigenous races. Situated at a distance of 70 kilometres from Algiers and 38 from Dellys, it is traversed by the main departmental road passing to Eastern Algeria. It stands in the midst of a highly fertile alluvial plain, 28 metres above the sea, and is watered by the Isser. This commune possesses the following municipal officers: mayor, deputy-mayor, justice of the peace, sheriff's officer, receiver of "contributions diverses," a recorder of the census, a manager of ponts et chaussées, a departmental business agent, a bureau of posts and telegraphs, a "médécin de colonisation," a midwife, and a pharmacien. Its spiritual and intellectual wants are provided for by a *curé* and two schools. Since 1873 a brigade of gendarmerie has been stationed in the village. The organisation appears excessively elaborated for so small a population; but we must remember how doubly necessary such arrangements become in a new colony, which without sufficient proofs of the strong arm of the law would speedily become lawless, and without the benefit of well-directed and properly enforced municipal arrangements would form an ill-regulated and degenerating community. The bureaucracy evidently enters largely into the French system of colonisation.

At the present moment a project is before the Chamber for the completion of the colonisation of Algeria by the creation of 300 new villages, which, like Bordj-Menaïel and the existing villages, are to be built and thoroughly organised before colonists are invited to accept the grant of land in the commune and take up their abode in the village. Such of the existing villages as we saw were of one and the same type: the church and water-supply in a central square, from which two or more streets proceeded; the mairie, a few shops, one or more inns, and a post-office. In some villages—Palestro, for example, many of the inhabitants of which were massacred by the Kabyles so recently as 1871—there was a large space, surrounded by a high wall furnished with loopholes, in which the inhabitants could take refuge in the event of a sudden descent of the natives. Many of the colonists are Alsatians or Lorrainers who emigrated at the close of the Franco-Prussian war. They all appeared happy and contented, and their farms and gardens were flourishing. Their worst enemies are drought and fever; the former is being provided against by new systems of irrigation, and the latter by the planting of thousands of eucalyptus-trees. At Blidah we found a perfect example of the most developed system of irrigation. A ready supply of water is obtained during many months of the year from the mountains, and this is led by small brick-lined watercourses through the gardens. A main watercourse passes a line of houses, the garden walls of which are furnished with small trap-doors by which at any time a portion of the stream can be diverted into the garden. Of course rain is always looked for with great anxiety, specially between the months of May and September, when the grain crops are wholly dependent upon it. In the south of Algeria there exist at this moment places where no rain has fallen for *six years*, and of course any attempt at cultivation is here impossible.

Towards the end of the Congress several of the sections showed greater vitality than at the commencement. In the section of Mathematics there was for the first time a fair show of papers, for the most part devoted to pure geometry. The foreign mathematicians—Leguine of Odessa, Oltramare of Geneva, and Fiedler of Zürich—contributed their quota. M. Trépied brought forward a project for the construction of an observatory at Algiers. M. Picquet has been elected president of this section for next year. In the section devoted to Civil Engineering the most important papers were by Col. Fourchault on defensive villages, and by M. Trémaux on irrigation. M. Gobin is president for next year. In the Physical section papers were read by M. Gaussen on photometric photography, and by Prof. Tacchini on the solar protuberances.

[1] Continued from vol. xxiii. p. 607.

There was no paper of special interest in the Chemical section, of which M. Grimaux is president for 1882. The section of Meteorology was very active at the last, and supplied some interesting papers on the meteorology of Asia, of the Sahara, and of the district between the Atlas and the Cevennes; also on meteorological instruments—thermographs and anemographs, and registering barometers. MM. Denza and Tacchini among the foreigners contributed largely to this section. M. Fines is the president for 1882. The Geological section had no communications of general or special interest. Professors von Szabo of Buda-Pesth and Villanova of Madrid both contributed papers, and an interesting communication was made by M. Fusch (who is president of the section next year), on the lead and iron mines of Tunis, and the copper mines of the Petite Kabylie, a district to the east of Algiers. The Botanical section announced only two papers of very limited interest for the last day but one of the Congress, and it did not meet at all on the last day. M. Ed. Bureau is president for 1882. The sections of Zoology and Zootechny also showed signs of languishing. On the other hand the section of Anthropology exhibited the greatest vigour from first to last, and on the last day of the Congress no less than sixteen papers, many of them of great local interest, were announced. Among these we may specially mention observations on the Kabyles of the Djurdjura, and on the Tziganes; on the Romans in Africa, and the Berber migration; on the civil, political, and religious institutions of the Jews; and on craniometrical studies in the oasis of Biskra. A prehistoric map of the north of Africa was discussed by M. Cartailhac. Anthropology is one of the sciences which has benefited most by the Algerian Congress. M. Henri Martin is president for 1882.

The interest in the Medical section continued to the last. We have before had occasion to remark that the Congress to a great extent was a medical *réunion*, and more than one-fourth of the members were medical men. Seventeen papers were announced for the last day of the session, and fourteen had been read the day before. The most interesting to the general non-medical members were on the epidemics of Algiers, on acclimatisation, and on the climate of Algiers as regards its influence on consumptive patients. Dr. Azam of Bordeaux is the president for 1882. The success of this section has been complete; a large number of very valuable papers have been communicated, and the attendance has always been large. The final papers communicated to the section of Agronomy related mainly to the development of the agriculture of Algiers: on the cultivation of cereals and of sorghum; on the rearing of cattle; on agricultural railways; and on watercourses. M. Dubort is president for 1882. The section of Geography was also mainly devoted to Algerian questions: the Trans-Saharian Railway; the geodesy and topography of North Africa; maps and geographical vocabulary. The section of Political Economy devoted its last hours to the dominant subject throughout—the colonisation of Algeria, the treatment of the indigenous races, and the peopling of the Sahara. Finally the section of Pedagogy visited the principal schools of the city, and collected as much information as possible concerning the methods of instruction.

At the commencement of the Congress the members were presented with a volume entitled "Notices Scientifiques, Historiques, et Économiques sur Alger et l'Algérie." The second volume is to be ready before the end of this month. The work is compiled by twenty-one residents in Algeria, each one very competent to discuss the subject of his contribution. Thus the geography is described by the president of the Algerian Geographical Society, the general administration by the secretary of the Council of Government, and the history by the Director of the École supérieure des Lettres. The whole constitutes the most complete and exhaustive history of Algeria which exists. Starting with the geography, hydrography, and climatology of the country, an able discussion of its complex meteorology follows. Statistics are given of the barometric pressure, mean temperature, hygrometry, winds and atmospheric currents, electricity, rain, evaporation, &c. There are forty meteorological stations in the country, extending from Mogador in Marocco, to Sfax in Tunis. A daily bulletin has been issued since 1875, and it is distributed over thirteen points on the coast. The very interesting geology and mineralogy of the country forms an article by M. Pomel, from which we learn that copper, argentiferous lead, zinc, iron, building-stones, and salt are profitably mined. About 3500 miners are employed, more than half of them finding occupation in the province of Constantine. The succeeding articles treat of botany, zoology, and anthropology. The major part of the indigenous population consists of two ethnic groups: on the one hand the Arabs, nomad tribes, shepherds, with a patriarchal organisation, and with warlike and religious feudality (*féodalité guerrière et féodalité religieuse*); on the other the Kabyles, cultivators of the soil, non-nomadic, and with a social organisation which is democratic and communalistic.

An article of much interest, by Dr. Liautaud, treats of the colonisation and peopling of Algeria. We have no space to give even an outline of its contents, but we commend it to every one interested in the present and future of the colony. It is followed by articles on the actual state of agriculture; industry and commerce, public works, the financial system, and the Algerian budgets. Then a general survey of the history of Northern Africa is given by M. Masqueray; archæology by M. MacCarthy; the general administration by M. Dimier; judicial organisation by M. Fau, Advocate-General of Algiers, and taxes and imposts by M. D'Aufresne.

We will glance for a moment at some of the statistics connected with public works and education. When the French took possession of Algiers in 1838 they found a *tabula rasa* as regards public works. In 1843 they set on foot the drainage of the Mitidja; between 1840 and 1846 ports were constructed, and lighthouses placed along the coast, and great systems of excellent roads were inaugurated; in 1857 railways were commenced. Forty-five lighthouses are now in operation, and there are seven ports. In 1830 the imports amounted to a value of 5,000,000 francs, and the exports to 1,500,000 francs; while in 1879 the values were respectively 272,126,102 francs and 151,918,421 francs. There are 10,506 kilometres of roads and 1282 kilometres of railways. There are now about 600 agricultural villages dispersed through the three provinces of Algiers, Oran, and Constantine.

As regards public instruction, higher instruction is given in four schools, which no doubt will soon be united into a university: law, medicine, letters, and science. Secondary education is provided by a Lycée in Algiers, ten communal schools, and three free schools. The former contain 3405 pupils, among whom there are 365 Jews, and 272 Mussulmans. The number of primary schools, including Arabic, Kabylic, and Arab-French schools is 703, in which are educated no less than 53,803 pupils—28,803 boys, and 24,501 girls. The boys consist of 11,639 French, 7336 foreigners, 7408 Jews, and 2420 Mussulmans.

On April 20 the various excursions commenced: eastward to the confines of Tunis, southward to the Sahara, and westward to the boundaries of Marocco. We have not space to tell how we went into the country of the *indigenes*—the Kabyles who were driven into the mountains when the Arabs first took possession of the land—how they met us headed by their chiefs, and conducted us, with accompaniments of barbaric music, and discharges of guns to awaken the echoes, through the magnificent gorges of Palestro; and how on the following morning

we breakfasted with them in one of the most picturesque spots imaginable, and finally how we realised with difficulty that we were only four days' journey from London, while breakfasting, under a bower of carouba branches, off sheep roasted whole over hot ashes placed in holes in the earth, manipulated with hunting-knives and without forks, flat circular cakes of Kabyle bread serving us for plates, and gigantic wooden bowls of *kous-kous* as a second course.

The amicable attitude of these once savage aborigines seemed to us to re-echo the key-note of the Algiers meeting of the French Association; to proclaim loudly and with no uncertain sound that the "conquête pacifique de l'Afrique septentrionale par les Français" has indeed become an accomplished fact. G. F. RODWELL

ELECTRIC LIGHTING[1]

II.

THE second district of the City which has been illuminated by electricity is that embracing Blackfriars Bridge, upon which there are seven lamps; Bridge Street, in which there are four lamps, Ludgate Circus taking three lamps; Ludgate Hill four lamps; north side of St. Paul's six lamps; and Cheapside, as far as King Street, eight lamps; thirty-two lamps in all, replacing 150 gas lamps; and moreover, all these lamps are fixed upon one circuit, which is operated practically by only one machine, and that fixed more than a mile away, at the manufactory of the Brush Company in York Road, Lambeth. The total length of this circuit is over 20,000 feet. It consists of a copper cable made up of seven best copper wires surrounded with a thick layer of gutta-percha protected externally with tape that has been well tarred. This wire gives a total resistance of 7·5 ohms., and is protected by iron pipes like the Siemens method, and laid on the principle so well known in telegraphy. The dynamo-machine is of the familiar Brush form, and at present there are two machines of the size known as No. 7 cabled up in series, but forming practically only one machine. These two machines are intended to be replaced by one dynamo-machine, which, at a velocity of 800 revolutions, and worked by 32 indicated horse-power, will maintain forty lamps burning. The main feature of the Brush system is its simplicity, one machine working a number of lights, and those who visit the engine-room of Messrs. Siemens, and then that of the Brush Company, cannot help being struck by the immense difference in the contrast between the two. In Siemens' engine-room one feels in the midst of a whirling cotton manufactory; at the Brush works one sees nothing but a single engine working a single machine quietly and without fuss or flurry.

At present a Galloway engine is used by the Brush Company, but when the large 40-light dynamo-machine is set to work a Brotherhood 3-cylinder engine of 32-horse power will be used. These engines are admirably adapted for electric light purposes; they are bolted on to the same frame as the dynamo-machine, and give a compactness and solidity to the whole that is very striking.

The Brush lamps differ but very little from the generality of arc lamps. The carbons are maintained apart by what is known as a "sucking" coil; and the lamp possesses one or two very ingenious designs to shunt it out of circuit when anything fails and to shunt it in when all is in order. It is a kind of duplex lamp, supplied with a double set of carbons, each of which burns for eight hours, the total illuminating durability of the lamp being therefore sixteen hours. These lamps are fixed upon the ordinary lamp-posts, which have been raised 2½ feet higher than usual, so as to maintain the lamp 16 feet from the ground. They are surmounted by very ugly roofs that are neither covers nor reflectors, and the mode in which these lamp-posts have been utilised is the least commendable feature of this system. Indeed the Company might have made much more of their facilities. Compared with the tall posts in the remainder of Cheapside they make an unfavourable impression. The globes that embrace the light are too small, and the Company itself seems not to have arrived at the proper decision as to the best quality of globe to use, for in some places the lamps have clear globes, in others ground-glass, and in others opal globes, made, we believe, by Mr. Frederick Siemens of Dresden.

It cannot be said that the mode of illumination adopted by the Brush Company is perfect at present. The theory of the proper distribution of the light has been neglected. The lamps are scattered about in an irregular manner that is quite offensive to the eye, and though the effect of each lamp is certainly brilliant, the effect of the whole is rather displeasing. It is very wonderful that so much light should be produced from a point so far, and there is no doubt that the public mind has been captivated by the brilliancy of the lamps. The scientific eye however sees room for improvement, and it is hoped for the sake of the success of the experiment that the Brush Company will be guided by the experience of disinterested persons. The great merit of their system is the simplicity of the machinery employed, as well as the brilliancy of the light, but occasionally the lights are subject to great want of steadiness, and it is much feared, with the quality of the cable they have used and the enormous electromotive force, that the usual faults accompanying underground wires will develop themselves rather largely. In fact, two very serious breaks-down have already occurred, and they are about to replace their conductor by a better one. They will have to pay dearly for their neglect of common experience.

So far the experiment has shown that the practical lighting of streets by electricity is not only feasible but practicable. Moonlight has certainly been thrown into the shade, for the streets of the City are better illuminated by electricity than by fair Luna. It is quite possible to read a letter or to see one's watch at any point in King William Street. Indeed the smallest object can be seen anywhere, even in the middle of the road. Running-over has been rendered impossible.

The outside districts not specially favoured are clamoring for electric lighting, but much has yet to be learnt before the experiment can be determined as final. For instance, we have the Lontin system to be tried, and we should certainly like to see that most energetic and successful engineer, Mr. Crompton, test his system in the London streets. Incandescent lamps are looking up and deserve a trial.

The best mode of distributing light has not yet been settled.

It would seem that a compromise between the centralised system of Siemens and the distributed system of the Brush Company is that needed to solve the problem of proper street illumination. But instead of carrying lamps irregularly down streets on hideous lamp-posts it would almost seem that the ancient defunct mode of swinging lamps across streets from housetops would be a better mode of illuminating streets. Take, for instance, Regent Street. If that street had suspended above it, at the height of 40 feet or 50 feet and at about every 100 yards, a Brush lamp fitted on the top of a graceful iron arch, or suspended on wire ropes between the tops of the houses, nothing could possibly be greater than the effect. Light arches thrown across the street might even be a convenient mode of suspending the wires forming the circuits, for overhead wires have a considerable advantage over underground wires in this, that they cool more rapidly and allow more electricity in consequence to pass through. More than that, they require no insulation, and the money thrown into their insulation could be thrown

[1] Continued from p. 7.

into their greater mass and greater strength. We illustrate this idea in Fig. 1. The City of London authorities who have shown so much energy and commendable zeal in carrying out this experiment would still further confer a favour on the public if they were to remove their hideous heraldric excrescence on the top of the so-called Temple Bar Memorial, and replace it by a handsome bronze pillar 30 feet or 40 feet high carrying a bright and brilliant electric light.

The Siemens system compares favourably with the Brush system in one respect, and that is they do not throw all their eggs into one basket. In their distributed system they have arranged the lamps on two circuits, so that each alternate lamp is on a different wire, and if anything goes wrong with the one circuit only alternative lamps go out, and not all. With the Brush system, on the contrary, if any fault occur in the wire in any of its length of nearly two miles, then every lamp on that circuit must go out.

The effect of fogs upon this system will be narrowly watched. It may happen, and probably will, that the fogs will be absolutely utilised by the electric light, for the reflection in the neighbourhood of the light by the small particles that constitute the fog throwing back the rays of light will help to illuminate the street, and so to a considerable extent relieve the impression now produced by dark fogs.

Of the efficiency of the system, as we have said, there can be no doubt. As to its economy, experience alone can determine. That the firms themselves require experience on this point is evident from the disparity in the charges made by the two firms competing. The Brush Company only estimate the cost of working their system at 660*l*., which is the cost of gas: Siemens Brothers

FIG. 1.

estimate the cost of working at 2270*l*., nearly four times the cost of the Brush. We are inclined to think that a mean must be taken between these two. It is hopeless to expect that electric lighting in every case can be done at the cost of gas. Electric lighting is a luxury, and as a luxury we must expect to pay for it. Nevertheless it must not be forgotten that gas utilised as a motor can produce eleven times the quantity of light by the aid of electricity than it produces by direct combustion. This calculation is interesting. One hundred cubic feet of gas per hour can be made by combustion to produce 300 candle-power. The same quantity of gas used in a gas-engine to rotate a dynamo-machine will produce a light equal to 3750 candles, an illuminating power twelve times greater. Hence there must also be some cases in which the electric light can economically supplant gas; whether it can do so in the case of street-lighting remains to be seen by the great experiment now being tried.

There are two or three very interesting points on the line of illumination where comparisons can be made between the different systems. For instance, from the middle of Blackfriars Bridge there is a good comparative test between the Brush and Jablochkoff systems; while at the corner of King Street there is an equally excellent opportunity to examine the merits of the Siemens distributed and the Brush systems. Photometric measurements at each place prove that the Brush Company's estimate of the light power of their lamps is absurdly exaggerated; 750 candles would be a very fair figure to give the lamp. The unhesitating opinion is that comparing each individual lamp, the Brush surpasses the others. In fact there is little difference between the smaller light of Siemens and the Jablochkoff, excepting this, that the Jablochkoff, by its variation and pinkish effects, irritates the eye considerably, although in bright weather the effect on the water is sparkling and brilliant. The Jablochkoff system has been entirely put in the shade by this interesting experiment. Though however we are obliged

to express our opinion that the Brush lamp, *per se*, is the best, their system of illumination and distribution of posts cannot be compared with the care and skill evinced by the Messrs. Siemens. If either company were to try our suggestion, and illuminate down the centre of the street, maintaining the line of the street in their line of lights, we should have another interesting experiment that would go far to solve this question.

One last point deserves attention. It is the effect which these strong and powerful electric light currents have on the working of the telegraph. It was feared that their presence would deteriorate the working capacity of telegraph wires, and undoubtedly it would be so were it not that, taking advantage of the warnings given them, the electric light people have in all cases adopted a *return* wire, so as to make their circuit completely metallic throughout. We are glad to learn that this has proved quite effective except in one instance, where along London Bridge the return wire has been taken round the other side of the bridge, and here considerable disturbances have been experienced in certain telephone circuits from the contiguity of the electric light currents. No other disturbance has as yet been experienced.

We have also as yet to experience the effect of weather. Up to the present moment it has been all in favour of the electric light—bright, clear, cheerful skies have given to the light a clearness and brilliancy that have created for it a strong feeling. When thick weather and rain and fog occur there may be a change in this opinion, not only from a disturbance of the penetrating power of the light, but on the effect of rain and moisture on the wires conveying the currents.

(*To be continued.*)

DR. HOLUB'S AFRICAN TRAVELS[1]

I.

FROM his boyhood days Emil Holub determined he would explore some of the out-of-the-way portions of the African continent, and in 1872 the opportunity was presented to him of travelling in the southern parts of that great and still unexplored country. The result of seven years labour, during which period of time he made three several journeys of investigation, are now in these volumes laid before the public. In Dr. Holub's first journey he started from Port Elizabeth, crossing the Cape Colony district and the extreme south-west corner of the Orange Free State, to Kimberley. As far as Grahamstown he could have had the modern convenience of a train, but preferred a two-wheeled cart drawn by four small horses, making about eight miles an hour. The country is very charming for the greater part of this route, the road being beneath the brow of the Zuur Mountains, which with their wooded clefts and valleys, and their little lakes inclosed by sloping pastures, afforded many interesting views. The fauna was as varied as the flora, and numerous captures were made by the way. Even large game like elephants were to be met with, and the author records a sad accident which happened in the underwood by the Zondago River, between Port Elizabeth and Grahamstown. A black servant sent to look for some strayed cattle had been met by a herd of passing elephants, some of whom knocked him down and then trampled him to death. In this district these big animals are under the protection of the Government, and not being often interfered with, they would seem to have no great fear of man. The springbok (*Antelope euchore*) is noticed as still in some districts swarming, though its numbers must be rapidly diminishing, as Dr. Holub saw whole waggon-loads brought to Kimberley, where the carcases were sold at prices varying from three to seven shillings a head. Among other wonderful instances given of the great skill of the Dutch Boers in bringing down these swift creatures, he tells of one expert marksman killing by a single shot from his breech-loading rifle two of these antelopes.

Towards Colesberg the country forms a high tableland; it is on an affluent of the Orange River. On this table-land there was a herd of upwards of fifty quaggas, the only herd the author could hear of in South Africa. The farmers have lately spared them, so that during the last ten years they had increased to their present number from a small herd of fifteen. Philippolis, the first town entered of the Orange Free State, is described as dreary-looking, the houses mostly unoccupied, and the general aspect most melancholy. On the way to the Diamond Fields nothing but bad roads and worse weather were encountered; the wind was piercing, and snow actually fell. Fauresmith, one of the most considerable towns in the republic, although consisting of not more than eighty houses, covered a considerable area. It was clean and pleasant-looking, and here the author thought of settling for a time, and by the practice of his profession saving enough of money to enable him to start afresh for other fields; but the fates were against him, and he was, after a few days, obliged to push on to the Diamond Fields, and the following extract will best tell of these:—

"The first day upon which I set my eyes upon the Diamond Fields will ever be engraved on my memory. As our vehicle made its rapid descent from the heights near Scholze's Farm, and when my companion, pointing out to me the bare plains just ahead, told me that there lay my future home, my heart sank within me. A dull dense fog was all I could distinguish. A bitter wind rushing from the hills, and howling around us in the exposure of our open waggon, seemed to mock at the protection of our outside coats, and seemed resolved to make us know how ungenial the temperature of winter in South Africa could be; and the grey clouds that obscured the sky shadowed the entire landscape with an aspect of the deepest melancholy. Yes; here I was approaching the Eldorado of the thousands of all nations, attracted hither by the hope of rich reward; but the nearer I came the more my spirit failed me, and I was conscious of a sickening depression. Immediate contact with the fog that had been observed from the distant heights at once revealed its true origin and character. It proved to be dense clouds of dust first raised by the west wind from the orange-coloured sand on the plains, and then mingled with the loose particles of calciferous earth piled up in heaps amidst the huts on the diggings. So completely did it fill the atmosphere that it would require little stretch of imagination to fancy that it was a sand-storm of the Sahara. As we entered the encampment the blinding dust was so thick that we could only see a few yards before us; we were obliged to proceed very cautiously, and before we reached the office of the friend I had to call on, another mile or so farther on, our faces and our clothes were literally incrusted. We only shared the fate of all new-comers in feeling not only distressed but really ill; the very horses sneezed and snorted, and showed that the condition of things was no less painful to them than to their masters. Here and there on both sides, right and left, wherever the gloom would permit me to see, I noticed round and oblong tents and huts intended for shops, but now closed, built of corrugated iron. Under the fury of the wind the tent-poles bent, and the ropes were subjected to so great a strain that the erections threatened every moment to collapse. Many and many a sheet of the galvanised iron got loose from the roofs or sides of the huts, and creaking in melancholy discord, contributed as it were to the gloominess of

[1] "Seven Years in South Africa. Travels, Researches, and Hunting Adventures between the Diamond Fields and the Zambesi." By Dr. Emil Holub (translated by Ellen E. Frewer). With about 200 original illustrations and a map. In two volumes. (London: Sampson Low, Marston, Searle, and Rivington, 1881.)

the surroundings. In many places too the pegs that had fastened the tents to the ground had yielded to the pressure, and sheets of canvas were flapping in the air like flags of distress. Truly it was a dreary scene, and I sighed at my dreary prospect."

Here Dr. Holub spent some months, making but one excursion at Christmas time (1872) just within the border of the Free State, to enjoy a little fresh air and have a shot at the baboons. In this portion of his work he gives a very interesting account of the diamond workings at Kimberley, and of the motley crew of workers that he met. Some views of the Kimberley Kopje as it appeared in 1871 are given, one of which (Fig. 1) will give our readers some idea of the dreary aspect of a "diamond quarry." His medical practice increased so rapidly that by the end of January, 1873, he was enabled to purchase a waggon and a good many of the requisites for travel, and early in February he actually started out on his first long journey of exploration, which, however, he from the first only regarded as one of reconnaissance, with the object of getting in part acclimatised to live in the open air, and to acquire by actual experience a knowledge of what would

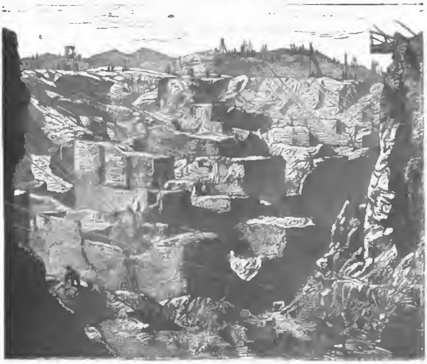


FIG. 1.

be necessary for a prolonged journey into the interior. The first village passed through was Pniel, a German missionary station among the Koranna. With the exception perhaps of the Matabele, no native tribe appeared to have been so little influenced by missionary labour. Their culture is of the very lowest grade. Of all the South African races the Koranna bestow the least labour upon the structure and the least care upon the internal arrangements of their dwellings. Their huts (Fig. 2) consist of a bundle of branches about six feet in length, the upper ends tied together, the lower arranged in a circle, some rush-mats thrown over this hasty framework, in which an aperture is left large enough to admit a human being on all fours. A hollow dug out in the centre is the only fireplace. Scarcely anything worthy of the name of agriculture is carried on, and their chief care is devoted to their corn and goats. Lazy, dirty, untruthful, living without a thought beyond the present, capable of any crime for the sake of drink, it seems no great pity that the tribe is dying out. Crossing the Vaal River, Klipdrift was reached; in the district between the Vaal and Harts Rivers herds of the striped grey gnu (*Catoblepas gorgon*) were to be seen. The roads were here no better than the channels of boulder streams. Formerly both shores of the lower part of the Harts River were in the possession of Yantje, the Batlapin chief, who is a dependant of the British with an income of 200*l*. a year. This chief now resides at Likatlong. The Batlapins are mostly of middle height, not so tall as the Zulus nor so powerfully built as the Fingos. Their complexions are bright and clear; they have very wide noses.

The sugar-cane was cultivated here and there, but the only use made of it was the chewing the more juicy portions of the stem. After Klipdrift, Bloemhof was visited; then the Maquassie River was crossed, and a few days were spent at Klipspruit, which would seem to be quite a paradise for the sportsman; the early morning hours never failed to exhibit many herds of gnus and antelopes. The bushes were the haunt of the guinea fowl. This breed of wild fowl is one of great interest; though hunted perpetually, it would appear to be still on the increase; most frequently it is found in flocks of from ten to forty in number.

Arriving at Wonderfontein, the underground fissures, sometimes several miles long, were examined. One fine cave, known as the "Grotto of Wonderfontein," was partially explored; a little brook rippled through it, and it was thickly inhabited by bats. After much enjoyment of the natural objects associated with this place, Dr. Holub determined to end here his first excursion, and from thence to make his way back to Dutoitspan; slightly altering his track back, he broke somewhat new ground. Here is a short account of an encounter with a mass of feathered life:—

"The bank on which we crouched was the boundary of a depression overgrown with grass and reeds, but now full of rain-water. In this pool were birds congregated in numbers almost beyond what could be conceived—birds swimming, birds diving, birds wading. Perhaps the most conspicuous were the sacred ibises, of which there would not have been less than fifty, some of them standing asleep with their heads under their wings, some of them striding about solemnly, pausing every now and then to make a snap at a smaller victim, and some of them hurrying to and fro, dipping their bills below the water in search of fish. On the far side, as if utterly oblivious of the world, a pair of grey herons stood pensive and motionless. From amongst the weeds, rose the unabated cackle of wild ducks, grey and speckled. Mingling with this were the deep notes of the countless moorhens, while an aspect of perpetual activity was given to the pond by the nimble movements of swarms of little divers. At a spot where the bank descended somewhat sharply to the edge of the pool several ruffs were wandering backwards and forwards, uttering their peculiar shrill whistle, and large flocks of sandpipers were to be noticed, either skimming from margin to margin of the water or resting passively just where they had alighted. The explanation of this enormous concourse of the feathered tribe was very simple. A storm of unwonted violence had washed down from the plain above into the hollow beneath myriads of worms, insects, lizards, and even mice, and so bountiful a banquet had attracted the promiscuous and immense gathering which had excited my wonder."

The second excursion was begun under somewhat better auspices, but it was only contemplated to journey over half the distance between the Diamond Fields and

the Zambesi. The expedition started in November, 1873, with four Europeans, a waggon and eight oxen, with a Griqua driver, a saddle-horse, and nine dogs. From Dutoitspan it first went to Musemanyana, which is the most northerly possession of the Koranna king of Mamusa. On the north and east it is bounded by plains abounding in game, to which the author gave the name of the "Quagga flats." They belong to Montsua, and are the common hunting grounds of Batlapins, Barolongs, Korannas, and the Dutch farmers from the Western Transvaal. From Musemanyana they journeyed to Moshaneng. At Konana they found an immense abundance of game: gnus, blessbocks, hartebeests, springbocks, and zebras grazed in herds. A very interesting account is given of Molema's town: the sale of brandy is prohibited; European cereals have been introduced; two missionaries were found here. Moshaneng is a Bechuana town, with a population of some 7000, many of whom have given up their heathen rites. Moloapolole was the next place visited. It was the residence of King Sechele, of whom we have lately heard a good deal. The number of his subjects was then estimated at about 35,000, while resident but non-tributary tribes amounted to from 18,000 to 20,000 more.

Dr. Holub describes the king, somewhat harshly we think, as looking every inch a hypocrite. The king's house, furnished in European fashion, had cost him some 3000*l*. He was the first of the six Bechuana kings to profess Christianity, but for all that he evidently is a believer in the doctrine that "the end justifies the means." Tea was served in cups; it was good, and the cakes unexceptionable; the sugar-basin, &c., were all of silver.

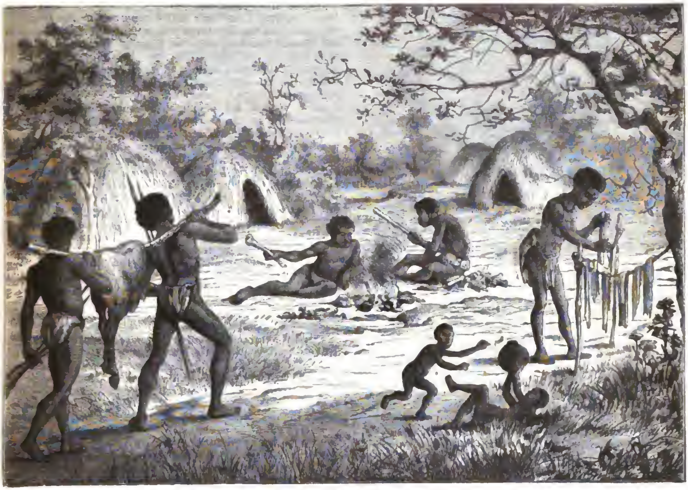


FIG. 2.

The king evidently enjoyed his tea, of which he swallowed nearly a quart. Addressing the king, Dr. Holub said: "When I was only thirteen years of age I read your name in Nyaka Livingstone's book. I little thought then that I should ever see you to speak to you; far more surprising is it to me to find myself drinking tea in your palace." The king, although still said to practise rain magic, replied sanctimoniously, "His ways are past finding out."

The Barwas and the Masarwas, although perhaps not really identical, are known by either name promiscuously amongst the Northern Bechuanas. They may be described as a cross between some branches of the Makalahari and the Bushmen. Their form, complexion, language, and customs afford various indications of their double origin. They are adepts at hunting, and are employed as hunters by their Bechuana masters. They use bows and arrows, and are very adroit in capturing animals by means of poisoned assegais. Their huts look something like large haycocks, consisting of a framework of stakes driven into the earth, fastened together firmly at the top, about five feet from the ground, and covered with a layer of twigs and dry grass. The Masarwas are of medium height, reddish-brown complexion, and a repulsive cast of countenance. They have a great aversion to agriculture and to cattle-breeding. They do not practise stone carving or use any stone utensils. They are very superstitious; treat their wives well, and show a great regard for their dogs. They pierce the nasal cartilage on reaching maturity; wear a body-cloth of hide. They suffer much from cold; but instead of lighting fires in their huts like the Korannas, regularly light these in the open air. The accompanying illustration shows these Masarwas at home.

From Molopole the route lay to Shoshong. This was

the northern limit of this journey. It is the capital of the Eastern Bamangwatos, and the most important town in any of the independent native kingdoms in the interior of South Africa. It lies on the River Shoshon. The king's residence was built around the Kotla; the place has a circular space inclosed by a fence of strong stakes, the entrance being on the south side, opposite to which was an opening leading to another smaller inclosure, which was the king's cattle kraal, where his farm stock was kept at night, the horses being accommodated in the

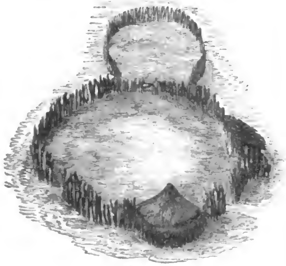


Fig. 3.

Kotla itself. Every night the entrances are made secure with stakes. Fig. 3 shows the king's kotla at Shoshong.

According to the missionary's (Mr. Mackenzie's) estimate, King Sekhomo's actual revenue was equivalent to about 3000*l*. a year, and consisted of cattle, ivory, ostrich-feathers, and skins; he had—happy man!—no state expenditure whatever. Dr. Holub's second expedition ended on April 7, when he arrived in safety with his large collections at Dutoitspan.

(*To be continued.*)

THE NEW INSECTARIUM IN THE ZOOLOGICAL SOCIETY'S GARDENS

ALTHOUGH of late years many entomologists have been in the habit of rearing insects in captivity for the purpose of watching their transformations and obtaining good specimens in each stage of existence, nothing like a systematic attempt, so far as we know, has been made to form a general collection of living insects for exhibition. As in former days as regards reptiles and the lower marine animals, so in the present instance as regards its insectarium, our Zoological Society seems to be first in the field; and so far as we can judge from the progress already made, to be likely, if not altogether successful, to attain many interesting and instructive results.

The building in the Regent's Park Gardens now used as an insectarium is constructed of iron and glass on three sides, with a brick back to it, and formerly formed part of the refreshment buildings. It was moved to its present site, on the north bank of the canal near the north entrance, last autumn, and has been used during the winter as a nursery for delicate monkeys and tropical birds. The cases containing the insects, to which it is now devoted, are arranged on stands all round the building, and also occupy two tables in the centre. The cases used for the principal specimens are formed of zinc plates. The upper part of them is glazed on all four sides, the top being formed of perforated zinc so as to admit the air. The food-plant or object required for the suspension of the chrysalises, when that stage of the insect is exhibited, is inserted into the case through a circular hole in the bottom, but the glass front also opens, so that ready access may be obtained to the interior. The larger cases in the front row measure about 24 inches in breadth by 18 in depth, and are 32 inches in height. The cases in the opposite row are of similar construction, but rather smaller in dimensions.

The cases on the south side (on each side of the entrance door) are mostly appropriated to the exhibition of the larger and finer species of silk-producing moths of the family Bombycidæ. Amongst them may be specially noticed Glover's Silk-moth (*Samia Gloveri*) and the Cercopian Silk-moth (*S. cecropia*) of North America, Perny's Silk-moth (*Attacus Pernyi*) of Northern China, the Tusseh Silk-moth (*A. mylitta*) of India, and the great Emperor Moth (*Saturnia pyri*) of Europe. These have been imported from their native countries in the stage of chrysalis. Of the first three above named, many examples are already hatched, and the splendid *imagines*, or perfect insects, are appearing one by one. Soon after appearing the sexes unite and eggs are produced, after which the parents quickly perish. The fertilised eggs remain to produce caterpillars, which will eventually form a second set of pupæ or chrysalises and thus continue the species.

On the north side of the Insectarium the smaller cases are devoted principally to the rarer and more noticeable moths and butterflies of Europe, such as the Swallow-tailed Butterfly (*Papilio machaon*), the Black-veined Butterfly (*Aporia cratægi*), the Purple Emperor (*Apatura iris*), and the Orange-tip (*Anthocharis cardamines*) among the former, and the Scarlet Tiger Moth (*Callimorpha dominula*) and Emperor Moth (*Saturnia carpini*) among the latter group. The series is continued, mixed with other forms, at the east end of the building. On the large tables in the middle of the Insectarium are examples of other butterflies, moths, beetles, mayflies, stoneflies, and aquatic insects of different kinds, all well worthy of attention and study. The whole series exhibited now contains examples of about fifty species, but daily additions are made to it.

Finally we should mention that every specimen in the Insectarium is distinctly labelled, and that over each of the principal cases is fixed a glazed box, in which are placed preserved specimens of the various stages of metamorphosis of the insect exhibited in the case beneath.

Nor must we forget to add that the Insectarium is under the sole charge of Mr. E. Watkins, an experienced entomologist and breeder of insects, whose name is well known to many naturalists. Mr. Watkins, whose services have been secured for the Zoological Society for the purpose of inaugurating this interesting exhibition, is in daily attendance at the Insectarium, and is most ready and willing to afford information and instruction to all who apply to him.

NOTES

THE numerous friends and still more numerous admirers of Prof. Gegenbaur will be glad to hear that he is now believed to be out of danger. It appears that the illness of the distinguished anatomist commenced with an attack of erysipelas, the origin of which is not known; it does not seem that the case was complicated by any blood poisoning, but there was an attack of peritonitis, which caused the very greatest anxiety for some days. Lately however his condition has considerably improved, but it will, of course, be a long time before he can be completely restored to health.

AT the *conversazione* given to Prof. Helmholtz at University College, Mr. Latimer Clark exhibited the accompanying

interesting unpublished letter from Sir Isaac Newton to Dr. Law :—

"*London, Dec.* 15, 1716

"Dear Doctor : He that in ye mine of knowledge deepest diggeth, hath, like every other miner ye least breathing time, and must sometimes at least come to terr ; alt for air.

"In one of these respiratory intervals I now sit doune to write to you, my friend.

"You ask me how, with so much study, I manage to retene my health. Ah, my dear doctor, you have a better opinion of your lazy friend than he hath of himself. Morpheous is my best companion ; without 8 or 9 hours of him yr correspondent is not worth one scavenger's peruke. My practizes did at ye first hurt my stomach, but now I eat heartily enow as y' will see when I come down beside you.

"I have been much amused by ye singular φενομενα resulting from bringing of a needle into contact with a piece of amber or resin fricated on silke clothe. Ye flame putteth me in mind of sheet lightning on a small—how very small—scale. But I shall in my epistles abjure Philosophy whereof when I come down to Sakly I'll give you enow. I began to scrawl at 5 mins frm 9 of ye clk, and have in writing consmd 10 mins. My Ld. Somerset is announced.

"Farewell, Gd bless you and help yr sincere friend

"(Signed) ISAAC NEWTON

"To Dr. Law, Suffolk"

VERY great progress is being made in Paris to render the forthcoming Electric Exhibition a success. There are sixty-four English exhibitors. The Post Office is going to make a very good display, and as the old apparatus of Ampère and Œrsted will be shown, it is hoped that those of Faraday and Wheatstone will be added.

L'Électricien is the title of a new fortnightly journal published in Paris and devoted to the interests of the science of electricity. It might have been thought that with *L'Électricité*, edited by M. Wilfrid de Fonvielle, and *La Lumière Électrique*, edited by M. le Comte du Moncel, appearing every week, the field would have been fully occupied. The latter journal is however somewhat more special in its aims, and the former appears to be at present given over to discursive maunderings on natural photophony and to rabid attacks upon Clerk-Maxwell's theory of electricity. At any rate there appears to be scope for a journal of a somewhat different order ; and the pages of No. 1, now before us, contain valuable contributions from well-known pens. M. Mercadier contributes an article on the use of selenium in the photophone ; M. Niaudet-Breguet writes upon the different systems adopted for central stations in telephone exchanges ; Dr. de Cyon has an interesting article on electrobiology ; M. Gaston Tissandier discourses on one of the domestic applications of electricity ; while Prof. C. M. Gariel contributes a valuable discussion of the graphic method of representing Ohm's law and other laws of current electricity. The acting editor is M. E. Hospitalier, the well-known electrical engineer. The publication, which is illustrated, is got up in admirable style by the house of Gustave Masson. We wish all success to the undertaking so excellently begun.

THE Paris Municipal Laboratory for testing all matters having any bearing on health, and the organisation of which is now quite complete, was opened to the public on March 1. The establishment, which is situated at the Prefecture of Police, Quai du Marché Neuf, will be formally inaugurated to-morrow. The laboratory is already regarded as a success, the number of objects presented for analysis amounting in April to not less than 700, mostly wine purchased in shops, and suspected of being adulterated. The number of falsifications amounts to 80 out of 100. In every case where adulteration has been detected the results have been communicated to the competent authorities, who have prosecuted. Milk has been also sent in great quantity, and in many cases proved adulterated or mixed with water. The results of these inquiries have created such an agitation among Parisian milkmen that when they were surrounded at Batignoles Terminus and their boxes about to be opened for inspection, they resisted. A scuffle ensued between them and the police, and the result was that a number escaped. French chocolate has also been found very defective in quality, an immense number of substances having been added to it. The head of this new service is M. Ch. Gerard, a chemist of reputation. All the assistants are selected by competitive examination, and are only to remain in the service for a few years. They belong mostly to the School of Medicine and Pharmacy, so that the institution may be considered as a public school of practical chemistry. The general organisation is said to be modelled after the Chemical Laboratory at South Kensington. Notable features are the use of spectroscopic analysis combined with the electric spark, a workshop for photography, and the special service for trichinæ. The ordinary market-inspectors are trained to use special microscopes for that purpose. A special instrument has been constructed for boring in ham small holes which are not visible when cooked, and the particles of flesh so extirpated are analysed microscopically. A special apparatus has been designed and is in constant use for trying swine, and even the muscles of patients.

MR. MORRIS, the Director of Public Gardens and Plantationg in Jamaica, has recently issued a pamphlet entitled "Notes on Liberian Coffee, its History and Cultivation." In this pamphlet Mr. Morris has brought together a great deal of valuable matter connected with this remarkable species of *Coffea*, which will prove not only interesting to those who wish to see the resources of our Colonies developed, but particularly to those about to embark in the cultivation of coffee as an article of commerce. The pamphlet commences with some historical remarks on the species, and then touches on its introduction into Jamaica, followed by a consideration of the plant as found in Liberia, in the West and East Indies, of its propagation and the establishing of plantations with regard to climate, soil, and various other details ; some interesting notes follow on the yield of Liberian coffee trees, and of the commercial value of the coffee itself. In view of this pamphlet being of considerable use to persons abroad who may be about to embark in the cultivation of this particular species, we may say that it is issued from the Government Printing Establishment at Jamaica, and that its price is sixpence.

A NEW medicinal oil has just been introduced into this country by Messrs. Burgoyne and Burbidges, the well-known chemists of Coleman Street. It is known as Oolachan oil, and is said to be scarcely distinguishable from cod liver oil. It is obtained from a fish called by the North American Indians Oolachan, or candle fish, from the fact that when dried the fish itself can be used as a torch or candle on account of the large quantity of oleaginous matter it contains. The fish is met with on the coasts of Vancouver's Island and British Columbia, and in the bays between the Frazer and Skuna Rivers. Similar in its habits to the salmon, it ascends the rivers to spawn once a year, but remains only for a very short period, sometimes not more than a day, and as this is the only time they can be caught by the Indians, the manufacture of the oil is somewhat precarious. The fish itself, which is about the size of a herring, is much esteemed by the Indians on account of its delicacy of flavour and valuable medicinal properties. In America the oil has already a great reputation as a valuable and efficient substitute for cod liver oil, and there is every probability as it becomes known in this country of its taking a prominent place as an important medicine.

M. HERVÉ-MANGON, the director of the Conservatoire des Arts et Métiers, has established a manufacture of pottery in the large hall, in order to make the Parisian public acquainted with several of the manipulations used in the large manufactories. This demonstration, which will be continued during several

Sundays, bears principally on the use of the lathe for modelling. M. Hervé Mangon, having established a Siemens electromagnetic machine for lighting purposes at the Conservatoire, sends by request supplies to the several laboratories of the establishment. Up to the present moment it has been used only by photographers.

AT the adjourned ordinary meeting of the Sanitary Institute, to be held at 9, Conduit Street, on Wednesday, May 18, at 8 p.m., the discussion will be continued upon the address delivered by Dr. Richardson, F.R.S., Chairman of Council—"Suggestions for the Management of Cases of Small Pox, and of other Infectious Diseases in the Metropolis and Large Towns."

AT the meeting of the Iron and Steel Institute last week the papers were almost entirely of a purely technical or commercial character.

MR. CHARLES E. TURNER, Lector at the University of St. Petersburg, will begin a course of five lectures at the Royal Institution, on the Great Modern Writers of Russia—Pouschkin, Lermontoff, Gogol, Tourgenieff and Nekrasoff—on Saturday, the 21st.

AN International Medical Congress meets at Madrid on the 20th inst.

THE extinction of the Brush electric light in the City last week is stated to have been caused by the defective insulation of the wires.

ALL the large railway companies in the country have intimated their intention of sending engines to the typical engine exhibition to be held at Newcastle on the occasion of the Stephenson centenary.

THE annual meeting of the U.S. Society for the Promotion of Agricultural Science will be held at Cincinnati on Tuesday, August 16, the day preceding the session of the American Association for the Advancement of Science.

THE fifth and concluding course of Cantor Lectures for the present session at the Society of Arts will be by Mr. R. Brudenell Carter, on the subject of "Colour Blindness, and its Influence on Various Industries." The course consists of three lectures, the first of which will be delivered upon Monday next, the 16th inst. This lecture will deal generally with the subject. The second lecture will treat of methods of testing for colour blindness, the prevalence of the affection, mistakes of the colour blind, and methods of endeavouring to counteract the defect. The subject of the third lecture is specially the industries chiefly affected by colour blindness. In it an account will be given of recent legislation on the subject in America, and the necessity for it in this country.

MASSON of Paris has issued a third series of Prof. Paul Bert's "Revues scientifiques," published in the *République Française*.

THE Annual Report of the Belfast Naturalists' Field Club for 1879-80 tells of its continued prosperity, and contains an account of the various excursions made during last summer. Appended are "A List of the Post-Tertiary Foraminifera of the North-East of Ireland," by Joseph Wright, F.G.S., and "A List of the Mollusca of the Boulder Clay of the North-East of Ireland," by S. A. Stewart.

THE Birmingham Natural History Society has issued a *Report and Transactions* for 1880, which in quantity and quality does its members great credit. There is an interesting address by the president, Mr. W. Southall, and a number of natural history papers, some by outsiders, and one or two on subjects connected with local natural history. The Society is now housed in the Mason College.

IN compliance with the provisions of a recent decree, the system of Algerian telegraphy has been *rattaché* to the French administration, and is governed from Paris. The head of the Algerian service has been appointed director at Lyons.

THE annual *conversazione* given by the President and Council of the Royal Society was held on Wednesday last week. It was well attended, and there were numerous scientific and artistic novelties on view.

MR. E. IM THURM is writing on Aspects of Plant Life in British Guiana, in the *Gardeners' Chronicle*.

THE additions to the Zoological Society's Gardens during the past week include a Black-faced Spider Monkey (*Ateles ater*) from Eastern Peru, a Collared Peccary (*Dicotyles tajaçu*) from South America, presented by Mr. E. H. Dance; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Sir Charles C. Smith, Bart.; a Swift (*Cypselus apus*), European, presented by Mr. H. H. Johnston; a Common Viper (*Vipera berus*), British, presented by Mr. John Poyer Poyer.

OUR ASTRONOMICAL COLUMN

THE COMET OF 1812.—Under certain suppositions as regards the epoch of perihelion passage of this comet, the return of which may now be expected, it will be necessary to search for it on a particular date, upon the assumption that it has yet a considerable orbital angle to describe before arriving in perihelion, because the geocentric position corresponding to a small orbital angle will place the comet too near to the sun's position to allow of observations. If we employ the elliptical elements deduced by Mr. W. E. Plummer from a new reduction and discussion of several of the most reliable series of observations in 1812, we find the following values of the comet's heliocentric equatorial co-ordinates and of the radius vector for intervals of 100 days to 60 days before perihelion passage; the co-ordinates are referred to the equinox of 1881·0.

Time from perihelion.	*x*.	*y*.	*z*.	Log. radius-vector.
− 100 days	+0·5619	−0·5939	+1·6649	0·2683
− 90 ,,	+0·5478	−0·4432	+1·5725	0·2363
− 80 ,,	+0·5305	−0·2904	+1·4712	0·2016
− 70 ,,	+0·5093	−0·1357	+1·3592	0·1637
− 60 ,,	+0·4832	+0·0209	+1·2337	0·1222

Combining these co-ordinates with the X, Y, Z of the *Nautical Almanac* for May 27·5 and June 26·5, days of new moon in the present year, we get the following results:—

For May 27·5

t.	R.A. °	Decl. °	Distance from earth.	Intensity of light.
− 100 days	15·2	+63·9	2·267	0·057
− 90 ,,	23·5	62·0	2·201	0·070
− 80 ,,	31·2	59·4	2·140	0·086
− 70 ,,	38·3	56·2	2·083	0·108
− 60 ,,	44·8	+52·2	2·030	0·139

For June 26·5

t.	R.A. °	Decl. °	Distance from earth.	Intensity of light.
− 100 days	35·7	+74·5	2·146	0·063
− 90 ,,	47·1	71·4	2·084	0·078
− 80 ,,	55·8	67·6	2·027	0·096
− 70 ,,	62·5	63·1	1·976	0·120
− 60 ,,	67·8	+57·9	1·931	0·153

These places will define the region of the sky where the comet should be sought, and telescopes of good optical capacity will be needed. When Pons discovered the comet on July 20, 1812, the theoretical intensity of light was 0·18.

The mean motion in 1812 not being ascertainable within very narrow limits, no attempt, so far as we know, has been made to determine the effect of perturbation in the present revolution, and we have therefore to be content with the method of careful sweeping over the region of the sky, on which the orbit may be projected at any time. Sir George Airy's orbit-sweeper, it is true, would limit the extent of sky-ground to be examined, but we suspect the only instrument of sufficient power yet mounted upon his principle is that at the Imperial Observatory at Strassburg, where it is not to be doubted that it will be put in active operation by Prof. Winnecke. We may remind the reader that

sweeping ephemerides for the whole year were published from Strassburg some time since, and will be found in the *Vierteljahrsschrift der Astronomischen Gesellschaft*, Jahrgang 12. Those given above apply to greater distance from perihelion.

THE TRANSIT OF VENUS, 1882.—At the sitting of the Paris Academy of Sciences, on the 2nd inst., the Minister of Foreign Affairs transmitted a letter from the British Ambassador, on the part of his Government, desiring to be informed with which French authorities the Royal Society of London should communicate, with the view to an interchange of opinions relative to the observation of the approaching transit of Venus. The letter was referred to a commission already nominated.

COMET 1880, V. (PECHÜLE, DECEMBER 16).—This comet was followed by M. Bigourdan until March 31, efforts having been made at the Observatory of Paris to observe it as long as possible on account of the resemblance of the orbit to that of the great comet of 1807. M. Bigourdan's last elements gave the place with errors of only 2s.·0 in right ascension, and 20″ in declination: they will be found in *Comptes rendus*, vol. xcii. p. 172.

COMET 1881, *a* (SWIFT, MAY 1).—We have received from the Imperial Observatory of Strassburg the following observation of the new comet, made by Dr. Hartwig with the "orbit-sweeper":—

May 5, at 14h. 56m. 9s.·8 mean time at Strassburg.
Right Ascension 0h. 19m. 17s.·76; Declination $+32° 19' 32''\cdot3$.

CHEMICAL NOTES

IN the *American Chemical Journal* Prof. Mallet describes a simple form of calorimeter whereby the specific heats of moderately small quantities of solids or liquids may be measured with a fair degree of accuracy. Mercury is employed, instead of water, as the material whose temperature is raised, and comparison is made, not of the total amounts of heat given out by different bodies on cooling, but of fractions of this heat rendered as nearly as possible equal.

IN *Compt. rend.* Berthelot gives several thermal measurements showing that in the substitution of halogens for hydrogen in hydrocarbons, the quantity of heat evolved varies according to the series and chemical function of the hydrocarbons employed, and is generally smaller the greater the number of halogen atoms substituted. The heat of formation of chloral alcoholate in various physical states is also considered by Berthelot: among other results it is shown that chloral hydrate is decomposed by an excess of absolute alcohol, but that the alcoholate is decomposed by much water; in the former of these actions there is exhibited the decomposition of a more volatile compound—chloral hydrate—and formation of a less volatile—chloral alcoholate; and at the same time the expulsion of a less volatile substance—water—by a more volatile—alcohol.

THE proto-salts of chromium (or chromous salts) are unstable and but little known: in *Compt. rend.* M. Moissau describes two salts belonging to this series, viz., chromous chloride, $CrCl_2$, and chromous sulphate, $CrSO_4 7H_2O$.

IN the *Berichte* of the German Chemical Society Herr C. Zimmermann states that potassium permanganate may be used for determining iron in presence of considerable quantities of hydrochloric acid, if a solution of manganous chloride, or preferably manganous sulphate, be added previous to titration.

ACCORDING to the hypothesis of Vant' Hoff, propyl glycol ought to be an optically active liquid, inasmuch as the molecule of this compound contains one *asymmetric* carbon atom, *i.e.* an atom directly united with four different radicles;—propyl glycol being formulated as $\begin{array}{c} CH_3{-}C{-}CH_2OH \\ \diagup\;\diagdown \\ H\quad OH \end{array}$. Le Bel has recently shown (*Compt. rend.*) that if ordinary propyl glycol—from glyceric acid—be subjected to partial fermentation, the unfermented residue exhibits slight dextrorotatory powers. Le Bel thinks that ordinary propyl glycol contains both an optically active and an optically inactive modification, and that the latter being decomposed by the ferment, the presence of the former is rendered evident. From optically active propyl glycol Le Bel has prepared an active propylene oxide boiling at 35°, which he states is the most volatile optically active compound at present known.

A SERIES of optically active amylamines is described in *Compt. rend.* by M. Plimpton. These compounds are obtained from amyl bromide—from active amylic alcohol—by the action of alcoholic ammonia.

IN *Chem. Centralblatt* E. Ludwig describes experiments on the localisation of arsenic, absorbed as arsenious oxide, in the animal organism: contrary to the results of many former experimenters, Ludwig asserts that an accumulation of arsenic occurs in the liver; neither the bones nor the brain retain arsenic for any length of time. Arsenic was detected in the liver of a dog forty days after the last dose had been administered, but no trace could be found in the brain, bones, or muscles. Ludwig's results are generally confirmed by Johnson and Chittenden (*Amer. Chem. Journ.*).

MM. DES CLOIZEAUX AND DAMOUR describe (*Compt. rend.*) a new selenite of copper, to which they give the name *Chalcomenite*. The mineral occurs in the Argentine Republic, in small green clinorhombic crystals, associated with selenite of lead, and selenite of lead and copper.

M. SULLIOT proposes (*Compt. rend.*) to employ "chamber crystals" as a disinfectant. He places a solution of these crystals in sulphuric acid in the room or other place to be disinfected; the atmospheric moisture slowly decomposes the liquid with liberation of oxides of nitrogen, which destroy noxious organic matter present in the air.

MR. M. W. WILLIAMS describes, in *Chem. Soc. Journal*, a method for freeing water, to be analysed by the process of Frankland and Armstrong, from nitrates and ammonia. He digests the water with carefully-prepared "copper-zinc couple," whereby all nitrates are reduced to ammonia; he then distils off ammonia, evaporates to dryness, and proceeds in the usual manner. The use of sulphurous acid, which has always been much objected to, is thus obviated.

IN the same journal there is a suggestive paper by Prof. Hartley on the "Relation between the molecular structure of carbon compounds and their absorption-spectra." Evidence is accumulated in favour of the view that the selective absorption exhibited by "aromatic" compounds depends on the vibrations of the carbon atoms within the molecule, but that these atomic vibrations are dependent upon the nature of the molecular vibrations themselves, and are probably to be regarded as harmonics of these fundamental vibrations.

THE second and third parts of the *Gazetta Chimica Italiana* for the present year exhibit very unmistakably the activity of Italian chemists, chiefly in the domain of organic chemistry. Schiff continues his researches on Glucosides; the derivatives of thymol are studied by Paternò and Canzoneri; Macagno describes experiments on the spectroscopic detection of artificial colouring matters in wines. Koenig, Schiaparelli, Barbaglia, and other known chemists contribute papers.

GEOGRAPHICAL NOTES

MR. EDWARD WHYMPER on Monday last addressed a large meeting of the Geographical Society on some features in his recent journey among the Great Andes of the Equator. His paper was not, however, of so popular a nature as those which he read before the Alpine Club and the Society of Arts. The chief facts left on the minds of his very attentive audience may be briefly stated Mr. Whymper found by careful experiments that aneroid barometers are not to be depended upon for the determination of heights, and that there is a remarkable difference in altitudes as fixed by the boiling point of water and the mercurial barometer. He asserted, as the result of his observations, that it is a mistake to suppose that there are two parallel chains in the Ecuadorian Andes, as usually shown on our maps. This is a point, however, on which more light is evidently required. Mr. Whymper's account of his ascent of the hitherto unknown peak called Sara-Urcu, was very interesting, and this achievement alone would stamp him a mountaineer of the highest skill and courage.

THE following award has just been made of the medals given annually by the Council of the Geographical Society for competition among a limited number of public schools:—Physical Geography (Mr. H. N. Moseley, F.R.S., examiner): Gold medal, E. G. Reid, Dulwich College; silver medal, Sydney Edkins, City of London School; Political Geography (Right Rev. Bishop Abraham, examiner): Gold medal, Theodore

Brooks, London International College; silver medal, Chas. Th. Knaus, Dulwich College.

THE *Willem Barents*, the little Polar ship which has already made three voyages to the Northern Polar Sea, has left Amsterdam for the fourth time. The crew consists of a lieutenant of the Royal Navy, H. van Broekhuyzen, as captain, two other officers, a physician, a zoologist, a photographer (the Englishman, Mr. Grant), and six sailors.

DOCTORS ARTHUR AND AUREL KRAUSE have left Bremen to spend some time in the neighbourhood of Behring Straits for the purpose of exploring and collecting, at the expense of the Bremen Geographical Society. They will visit the Chukchi peninsula, Behring Islands, and Alaska, where they will make zoological collections and carry on various scientific observations.

MR. HENRY SOLTAU and Mr. J. W. Stevenson, of the China Inland Mission, have successfully made the journey from Bhamo into China, reaching I-chan-fu, on the Yang-tsze-kiang, on March 14. This is the first time that it has been accomplished by Europeans, and the time occupied was about four months.

THE first paper in the May number of *Petermann's Mittheilungen* is a study of the Padolian Dniester region by Ritter v. Habdank Dumkowski. This is followed by the continuation of Dr. Radde's account of his journey to Talysh, Aderbeijan and the Sawalan; M. Charnay's expedition to Central America from the *North American Review*; M. Potanin's researches in Western Mongolia in 1876-77, with a map; Recent Surveys in the Western United States, with a map; and the usual Monthly Notes. Among the latter is a long account of Dr. Lenz's journey to Timbuctoo, with a sketch-map.

WE have received Nos. 1 and 3 of the *Bulletin* of the American Geographical Society, the two most important papers in which are on the recent investigations of the Gulf Stream, by the U.S. Coast Survey steamer *Blake*, by Commander J. R. Bartlett, and Changes in the Physical Geography of the Ancient Home of Man in Central and Western Asia, by the Rev. Owen Street.

DR. MOFFAT, the venerable missionary and pioneer explorer in Africa, was entertained at a banquet in the Mansion House on Saturday.

THE *Bulletin* of the Society of Commercial Geography of Bordeaux contains a brief statement of M. Paul Soleillet's views on the African question. After addressing the Society M. Soleillet proceeded to Paris, but he entertains hopes of being able to return to West Africa in November.

THE PRODUCTION OF SOUND BY RADIANT ENERGY [1]

IN my Boston paper on the photophone (NATURE, vol. xxii. p. 500) the discovery was announced that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight. The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of the various substances which became sonorous in the condition of thin diaphragms, but this failure was explained upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was therefore supposed that if we could lead to the ear air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. The first experiments made to verify this hypothesis pointed towards success. A beam of sunlight was focussed into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam, a clear musical tone was heard, the pitch of which depended upon the frequency of the interruption of the light and the loudness upon the material composing the tube.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds produced by masses, but would also permit us to test the more general proposition that sonorousness, under the influence of intermittent light, is a property common to all matter. The substance to be tested was to be placed in the interior of a transparent vessel made of some material which (like glass) is transparent to light but practically opaque to sound.

Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing tube.

Some preliminary experiments were made in Paris to test this idea, and the results were so promising that they were communicated to the French Academy on October 11, 1880, in a note read for me by Mr. Antoine Breguet.

I wrote to Mr. Tainter suggesting certain experiments, and upon my return to Washington in the early part of January, Mr. Tainter communicated to me the results of the experiments he had made in my laboratory during my absence in Europe. He had commenced by examining the sonorous properties of a vast number of substances inclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard rigid bodies like crystals, or diaphragms such as we had hitherto used.

In order to study the effects under better circumstances he inclosed his materials in a conical cavity in a piece of brass closed by a flat plate of glass. A brass tube leading into the cavity served for connection with the hearing-tube. When this conical cavity was stuffed with worsted or other fibrous materials the sounds produced were much louder than when a test-tube was employed. Mr. Tainter next collected silks and worsteds of different colours, and speedily found that the darkest shades produced the best effects. Black worsted especially gave an extremely loud sound.

About a teaspoonful of lamp-black was placed in a test-tube and exposed to an intermittent beam of sun-light. The sound produced was much louder than any heard before. Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lamp-black surface towards the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lamp-black surface turned from the sun the sound was much feebler.

Upon smoking the interior of the conical cavity and then exposing it to the intermittent beam with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube. The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver.

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor reinforcements were constantly occurring, which became more and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The extremely loud sounds produced from lamp-black have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lamp-black receiver at forty metres distance.

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the colour are two conditions that markedly influence the intensity of the sonorous effects. The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colours. The materials from which the best effects have been produced are cotton-wool, worsted, fibrous materials generally, cork, sponge, platinum and other metals in a spongy condition, and lamp-black.

The loud sounds produced from such substances may perhaps be explained in the following manner:—Let us consider, for

[1] Abstract of a paper by Prof. Alexander Graham Bell, read before the National Academy of Arts and Sciences, April 21, 1881.

example, the case of lamp-black—a substance which becomes heated by exposure to rays of all refrangibility. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass the particles of lamp-black are heated, and consequently expand, causing a contraction of the air-spaces or pores among them. Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge. The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lamp-black. When the light is cut off the converse process takes place. The lamp-black particles cool and contract, thus enlarging the air-spaces among them, and the inclosed air also becomes cool. Under these circumstances a partial vacuum should be formed among the particles, and the outside air would then be absorbed as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon lamp-black, and a wave of rarefaction is originated when the light is cut off. We can thus understand how it is that a substance like lamp-black produces intense sonorous vibrations in the surrounding air, while at the same time it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.

In his paper read before the Royal Society on March 10 Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to the vibrations of the confined air, and that the disks do not vibrate at all.

But for reasons stated Mr. Bell concludes that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, independently of any expansion and contraction of the air confined in the cavity behind the diaphragm. Lord Rayleigh has shown mathematically that a to-and-fro vibration, of sufficient amplitude to produce an audible sound, would result from a periodical communication and abstraction of heat, and he says: "We may conclude, I think, that there is at present no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating" (NATURE, vol. xxiii. p. 274).

[Mr. Bell then describes experiments (devised by Mr. Tainter) which have given results decidedly more favourable, in his opinion, to the theory of Lord Rayleigh than to that of Mr. Preece.]

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here, and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment.[1]

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absorptive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light, but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment. In the experiments made in my laboratory a very long test-tube was filled with the liquid under examination, and a flexible rubber-tube was slipped over the mouth far enough down to prevent the possibility of any light reaching the vapour above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focussed upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

Results

Clear water	No sound audible.
Water discoloured by ink	Feeble sound.
Mercury	No sound heard.
Sulphuric ether *	Feeble, but distinct sound.
Ammonia	,, ,,
Ammonio-sulphate of copper	,, ,,
Writing ink	,, ,,
Indigo in sulphuric acid	,, ,,
Chloride of copper *	,, ,,

The liquids distinguished by an asterisk gave the best sounds.

[1] Carbon and thin microscope glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy (*Comptes rendus*, vol. xcl., p. 595) All these substances have since yielded sounds under more careful conditions of experiment.

Acoustic vibrations are always much enfeebled in passing from liquids to gases, and it is probable that a form of experiment may be devised which will yield better results by communicating the vibrations of the liquid to the air through the medium of a solid rod.

The vapours of the following substances were found to be highly sonorous in the intermittent beam:—Water vapour, coal gas, sulphuric ether, alcohol, ammonia, amylene, ethyl bromide, diethylamene, mercury, iodine, and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen. I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition (solid, liquid, and gaseous), and the probability is therefore very greatly increased that sonorousness under such circumstances will be found to be a universal property of matter.

At the time of my communication to the American Association the loudest effects obtained were produced by the use of selenium, arranged in a cell of suitable construction and placed in a galvanic circuit with a telephone. But the selenium was very inconstant in its action, and from experiments by Dr. Chichester Bell of University College of London, it was found that all the selenium used was tainted with impurities.

Prof. W. G. Adams (*Proceedings* Royal Society, vol. xxiv. p. 163) has shown that tellurium, like selenium, has its electrical resistance affected by light, and we have found that when this tellurium spiral is connected in circuit with a galvanic battery and telephone and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone.

It occurred to Mr. Tainter before my return to Washington last January that the very great molecular disturbance produced in lamp-black by the action of intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lamp-black could be employed in place of selenium in an electrical receiver. This has turned out to be the case, and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment, and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects, but our researches are not yet complete. When a beam of light is brought to a focus by means of a lens the beam diverging from the focal point becomes weaker as the distance increases in a calculable degree. Hence if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity we can calculate their relative sonorous powers. Preliminary experiments were made by Mr. Tainter during my absence in Europe to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between different substances in this respect.

Distance from Focal Point of Lens at which Sounds become Inaudible with Different Substances

	m.
Zinc diaphragm (polished)	1·51
Hard rubber diaphragm	1·90
Tin foil ,,	2·00
Telephone ,, (japanned iron)	2·15
Zinc ,, (unpolished)	2·15
White silk (in receiver)	3·10
White worsted ,,	4·01
Yellow worsted ,,	4·06
Yellow silk ,,	4·13
White cotton-wool ,,	4·38
Green silk ,,	4·52
Blue worsted ,,	4·69
Purple silk ,,	4·82
Brown silk ,,	5·02
Black silk ,,	5·21
Red silk ,,	5·24
Black worsted ,,	6·50
Lamp-black. In receiver the limit of audibility could not be determined on account of want of space. Sound perfectly audible at a distance of	10·00

Mr. Tainter was convinced from these experiments that this field of research promised valuable results, and he at once de-

vised an apparatus for studying the effects, which he described to me upon my return from Europe. [The apparatus has since been constructed, and Mr. Bell gave a detailed description of it.]

The meaning we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage quoted from my Boston paper:—

"Although effects are produced as above shown by forms of radiant energy, which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone,' because an ordinary beam of light contains the rays which are operative."

To avoid in future any misunderstandings upon this point we have decided to adopt the term "*radiophone*" proposed by M. Mercadier as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone*, *photophone*, and *actinophone* to apparatus for the production of sound by thermal, luminous, or actinic rays respectively. M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum (*Comptes rendus*, December 6, 1880). We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier. A beam of sunlight was reflected from a heliostat through an achromatic lens, so as to form an image of the sun upon the slit. The beam then passed through another achromatic lens and through a bisulphide of carbon prism, forming a spectrum of great intensity, which, when focussed upon a screen, was found to be sufficiently pure to show the principal absorption lines of the solar spectrum. The disk-interrupter was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the receiver, which was so arranged that the lamp-black surface exposed was limited by a slit, as shown.

Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to decrease, and then stopped so suddenly that a very slight motion of the receiver made all the difference between almost maximum sound and complete silence.

2. The lamp-blacked wire gauze was then removed and the interior of the receiver was filled with red worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

3. Upon substituting green silk for red worsted the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

4. Some hard-rubber shavings were now placed in the receiver. The limits of audibility appeared to be on the one hand the junction of the green and blue, and on the other the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red, and to his ear the maximum was about the junction of the red and orange.

5. A test-tube containing the vapour of sulphuric ether was then substituted for the receiver. Commencing at the violet end, the test-tube was gradually moved down the spectrum and out into the ultra-red without audible effect, but when a certain point far out in the ultra-red was reached a distinct musical tone suddenly made its appearance, which disappeared as suddenly on moving the test-tube a very little further on.

6. Upon exploring the spectrum with a test-tube containing the vapour of iodine the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

7. A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were obtained in all parts of the visible spectrum, but no sounds were observed in the ultra-red.

The maximum effect seemed to me to be in the blue. The sounds were well-marked in all parts of the violet, and I even fancied that the audible effect extended a little way into the ultra-violet, but of this I cannot be certain. Upon examining the absorption-spectrum of peroxide of nitrogen it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption lines made their appearance.

8. The spectrum was now explored by a selenium cell, and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red. The audible effect extended a little way into the ultra-red on the one hand and up as high as the middle of the violet on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that the nature of the rays that produce sonorous effects in different substances depends upon the nature of the substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.

Our experiments upon the range of audibility of different substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis. The eye-piece of a spectroscope is removed, and sensitive substances are placed in the focal point of the instrument behind an opaque diaphragm containing a slit. These substances are put in communication with the ear by means of a hearing-tube, and thus the instrument is converted into a veritable "spectrophone."

Suppose we smoke the interior of our spectrophonic receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum (visible and invisible) except the ultra violet. Now pass a rapidly-interrupted beam of light through some substance whose absorption spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption bands. Of course the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum; but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum lamp-black alone may be used in the spectrophonic receiver. Indeed the sounds produced by this substance in the ultra-red are so well marked as to constitute our instrument a most reliable and convenient substitute for the thermo-pile. A few experiments that have been made may be interesting.

1. The interrupted beam was filtered through a saturated solution of alum.

Result: The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

2. A thin sheet of hard rubber was interposed in the path of the beam.

Result: Well-marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time.

3. A solution of ammonia-sulphate of copper was tried.

Result: When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear however the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination, but I do not wish you to understand that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man, and I feel that our first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognise the fact that the spectrophone must ever remain a mere adjunct to the spectroscope, but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption spectra in the ultra-red.

ON AN ACOUSTIC PHENOMENON NOTICED IN A CROOKES TUBE [1]

A SHORT time since, while experimenting with a Crookes tube, I noticed a phenomenon which was quite striking, and so evident that it hardly seems possible that it has not frequently been observed before; but as no allusion to the effect in question has come to my notice, I venture to call attention to it.

In working with the tube in which a piece of sheet platinum is rendered incandescent by the concentration upon it of electrified particles, repelled from a concave mirror, I noticed that when the mirror was made the negative electrode, so that this concentration took place, a clear and quite musical note issued from the tube. I thought at first that the pitch of the note would coincide with that produced by the circuit-breaker used with the coil (which made about 100 breaks per second), but this did not prove to be the case. In fact very great changes in the rate of the circuit-breaker did not affect the note given by the tube. The effect seemed to be produced by the vibration of the sheet-platinum in its own period, under the influence of the molecular impact, which vibration was communicated to the glass walls of the tube by the enamel rod to which the platinum was attached, giving rise to a sound somewhat resembling the pattering of rain against a window-pane, but higher in pitch and more musical. This sound changed its character very greatly when the direction of the current was reversed, a feeble murmur only being heard. I obtained a similar musical note, though far less loud, with the "mean free-path tube," best when the middle plate was positive. With a tube containing phosphorescent sulphide of calcium, the note was very dull in its quality and low in pitch, but still quite perceptible. With this tube a change in the direction of the current, as might be expected, did not affect the sound produced. I did not obtain this musical note from any tube that I have in which the current enters and leaves by a straight wire, except in the case of a single Geissler's tube exhausted so as to give stratifications, in which it was very feebly heard.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A LARGE number of ladies and gentlemen assembled on Tuesday in Cowper Street to witness the laying of the foundation-stone of the Finsbury Technical College which it has lately been resolved to establish by the City and Guilds of London Institute, by His Royal Highness Prince Leopold. According to the report of the Council of the Institute to the Governors, the projected building is estimated to cost 20,382*l.*, exclusive of the professional charges, fittings, and other incidental expenses, which will amount to some 5000*l.* In the plans ample accommodation is provided for instruction in the application of physics, chemistry, and mechanics to the various industries. The building will contain thirty-two rooms, including a large laboratory, two lecture theatres, class, drawing, private, and engine rooms, workshops, and clerks' offices. Lord Selborne, in welcoming the Prince, after noting the progress of science as applied to arts and manufactures in this and other countries, said that in the race of competition the prize must in the end belong to those who best knew how to build the superstructure of arts and manufacture on their handicrafts with a sound foundation of scientific knowledge. The ceremony of laying the stone was gone through by Prince Leopold, who in the course of his remarks said that the institution has proclaimed its determination to enter into generous rivalry with other countries in those branches of trade and commerce in which, one must needs confess, our native industries have of late years not taken that position which we as Englishmen would wish them to occupy. We are beginning to realise that a thorough and liberal system of education must be placed within the reach of the British artisan in order to enable him to hold his own against foreign competition. Mr. Mundella said that by instituting this college they were taking the same step in applying science to industries which had been taken in applying arts to manufactures at South Kensington. Among the articles deposited in the cavity of the foundation-stone was a copy of NATURE.

[1] Read by C. R. Cross at a meeting of the American Academy of Arts and Sciences, November 10, 1880.

THE Berlin correspondent of the *Times* states that a movement is afoot among the Germans in the United States for the creation of a native University on the model of those in the old country, to be called the Kaiser Wilhelm Universität, in commemoration of the "glorious resuscitation of the Fatherland." Milwaukee is mentioned as the likeliest candidate among all the cities of the West that aspire to the honour of harbouring this plant of pure Teutonic culture, which would cost, to begin with, about two million dollars. It is not at all probable, however, that the scheme will come to anything.

SCIENTIFIC SERIALS

THE *Journal of Anatomy and Physiology* for April, 1881 (vol. xv. part 3), contains:—On the bones, articulations, and muscles of the rudimentary hind-limb of the Greenland right whale (*Balæna mysticetus*), by Dr. J. Struthers.—On the stridulating apparatus of *Callomystax gagata*, by Prof. A. C. Haddon (Plate 20).—On the sternum as an index of age and sex, by Dr. Thomas Dwight (concludes that the breast-bone is no trustworthy guide either to the sex or the age).—On the mechanism of costal respiration, by Dr. J. M. Hobson (with figures).—On the membrana propria of the mammary gland, by Dr. C. W. M. Moullin (with figures).—On double and treble staining of microscopical specimens, by Dr. W. Stirling.—On the comparative anatomy of the lymphatics of the mammalian urinary bladder, by Drs. George and F. Elizabeth Hoggan (plate 21).—Notes on a dissection of a case of epispadias, and on the morphology of the muscles of the tongue and pharynx, by Dr. R. J. Anderson. —On the so-called movements of pronation and supination in the hind-limb of certain marsupials, by Dr. A. H. Young.—A contribution to the pathological anatomy of pneumonokoniosis (*Chalicosis pulmonum*), by Thos. Harris (plate 22).—On the histology of some of the rarer forms of malignant bone tumours, by Robt. Maguire.—On the morbid histology of the liver in acute yellow atrophy, by Prof. Dreschfeld.—On the relationship between the muscle and its contraction, by Dr. J. Theodore Cash. —Anatomical notes.

The Quarterly Journal of Microscopical Science, April, 1881, contains—On the minute anatomy of the branchiate echinoderms, by P. Herbert Carpenter (plates 11 and 12).—On young stages of Limnocodium and Geryonia (plate 13), and observations and reflections on the appendages and on the nervous system of *Apus cancriformis*, by E. Ray Lankester (plate 20).—On the origin and significance of the metamorphosis of Actinotrocha, by Edmund B. Wilson (plates 14 and 15).—A further contribution to the minute anatomy of the organ of Jacobson in the guinea-pig (plates 16 and 17), and histological notes, by Dr. E. Klein.—On the development of microscopic organisms occurring in the intestinal canal, by Dr. D. Cunningham (plate 18).—Researches upon the development of starch-grains, by A. F. W. Schimper (plate 19), translated from the *Botanische Zeitung*.—On the cause of the striation of voluntary muscular tissue, by Dr. J. B. Haycraft.—On the relation of micro-organisms to disease, by Prof. Lister; with notes and memoranda.

The American Naturalist, April, 1881.—Wm. Trelease, on the fertilisation of *Salvia splendens* by birds. (The fertilisation is apparently effected by a humming-bird.)—Prof. E. D. Cope, on the origin of the foot-structures of the Ungulates.—C. A. White, progress of invertebrate palæontology in the United States for 1880.—Carl F. Gissler, evidences of the effect of chemico-physical influences on the evolution of branchiopod Crustaceans.—Dr. R. W. Schufeldt, notes on a few of the diseases and injuries of birds.—A. S. Packard, jun., the brain of the locust (with three plates).

Bulletin of the United States Geological and Geographical Survey of the Territories, vol. vi. No. 1.—On the vegetation of the Rocky Mountain region and a comparison with that of other parts of the world, by Asa Gray and Joseph D. Hooker (pp. 1, 77).—On some new Batrachia and Reptilia from the Permian beds of Texas; on a wading bird from the Amyzon shales; on the Nimravidæ and Canidæ of the Miocene period; and on the Vertebrata of the Wind River Eocene beds of Wyoming, by E. D. Cope.—The osteology of *Speotyto cunicularia*, var. *hypogæa*, and on the osteology of *Eremophila alpestris*, by Dr. R. W. Schufeldt.—A preliminary list of the North American species of Agrotis, by A. R. Grote.

Revue internationale des Sciences biologiques, March, 1881.—Prof. Strasburger, the history of the actual state of the cell theory.—M. Debierre, physical and biological dynamism.—Prof. Ray Lankester, embryology and classification of animals.

Brain: a Journal of Neurology.—Part 13 for April, 1881, contains, of original articles:—Dr. J. C. Bucknill, on the late Lord Chief Justice (Sir A. Cockburn) of England.—Dr. B. Bramwell, on the differential diagnosis of paralysis.—Dr. A. Flint, jun., on the cause of the movements of ordinary respiration.—Dr. Julius Althaus, on some points in the diagnosis and treatment of brain disease.—Dr. C. S. W. Cobbold, observations on certain optical illusions of motion.—Bevan Lewis, methods of preparing, demonstrating, and examining cerebral structure in health and disease.

Revue des Sciences Naturelles, 2me série, tome 2, No. 4, March, 1881, contains:—M. A. Salvatier, on the mechanism of respiration in the Chelonians (plates 5 and 6).—Dr. E. Jourdan, notes on the anatomy of *Distomum clavatum*, Rnd (plates 7 and 8).—M. A. Villot, another word on the fresh-water Pliocene of the Bas Dauphiné.—M. Collot, provisional study of the Anthracotherium remains from the lignites of Volx.—M. Viguier, note on the lithographic chalks of Nebias.—M. Kieffer, on the herborisations of Strobelberger at Montpellier in 1620 (*finis*).—Scientific review of works published in France on zoology, botany, and geology.

Journal of the Asiatic Society of Bengal, 1880, No. 4 (vol. xlix. Part 2).—W. T. Blanford, contributions to Indian Malacology, No. 12—new land and fresh-water shells from Southern and Western India, Burmah, the Andamans, &c. (plates 2 and 3).—J. Wood Mason and L. de Nicéville, diurnal Lepidoptera from Port Blair, with descriptions of some new or little-known species, and of a new species of Hestia from Burmah (plate 13).—W. T. Blanford, description of an Arvicola (*A. Wynnei*) from the Punjab Himalaya.—Capt. G. F. L. Marshall and L. de Nicéville, new species of Rhopalocerous Lepidoptera from the Indian region.—J. Wood Mason, Parantirrhœa Marshalli, the type of a new genus and species of Rhopalocerous Lepidoptera from South India.

Verhandlungen der k. k. zoologisch-botanischen Gesellschaft in Wien, Bd. xxx., Heft 2, 1881, contains the minutes of proceedings, June to December, 1880, and the following memoirs:—F. Krasan, report in connection with new investigations on the development and origin of the lower organisms (plate 7).—Dr. A. v. Krempelhuber, a new contribution to the lichen flora of Australia.—Th. Beling, the metamorphosis of *Cænomyia ferruginea*, Scop.—Prof. Josef Mik, on the mounting and collecting of Diptera, descriptions of new Diptera, and dipterological notes (plate 17).—H. B. Moschler, contribution to the Lepidopterous fauna of Surinam, No. iii. (plates 8 and 9).—S. Schulzer, mycological contributions.—J. Stussiner, *Leptomastax Simonis*, a new species of subterranean beetle.—Hans Leder, on the Coleopterous fauna of the Caucasus, No. iii., in co-operation with Dr. Eppelsheim and E. Reitter.—D. Hirc, the Molluscan fauna of the Liburnian Karst.—Fritz Wachtl, contribution to our knowledge of the European gall-producing insects (plate 18).—Count E. Keyserling, new American spiders (plate 16).—Dr. Ludwig Lorenz, on *Distomum robustum*, sp. n., from the African elephant (plate 19).—A. von Pelzeln, on a hornless deer.—Dr. F. Löw, on a more exact knowledge of the procreativeness of the sexual individuals in Pemphigus.—Dr. R. Drasche, on a new species of Echiurus from Japan (*E. unicinctus*), and remarks on *Thalessema erythrogrammon*, Leuckart (plate 20).—Dr. R. Bergh, monograph of Polyceridæ (plates 10 to 15).

Gegenbaur's morphologisches Jahrbuch, vol. 17, part 1, contains—Prof. Oscar Herburg, on the exoskeleton of fishes: No. 3, the Pediculati, the Discoboli, the genus Diana, the Centriscidæ, some genera of Triglidæ, and the Plectognathi (plates 1 to 4).—On the duplex nature of the ciliary ganglion, by Prof. W. Krause (plate 5).—On the abdominal muscles of the crocodiles, lizards, and tortoises, by Dr. Hans Gadow (plate 6).—Contributions to the developmental history of Petromyzon, by W. B. Scott (Princeton), (with plates 7 to 11).—On the "pars facialis" of the lachrymal bone, by Prof. Gegenbaur.

Rivista Scientifico-Industriale, No. 6, March 31.—On earthquakes, by Dr. Bassani.—New plant, by S. Fenzi.—Determination of the velocity of sound in chlorine, by Prof. Martini.

Sitzungsberichte der naturforschenden Gesellschaft zu Leipzig, 1879-80.—On double monstrosities in fishes, by Prof. Rauber.—On the finer structure of milk-glands, by the same.—On Aphthæ, by Prof Hennig.—On results of glacier thrust, by Prof. Credner.—On the geological results of a deep boring at the Berlin Railway at Leipzig, by the same.—On the reduction of anatomical forms to equal size, by Prof. Hennig.—On the system of spinal ganglia, by Prof. Rauber.—On chlorophyll, by Dr. Sachsse.—On an optical combination which may be applied as objective of a telescope, by Dr. von Zahn.—On *Lichen bombicinus*, by Prof. Hennig.—On the development of cells to organs of locomotion, by Dr. Simmroth.—On Negrito skeletons from the Philippines in European museums, by Herr Meyer.—On the cycle of forms of some unicellular algæ, by Herr Richter.

Atti della R. Accademia dei Lincei, vol. v. fasc. ix.—On the discharge of condensers, the theory of the electrophorus, and its analogy with condensers, by Prof. Villari.—New observations of Pechüle's comet at the Royal Observatory of the Roman College, by P. Tacchini.—Two solar regions in continuous activity during 1880, by the same.—On the motion of a heterogeneous fluid ellipsoid, by S. Betti.—New method for the volumetric evaluation of molybdenum, by Signors Mauro and Danesi.—On some compounds of the furfuric series, by Signors Ciamician and Dennstedt.—Separation and determination of nitric and nitrous acid, by S. Piccini.—Observations on the method commonly adopted in treatment of like fundamental questions of infinitesimal analysis, by S. Casorati.—On the drainage works of the Roman subsoil, by S. Tommasi Crudeli.

SOCIETIES AND ACADEMIES

London

Zoological Society, May 3.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—Prof. F. Jeffrey Bell, F.Z.S., read the first of a series of papers on the systematic arrangement of the *Asteroidea*. In the present communication the author directed attention to the large number—more than eighty—of described species of the genus *Asterias*, the subdivision of which had never yet been attempted. After a list of the species with reference to one description of each, and a list of the synonyms, he proceeded to describe and make use of certain characters as an aid in the classification of the species; the number of rays, of madreporiform plates, and of ambulacral spines forming the more important, and the form and character of the spines the less important points. The author then proposed a mode of formulating results by the use of certain symbols, and concluded by describing five new species.—A communication was read from Dr. M. Watson, F.Z.S., containing some observations on the anatomy of the generative organs of the spotted hyæna, in continuation of a previous paper on the same subject.—Mr. Oldfield Thomas, F.Z.S., read a memoir on the Indian species of the genus *Mus*. The present paper was an attempt to clear up the existing confusion in the synonymy of the Indian species of this genus, of which the author recognised about nineteen as valid.—A communication was read from Mr. Edgar A. Smith, containing remarks on some specimens of *Cypræa decipiens*, lately received by the British Museum.—A second paper by Mr. Smith contained the description of two new species of shells from Lake Tanganyika.—Capt. G. E. Shelley read a paper containing an account of seven collections of birds recently made by Dr. Kirk in the little explored regions of Eastern Africa. Two new species were proposed to be called *Coccystes albonotatus* and *Urobrachya Zanzibarica*.—Mr. Arthur G. Butler, F.Z.S., read a paper on a collection of Lepidoptera made in Western India, Beloochistan, and Afghanistan by Major Charles Swinhoe. The collection contained examples belonging to three new genera and fifteen new species.

Chemical Society, May 5.—Dr. Roscoe, president, in the chair.—The following papers were read:—On the action of humic acid on atmospheric nitrogen, by E. W. Prevost. The author has repeated some of the experiments of E. Simon (*Land. Vers. Stats*, xviii.) on the above action; he is quite unable to confirm the results of that investigator, and concludes that under ordinary circumstances no formation of ammonia takes place when humic acid and nitrogen are allowed to remain in contact.—On the active and inactive amylamines corresponding to the active and inactive alcohols of fermentation, by R. T. Plimpton. The author has prepared and examined the mono-, di-, and triamylamines and some of their compounds. The active amylamines polarise strongly; their salts do not crystallise

so well as those of the inactive amylamines; there is also some difference in the boiling-points and specific gravities of these two classes of bodies.—On the action of sodium alcoholates on fumaric ethers, by T. Purdie. An acid is formed which is an ethylethermalic acid isomeric with the monethylmalate of Desmondisir. The action of sodium isobutylate on isobutyl fumarate was also studied; an isobutylmalic acid was formed.—On the products of the action of alkalies on ethylic β ethylacetosuccinate, by L. T. Thorne. An ethylsuccinic acid was obtained by the action of strong potash identical with that obtained from the a succinate; with weak potash 5 per cent. a-ethyl-β-aceto-propionic acid was obtained, which on boiling gave off water and formed a body $C_7H_{10}O_2$.—On some carbazol compounds, by C. H. Rennie and W. R. Hodgkinson. The authors have studied the action of potassium carbazol on ethyl chlorocarbonate; a new urethane was obtained.

Geological Society, April 27.—R. Etheridge, F.R.S., president, in the chair.—Samuel Gerrard Kirchhoffer, Arthur Henry Shakspere Lucas, and Lieut. Frederick Thomas Nelson Spratt were elected Fellows of the Society. The following communications were read:—On the precise mode of accumulation and derivation of the Moel Tryfan shelly deposits; on the discovery of similar high-level deposits along the eastern slopes of the Welsh mountains; and on the existence of drift-zones showing probable variations in the rate of submergence, by D. Mackintosh, F.G.S.—On the correlation of the Upper Jurassic rocks of England with those of the Continent, by the Rev. J. F. Blake, M.A., F.G.S. Part I. the Paris basin. This was an attempt to settle the many questions of correlation arising out of the detailed descriptions given of the various localities in the Paris basin where Upper Jurassic rocks are developed, by a consecutive survey of them all; undertaken by the aid of a grant from the "Government Fund for Scientific Research." In previous papers the names used for the great sub-divisions and their boundaries were adopted without material modification; in the present such modifications were proposed as may bring the English and Continental arrangements into harmony. Five distinct areas were considered in this paper:—(1) The southern range; (2) the Charentes; (3) Normandy; (4) the Pays de Bray; (5) the Boulonnais. From this study it was proposed—that the "Lower Calcareous grit" and almost all the Coralline oolite should be placed in the Oxfordian series as the upper division, under the name "Oxford Grit" and "Oxford Oolite"; that the Corallian consists of two parts, the Coral Rag and the Supracoralline beds; that the Kimmeridgian should include the Astartian and Virgulian, the Pterocerian being a subzone; that the "Upper Kimmeridge" and the Hartwell clay, with the "Portland sand," should make a new sub-division to be called Bolonian, the northern and southern types being both represented at Boulogne, which may be divided into upper and lower; and that the true Portland limestone and the Purbeck be united into one group, as Lower and Upper Portlandian; the fact of the latter being freshwater being paralleled by parts of the true Portland having that character.—On fossil chilostomatous Bryozoa from the Yarra-Yarra, Victoria, Australia, by Arthur William Waters, F.G.S.

Anthropological Institute, April 26.—Prof. W. H. Flower, F.R.S., vice-president, in the chair.—Mr. J. E. Price exhibited a collection of bones of man and other animals discovered by himself and Mr. Hilton Price at the Roman villa at Brading, Isle of Wight. The bones had been examined by Prof. Flower, who reported that they were all in much the same state of preservation, and probably all contemporaneous. They consisted of (1) Man: fragments probably of one and the same skeleton. From the condition of the bones it is certain that the individual was adult and probably of middle age and about the average stature. (2) Dog: Numerous remains of at least three individuals, all of nearly the same age and size, not more than half-grown, having only the milk teeth in place. (3) Ox: Young. (4) Horse: One incisor tooth.—Mr. A. L. Lewis read a paper on some archaic structures in Somersetshire and Dorsetshire. The author, in speaking of the great stone circles at Stanton Drew, near Bristol, mentioned the elaborate astronomical theories which had been propounded concerning them by antiquaries of the last century, and said that, while he had no belief in them, he thought that the larger stone circles, of which this group was a specimen, had been used as places for solar worship; there was in nearly all of them some special reference to the north-east, the quarter in which the sun rose on the longest day; in some, however, there were outlying stones towards the south, and this was the case at a circle at Gorwell in Dorsetshire; these stones, whether to the south or the north-east, were evidently so placed for some special object, as the number of instances in which they occurred was too great for their position to be merely accidental. The paper was illustrated by the exhibition of plan, model, and some worked flints, &c., found by the author at some of the monuments mentioned by him.—Mr. G. M. Atkinson read a paper on a new instrument for determining the facial angle. A needle is inserted into each optic foramen, and fixed at a point in the centre of each orbit; the needles are connected by an axle with flat ends which slide on the needles; an index-pointer is attached to the axle in the middle, and is in the same visual horizontal plane as the needles. A bar, carrying a semicircular protractor, is constructed to be affixed at the centre point of the protractor, and to have free movement in a vertical plane alongside the index-pointer. If this bar-protractor be placed in position on the skull so as to touch the ophryon and alveolar points, the number of degrees in the facial angle, by this method, will be indicated by the index-pointer on the protractor.—The Rev. W. S. Caiger read a paper on Thomas of Aquinum and anthropology.

Royal Microscopical Society, April 13.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—A paper by Mr. W. H. Shrubsole and Mr. F. Kitton, on the diatoms discovered by the former in the London clay, was read. Also one by Dr. Anthony, on sliding stage diaphragms.—The other subjects discussed were E. Hallier's view of the cause of the movements of diatoms, the "Society" standard screw, *Amphipleura pellucida* illuminated by the vertical illuminator, and the structure of wood-sections exhibited by Mr. Stewart.—Mr. Powell exhibited an oil-immersion $\frac{1}{8}$-inch objective of the exceptionally large aperture of 1·47 N.A. (1·0 = 180° in air).

EDINBURGH

Royal Society, April 17.—Sir William Thomson, honorary vice-president, in the chair.—Prof. Helmholtz, in an interesting communication on electrolytic conduction, stated that the experiments he was about to describe were a continuation of experiments he had formerly made in connection with certain objections that had been urged against Faraday's law of electrolysis. He had already shown that a feeble galvanic current could be passed through an electromotive preparation of acidulated water, even though the electromotive force was not sufficient to decompose the water. The action of such a current would be, in the first place, to coat the electrodes, the one with hydrogen, the other with oxygen. The hydrogen however speedily combined with the free oxygen in the air and liquid to form water, while the oxygen on the positive electrode as speedily dissipated itself. In this way the polarisation in the electrolytic cell was kept down, so that the original current was never wholly destroyed. In the later experiments Prof. Helmholtz had completely removed the air from the neighbourhood of the electrolyte. This was effected by an ingenious use of the property possessed by palladium of holding large quantities of hydrogen gas in its pores. With this specially-prepared cell he found that a feeble current passed through it fell down to zero in a very short time, the difference of potential due to the polarisation of the electrodes quite balancing the original electromotive force. On throwing off the battery the polarised electrolytic cell showed on a delicate galvanometer a reversed current, which rapidly fell to zero from an intensity equal to that of the original current before polarisation set in. Another result to which his researches had led him was that there were no *chemical* forces acting between the molecules of an electrolyte other than those that existed in virtue of what might be called their electric charges—a result which cannot fail to have an important bearing upon the question of chemical constitution.—Sir William Thomson communicated a short paper on the average pressure due to impulse of vortex-rings on a solid. When a vortex-ring is approaching a plane large in comparison to the dimensions of the ring, the total pressure over the surface is *nil*. When a ring approaches such a surface it begins to expand, so that if we consider a finite portion of the surface the total pressure upon it due to the ring will have a finite value when the ring is close enough. In a closed cylinder any vortex-ring approaching the plane end will expand out along the surface, losing in speed as it so does, until it reaches the cylindrical boundary, along which it will crawl back, on rebounding, to the other end of the cylinder. As it approaches, it will therefore exert upon the plane surface a definite outward pressure, whose time-integral is equal to the original momentum of the vortex, and

a precisely equal pressure as it leaves the surface. Hence, in the case of myriads of vortex-rings bombarding such a plane surface, though no individual vortex-ring leaves the surface immediately after collision, for every vortex-ring that gets entangled in the condensed layer of drawn-out vortex-rings another will get free, so that in the statistics of vortex-impacts the pressure exerted by a gas composed of vortex-atoms is exactly the same as is given by the ordinary kinetic theory, which regards the atoms as hard elastic particles.—Prof. Tait, in a brief paper on the crushing of glass by pressure, indicated certain results he had obtained by experiments, which were in good accord with the mathematical theory of the strains to which a closed cylindrical glass tube under high pressure is subjected. Of the three stresses, radial, tangential, and longitudinal, which may be regarded as acting upon any elementary portion of the wall of the tube, the two former have a *shearing* effect, to which the crushing of the tube is due. From the few experiments that had been completed it appeared that the shear required to disintegrate ordinary lead glass was about $1 \pm \frac{1}{10}$.—Prof. J. Blyth gave an account of experiments which he had made on the cause of the sounds produced in the microphone receiver. He also exhibited another form of telephone, in which the vibrating membrane was attached rigidly to a copper wire dipping into a column of mercury which formed along with the wire part of the circuit. The inductive effect of the current on itself caused the wire and the attached membrane to vibrate in exact correspondence with the variations of the current.

Paris

Academy of Sciences, May 2.—M. Wurtz in the chair.—The following papers were read:—Note on a property of the indicatrix, relative to the mean curvature of convex surfaces, by M. Faye.—On the inverse electromotive force of the voltaic arc, by M. Jamin. With a continuous battery current this inverse force presents a resistance to be first overcome; but with alternately contrary currents from a magneto-machine renewed at least 500 times per second, the current at each inversion profits momentarily by the inverse force called forth during the previous emission. Hence the possibility of lighting several arcs in the same circuit of a machine (and the number increases rapidly with the velocity).—Formation of a marine zoological station in the Eastern Pyrenees, by M. de Lacaze-Duthiers. Some proposed harbour alterations at Port Vendres led the author to look about for another suitable locality. Banyuls-sur-Mer made prompt application, with generous offers of help in the case of being chosen. The Port Vendres authorities were also urgent. At Perpignan the project was cordially received. Thus promises have been made of a capital of 32,000 francs, an annual sum of 750 francs, a site, a boat, and the product of a subscription. The President expressed the satisfaction of the Academy.—The derangements of progression, of station, and of equilibration arising in experiments on the semicircular canals or in maladies of these canals, are not the effects of these, but of the influence they have on the cerebellum; note by M. Bouillaud.—On the inequalities with long periods in the movements of heavenly bodies, by M. Gyldén.—On the stratigraphic series of rocks which form the ground in Upper Auvergne, by M. Fouqué. Apart from unimportant flows of Miocene basalt the series of volcanic rocks there comprises two distinct periods, both commencing with strong projections and eruptions of trachytic and acid Andesitic rocks, and terminating with very basic eruptions, porphyroid basalt and basalt of plateaux.—Examination of some artificial products obtained by James Hall, by MM. Fouqué and Lévy. Living in the end of last century, he seems to have been the first who artificially reproduced an eruptive crystalline rock (viz., whinstone).—On salicylic acid and its applications, by M. Schlumberger. *Inter alia*, it has been given daily to animals in some places for years as a protective against contagious disease. To preserve beer it is introduced twice, the first dose being only sufficient to act on lactic ferments, not yeast; a second dose prevents the alcoholic degenerating into acetic fermentation. The two doses together amount to not more than $\frac{1}{20000}$ or 0·05 gr. per litre. It is estimated that 5,000,000 hectolitres of wine were salicylised in France in 1880.—Observations of the comet *f* 1880 (Pechüle) at Paris Observatory, by M. Bigourdan.—On the principle of conservation of electricity, or second principle of the theory of electric phenomena, by M. Lippmann. The algebraic sum of all the simultaneous variations of charge is always nil. Hence the sum of the quantities of free electricity is invariable, since its total variation is always equal to zero. This law extends to all the phenomena hitherto studied. M. Lippmann translates it into analytical language.—On the protobromide and protoiodide of chromium, and on the oxalate and protoxide of chromium, by M. Moissan.—On the acetylic derivatives of cellulose, by M. Franchimont.—Action of sulphuric acid on acetic anhydride, by the same.—On a reagent fitted to distinguish ptomaines from vegetable alkaloids, by MM. Brouardel and Boutmy. This reagent is ferricyanide of potassium, which, in presence of pure organic bases produced in the laboratory or extracted from a body after alleged poisoning, is not any way modified, but when brought into contact with ptomaines (cadaveric alkalies) is changed at once to ferrocyanide, and then becomes capable of forming prussian blue with salts of iron.—On a combination of iodoform and strychnine, by M. Lextrait.—On some felspars of the valley of Bagnères-de-Luchon (Haute-Garonne), by M. Filhol.—On the physiological and pharmaco-therapic effects of inhalation of oxygen, by M. Hayem. Inhalation of oxygen is a useful auxiliary to treatment of chlorosis with iron. The action is similar to that of hydrotherapy, which stimulates the nutritive movement and the formation of red corpuscles, without sensibly modifying the individual alterations of these elements. The method effectually suppresses vomiting when not caused by organic lesion of the stomach.—On an approaching scientific voyage to the whale fishery of Vadsö, by M. Pouchet. Vadsö is on the east coast of Finmark. A steam advice-boat, *Le Coligny*, has been placed at M. Pouchet's disposal by Government. The marine fauna and flora and the rocks of the Varanger fiord will be studied, and certain questions in the biology of fishes especially.—Migration of the puceron of the poplar (*Pemphigus bursarius*, Lin.), by M. Lichtenstein.—Trichinæ encysted in the intestinal walls of the pig, by M. Chatin.—Study on some points of the anatomy of *Sternaspis scutata*, by M. Rietsch.—On two meteors observed at Nouvion-en Thierache, by M. Baudrin.

Vienna

Imperial Academy of Sciences, May 5.—L. Fitzinger in the chair.—The following papers were read:—F. Steindachner, contributions to the knowledge of the river fishes of South America, Part iii.; ichthyological contributions, Part xi., by the same.—Dr. Karl Richter, contributions to a precise knowledge of cell-membranes of the fungi.—Dr. R. Benedikt and v. Hubl, on dinitro- and trinitroresorcin.—K. Fischer, on the salts of resorcinsulphonic acid.—Prof. H. Durège, on bodies (figures) of four dimensions.—A. Brezina, on the meteor-iron of Bolson de Mapimi.—Dr. T. Domac, on hexylene of mannite.—Prof. Stefan, on the evaporation at a circular or elliptical basin.—T. Holetschek, computation of the orbit of the "Peitho" planet (118), discovered in 1872 by Dr. R. Luther at Düsseldorf.—Dr. H. Seeliger, on the ratio of motion in the asterism of ζ Cancri.

CONTENTS

THURSDAY, MAY 19, 1881

"A BOOK OF THE BEGINNINGS"

A Book of the Beginnings. By Gerald Massey. Two Vols. (London: Williams and Norgate, 1881.)

IN two large volumes Mr. Gerald Massey has collected together all the principal facts known about Egypt, with a view to trace the origin of mankind. Some portions of his theories are undoubtedly correct, especially those which go to prove that the Egyptians are the oldest known historical race, that they are an African people of a peculiar type, and by no means an Asiatic tribe filtered through the Isthmus of Suez, and in course of time building up a Semitic population in Africa; that evidence of their primitive development is to be found in their physical type; for Mr. Massey is a decided evolutionist, and regards man as evolved from some of the anthropoid apes, especially the black races, whose colour he considers marks their animal descent; that flint and stone weapons, principally of the Neolithic period, have been found in Egypt at different points is undoubted; and that the aboriginal inhabitants of the Nile Valley gradually rose to a higher state of civilisation, and that without a foreign predisposing them, is probably true. When however tbe author leaves the realms of ethnology and dashes into philology his results are startling, and his deductions so weird and transcendental that they fail to command acquiescence. It is the rash seizing of any word in any dialect which is totally inadmissible, as from such arbitrary selections any absurdity may be perpetrated.

Still more extraordinary is the separation, arbitrary as it appears, of dissyllable words into syllables, and comparing each syllable with any Egyptian one that will give such a meaning as the inquirer wishes. To such proceedings there are no limits, and some of the results are grotesque. The first requirement in the study of a language is to separate the original from the introduced words, and to apply to each a distinct etymology. In all languages nouns are of uncertain origin, verbs and original inflections, affixes and prefixes are more typical. Such derivations, for example, as butter and butterfly from the Egyptian *Put*, "food," and *Ter*, "entire" or "total," and moth from the Egyptian *Mut*, "death," and cooper from the Egyptian *Kheper*, "a bottle," are too far-fetched to entitle them to the designation of philological deductions. But with all this straining at gnats the number of English words, whether original or derivative, which can be tortured into supposed Egyptian origin, is remarkably small. Objecting, as is imperative, to all such vain delusions, it must be admitted that the author has a full right to oppose that system of comparative philology which has been built up from the Sanskrit, the supposed oldest representation of the Aryan languages, to the utter neglect of the older Egyptian, Sumerian, Babylonian, and Chinese. The stately edifice built upon the sand of Sanscritism already shows signs of subsidence, and will ultimately vanish like the baseless fabric of a vision. For by it not the study of the general laws of speech, but only of a comparatively recent development is exhibited. The weakness of the author is however equally manifest here, as he deals with languages which he does not understand, and institutes comparisons on imperfect data, nor does he seem to be aware of the knowledge recently acquired of a prehistoric Chinese. He is however right when he points out that such a Hebrew, not British, name as Adam is more likely to be derived from *Tem* or *Atem*, the Egyptian word for "creator" and "created" being, than the Sanscrit Adima, proposed by Max Müller, the more so that the Pentateuch abounds in Egyptian words, and Sanscrit philology is vainly and ridiculously applied to it. But in treating of the Egyptian word for cat and its vocative form pussy, although the different forms cited may amuse those interested in the "great cat question," the learning expended is not on an original, but an introduced word. The cat was doubtless an African and Chinese animal unknown to the Greeks till a very late period, not introduced till late into the houses of the Romans, and not seen on Egyptian sculptures as a pet till about 1500 B.C. The immense deal of reading and the fanciful comparisons of the section of the Egyptian names of personages are too daring and startling. No doubt there is a peculiar fascination in playing with words, and if the combinations are neither correct nor harmonious, they are at all events amusing, as to find that the Chinese expression *fieng yue* is the same as the word fiend, after all only the Egyptian *fenti*, and "old Bendy," the English nick-name for the devil. The same remarks may also be applied to the attempts to refer British symbolical customs to Egyptian names, and the identification of the Egyptian deities in the British Isles, although a great deal of reading has been wasted. In the wriggling over the word Tasc on British coins, the well-known abridgment of Tasciovanus, the father of Cunobelinus, or Cymbelin, there is an unusual degree of floundering. It is referred to the Egyptian word *tes* and the English *tas*, a reaper, and this example will give an idea of the manner in which the subject is treated. At some spot in Herefordshire certain services were performed over "old Tom"—not tbe spirit, but as the departed year was called; and this is supposed to be part of the myth or legend of the Egyptian god Atum, or the Creator, Tom in the game of noughts and crosses, and so is Tommy Dodd. The only difficulty is to conceive how such a transformation can have got into any English head, for the word Tom suggests a vulgar familiarity and a contracted form of Thomas; and in the same strain run on the consideration of the types, names, and similar subjects, all on the same plan. From the consideration of Egyptian origins in Britain, a more than doubtful thesis, Mr. Massey however goes into deeper water when he ventures on Egyptian analogies in the Hebrew scriptures, although the subject is by no means novel, and has been mentioned by various Egyptologists, Chabas, De Rouge, Ebers, Brugsch, and others, besides the extensive use of Egyptology made by German theologians. The identification of biblical personages is another of the attempts of the author to grasp at faint analogies with Egyptian words that might possibly be compounded into the Hebrew syllables forming the Hebrew names; the slightest probability is grasped at as if an absolute proof, with the undaunted boldness of a preconceived theory. Such researches may dazzle those unacquainted either with Egyptian or Hebrew, but it is more than doubtful

if such averments will commend themselves either to Egyptologists or Hebraists; they are so transcendental that they do not belong to the domain of pure or comparative philology, but appertain rather to the province of comparative mythology, and the interpretations so liberally accorded of the myths of one nation by the philology of another. They resemble the labours of the school of Bryant, which expended so much learning, obtained such few results, and established no important fact. So with an immense amount of Egyptian reading and learning the real amount of new facts acquired by ingenious comparisons is small, not to say of the most doubtful character. Amongst one of the most startling ideas is that the Arsu, who ruled during the anarchy which preceded the reign of Setnethk or Nekhtset, is no other than Moses. The search for Moses amongst Egyptologists has been most exhaustive, and Prof. Lanth, who also belongs to the imaginative rather than the critical school, has long ago thought that he identified not only the Jewish lawgiver, but all the members of his family, on an Egyptian sepulchral tablet. It is needless to remark that no other Egyptologist recognises in the polytheistic worshipper of Apis the monotheistic leader of Israel.

No doubt many identical verbal roots occur in Egyptian, Assyrian, and Accadian; those of Hebrew and Coptic have already been pointed out and alluded to; still the languages are essentially distinct in their constructions, and belong to different families. The Assyrian may be classed as the oldest form of the Semitic family, at all events the Babylonian must be considered so. Greater difficulty indeed exists about the Accadian, which has been referred with probability by some to the Ugrian family of languages, and with doubtful success by others to the oldest Chinese, as the theory is based on the comparison of few words, some of which are of uncertain meaning, and they cannot be historically traced as the descendants of one another. Some of the Accadian nouns, indeed, resemble the Finnish, but the verbs are totally dissimilar. Many Egyptian words, however, it would appear from the comparative table of Mr. Massey, resemble Accadian, and this may be considered a new departure, and one perfectly legitimate, as the two languages may have started from a common origin; indeed by some linguists the origin of the Semitic has been referred to Africa; but as already clearly pointed out, although certain phases of construction ally the Egyptian with the Semitic languages, there is not the most remote similarity with the Accadian, which is not only of a totally different family from the Semitic, but also the Egyptian or Hamitic tongues. When however Mr. Massey claims to trace Egyptian words in the Maori, he has no doubt been more fascinated by the theory of the Egyptians belonging to a primitive continent subsequently broken into the islands of the Polynesian group than the actual coincidences of the two tongues or the similar words in the two languages. It must always be remembered that, like the Chinese, the Egyptian is a very poor language, and expresses a great variety of ideas by a single monosyllable: no wonder, then, if coincidences occur. The African origin of the Maoris of course demands further consideration. Ethnologically and philologically they were formerly classed as a probable offset of the Malay race, but how Egyptian words passed to them is another question. Some words certainly look like Egyptian; but that is not sufficient, as some Egyptian words resemble those in all other languages.

More in accordance with probability is the hypothesis that Egyptian words may be found in all the African languages, although their structures differ. This has been long recognised as a fact in the Berber, and also in some of the other African stems, but again the great difference of structure and the doubt how and when the Egyptian words were introduced cloud the inquiry in investigating languages that have had no inscriptions or written literature. Yet the old Egyptian must have been a development of one of the old African languages which subsequently became extinct.

Notwithstanding the difference of opinion about the results and the methods by which they have been obtained, great credit is due to Mr. Massey for the ingenuity with which he has endeavoured to build up his theory and, to his mind, discoveries. He has read through all the principal works on the subjects he treats, and his collection of words, legends, and data is enormous. He has produced a work which will be read with pleasure by some, with amazement by others, and incredulity by specialists. He has taken all reasonable care to insure a fair and correct list of words and facts: yet for all that the embroidery of his particoloured threads has produced a weird and grotesque pattern of strange and fantastic conceptions such as might have been planned by elves or fairies to dazzle and bewilder mortal imagination as much as to amuse and delight themselves. It is too warm and rosy for the chill glance of science.

THE SCOTTISH CELTIC REVIEW

The Scottish Celtic Review. No. 1, March, 1881; pp. 80, 8vo. (Glasgow: James Maclehose.)

THIS is a quarterly review of which the first number has just appeared, published by Mr. Maclehose of Glasgow; but the name of the editor is not given, nor of the writers of the articles. The work however is done in a way which shows that there are at least a few persons in the North who feel a deep interest in Celtic philology and the language and literature of the Scotch Highlands. The programme is an excellent one, and embraces among other things the application to the study of Gaelic of those methods of investigation which have been so fruitful in the fields of English and German philology. It is intended also to help, by means of translations, to make English readers better acquainted with Gaelic literature, and to collect for publication all fragments of unwritten literature which still may happen to linger in the Highlands, as well as to afford room for the discussion of questions relating to Gaelic grammar and orthography. This last, it seems to us, is a subject with which the Gaelic scholars of the Highlands trouble themselves a great deal too much. Modern Gaelic orthography, whether in Ireland or in Alban, is simply incorrigible, and had better be left alone for the rest of the natural lives of the surviving dialects. This involves no great inconvenience; for no scholar who wants to understand the history of a Gaelic word ever thinks of being guided by any of the modern spellings which may be in use, but goes back to the Irish of the Middle Ages, or farther still,

to what is technically known as Old Irish. It is some consolation to Englishmen to know that English orthography is not quite the worst in the world, and that Tonald seldom writes, but that when he does he spells more outrageously than the most wayward spelling-book ever known in the land of the Southron.

The philological articles in this review are very well done, and will be found very instructive, and specially adapted for beginners in the study of Celtic; but what we presume would most attract the readers of NATURE in this number is the tale which it contains, published for the first time. It was taken down some years ago in the Island of Tiree, the Terra Ethica of Adamnan's "Life of St. Columba." This is a summary of it:—The King of Ireland's heir was returning from hunting towards the evening, when he was overtaken by a shower, out of which came a big fellow with a fine steed and a marvellously handsome woman. The big fellow challenged the prince to play with him; he did so, and the big fellow was beaten, whereupon the prince took away his lady companion. He met the same big fellow another day and beat him again; according to the woman's advice he asked this time for the steed, which he took away with him home. The woman told him he would be beaten the next time, and how he was to act under his defeat. It happened just as she had told him, the big fellow laying him under charms, that he should have no rest or peace until he discovered how the Tuairisgeul Mor met with his death. He in his turn laid the big fellow under a charm not to leave the spot until he should return from the difficult expedition which was before him, and in which ever so many kings' sons had perished in former times. With the aid of the counsel of the woman he had taken from the big fellow, and with the assistance of her three wonderful brothers, to whom she recommended him, he managed to execute the first part of his business. On his way back on his horse, just as he had ridden through a wide loch and cut it into two, he was met by a youth who made unheard-of offers for the horse; according to previous advice he was to accept none of them, but to give away the horse only for a grey old man the youth had at home. The hero of the tale carries the grey old man on his shoulders and is guided by him, but is always to do the reverse of what he says. Each time this happened the old man would say, "That gives longer life to you and shorter life to me." At last they sat down in a house, and the old man had to relate the tale of his life, which was to yield the prince the information he was in quest of. He said that he was one of the three sons of a king, who were turned into wolves by their stepmother with her mallet of Druidism. They avenged themselves on her by killing her hens, until she got all the sportsmen in the land assembled to destroy them, when they were driven to shelter themselves under a big rock near the sea. There two died, and the surviving one, seeing a ship not far off, swam so near it that the captain ordered him to be picked up. By and by he became a pet of the captain's, who took him home to his wife. Some time afterwards she was confined of a boy, and the midwives, after dressing the baby, went to sleep, while the wolf lay quietly below the bed; ere long he saw a big fist coming in through the roof and snatching the baby away. When the midwives woke they smeared blood on the animal, and laid the blame on it of having devoured the child, in order to clear themselves of neglect. The captain was loath to kill his pet wolf. The same thing happened another year; but the third time the beast watched, and beheld the fist coming in through the roof, when he seized hold of it, and tore it off at the shoulder; however, the other hand seized the child, but the wolf gave chase, and made its way into a little island with a cave in it where he found that the robber was a giant. The baby was under his arm, and the children previously stolen were playing in the cave. The giant being asleep, he got at his throat, and so the Tuairisgeul Mor found his death. After relating how the three children were brought home to their father, the captain, and how he himself recovered his human form, the old man said: "I am not to live any longer; throw me into yonder cauldron." The King of Erin's son now returned to the hill, where the big fellow who used to challenge him to play, lay with his bones by this time bleached by the wind and the rain; but when the prince told him how the Tuairisgeul Mor had been put to death he was gathered together, and rose from the hillock alive and well, while the young prince went home to marry the beautiful maiden who had enabled him to overcome all the difficulties which had met him.

We have read various tales at different times containing similar incidents, but the only one we shall mention here is that of Pwyll, Prince of Dyved, in Lady Charlotte Guest's "Mabinogion," where it is related how he lost his first-born the night he was born; and how another prince of South Wales used to lose the colts of a remarkable mare he had about the same time. At last the latter watched, and cut off the hand that was in the act of seizing a colt through a window; but what we wished to come to was this—the time is specified in the Welsh tale, namely the first day of May every year. Possibly this may suggest to somebody who has made a study of such legends what they really mean; but we abstain from giving any crude theories of our own on the matter.

OUR BOOK SHELF

Zwangsmässige Lichtempfindungen durch Schall und verwandte Erscheinungen auf dem Gebiete der anderen Sinnesempfindungen (Sensations of Light generated by Sound, and related Phenomena in the Sensations of other Organs of Sense). By E. Bleuler and K. Lehmann 8vo, pp. 96. (Leipzig: Fues's Verlag, 1881.)

As the authors (two medical students of Zürich) were conversing on chemistry in the autumn of 1878, Bleuler being asked what was the appearance of *cetones* (substances of which *acetone* or naphtha is the type), got out of the difficulty at once by saying, "They are yellow, because their name contains an *o*." Lehmann, astonished, inquired what such an apparently absurd answer meant, and then found that from childhood Bleuler, on hearing, or even thinking of any vowel or word, immediately saw a colour, and that many of his relatives were in the same condition. Such was the origin of this investigation, and it is remarkable for having been carried on by one who always saw the colours (Bleuler) and one who never saw them (Lehmann). Such appearances of colour generated by sound are here called *photisms*, while sensations of sound generated by colour are termed *phonisms*, and both are called "secondary sensations or perceptions,"

the authors not knowing exactly in which category to place them. The authors have examined 596 persons (383 men and 213 women), and found among them 76 "positive" (that is, capable of seeing photisms), and 520 "negative" (that is, incapable of seeing photisms). This proportion is about 1 to 7. Particulars of the examinations of all are given. The photisms for the same sounds differ much from individual to individual, but remain constant for the same individual, as shown by receiving identical answers to thousands of questions after intervals of more than a year. The photisms are not always distinct or of definite forms, but are projected on to the spot whence the sounds arise. Other senses produce sensations of colour as well as hearing; thus there are taste and smell photisms. There are also emotional photisms. The authors are unable to give any explanation, but they are clear that simple association does not suffice, and they examine a number of suggestions made to them, showing that they do not account for cases observed. They themselves think that the solution of the difficulty is to be sought in the nature of nervous processes, but they do not admit that "secondary sensations" are psychopathological. This little book is full of curious and interesting details evidently connected with Francis Galton's "mental images," and localisation and sometimes colouring of numbers in the mind's eye. The following account of the general conclusions obtained, given on the last page of the book, will show what a curious page of nervous physiology is here opened out.

1. *Bright photisms* are excited by musically high sounds, severe pain, sharply-defined sensations of taste, small forms, pointed forms. *Dark photisms* by the contrary.

2. *Musically high phonisms* are excited by bright light, clear definition, small forms, pointed forms. *Deep phonisms* by the contrary.

3. Photisms with sharply defined forms, small photisms and pointed photisms, are all excited by the sensations of musically high sounds.

4. Red, yellow, and blue are common colours of photisms; violet and green are rare, blue is of medium frequency.

5. Thorough agreement of the separate assertions of different individuals does not occur.

6. Unpleasant primary sensations may excite pleasant secondary sensations, and conversely.

7. Secondary sensations are scarcely more influenced by psychical circumstances than are primary sensations; and they are inalterable.

8. The disposition to have secondary sensations is hereditary.

9. Traces of secondary sensations are widely spread. Well-developed secondary sensations could be established to exist for one in eight persons examined.

10. Secondary sensations are not more frequently met with in psychopathically afflicted persons than in those of a normal condition.

A List of European Birds. By Henry E. Dresser. (London: Published by the Author, 1881.)

THIS "List of European Birds," including all the species found in the Western Palæarctic region, has been very carefully revised by Mr. Dresser, and appears opportunely on the completion of his great work on the "Birds of Europe." It will be most useful as a check list for labelling, or for reference in making exchanges of birds and birds' eggs. The classification is the same as that adopted in the "Birds of Europe," and follows that of Prof. Huxley, which still appears to Mr. Dresser to be the best as yet elaborated. The species are numbered consecutively, in order to facilitate reference. A very few alterations in the nomenclature have been made: 623 species are enumerated, and the list is published at the low price of one shilling.

The Seals and Whales of the British Seas. By Thomas Southwell, F.Z.S. (with Illustrations). (London: Jarrold and Sons, 1881.)

THIS neat little volume, though it adds little if anything to our scientific knowledge of the British seals and whales, will be welcome to many as telling a good deal about these interesting mammals which could only be found after a prolonged search through many of our scientific periodicals. It will form a pleasant addition to sea-side libraries, and, telling what is known about these creatures, it may thus be the means of indicating what is not known about them, and so do something towards advancing knowledge. A good deal of the information in this little volume appeared originally in the pages of *Science Gossip;* it has however not only been carefully revised, but several additional woodcuts have been added. It has also had the supervision of Mr. J. W. Clark and of the late E. R. Alston.

The more advanced student would have liked a short chapter on the literature relating to our British marine mammalia, which perhaps in a future edition might be given, and an analytic key to the species of British cetacea would be a great help to those living in suitable localities who would venture to take up the study of these very interesting but not easily preserved creatures.

A Sequel to the First Six Books of the Elements of Euclid, containing an Easy Introduction to Modern Geometry, with numerous Examples. By John Casey, LL.D., F.R.S. (Dublin University Press Series, 1881.)

THERE are many geometrical results which are not directly formulated or stated in Euclid's Elements, which are yet constantly turning up in the solution of geometrical problems, and it is very desirable to have a handy book of reference, the propositions in which may be cited, so obviating the necessity of a lengthy proof. The "Exercises on Euclid and in Modern Geometry" of Mr. McDowell is a useful book for this purpose, as all the propositions are fully worked out. Dr. Casey, in the course of teaching, has frequently had to contend against the defect above referred to, and had to interrupt the course of the demonstration of an advanced proposition by turning on one side to prove some well-known result, because he could not cite Euclid as an authority for it. This handy little book, which appears to us quite up to the level of the author's reputation as a geometer, is intended to meet this felt want, and paves the way to a deeper study of the modern geometry contained in the exhaustive works by Chasles, Townsend, Mulcaby, and many Continental writers. A great number of classical problems are led up to, and they themselves discussed and established. The size and style of the book fit it for use in the higher forms of our schools, and more advanced students will find it a convenient book for citation.

Accented Four-Figure Logarithms and other Tables for Arithmetical and Trigonometrical Purposes and for Correcting Altitudes and Lunar Distances, with Formulæ and Examples. Arranged and accented by Louis D'A. Jackson. (London: W. H. Allen, 1881.)

MR. JACKSON is an experienced editor and computer of logarithmic tables, having already published "Accented Five-Figure Logarithms," "Pocket Logarithms and other Tables," &c. Different calculations require different degrees of approximations, and the computer learns by experience which kind of tables are best suited for the end he has in view. In his Introduction our author carefully discusses the question, and states to what extent the present tables are efficacious. His system of accentuation appears to be a good one. Certainly it insures a much closer degree of accuracy than is to be got from ordinary four-figure tables. Each logarithm, on its face, shows whether it is in excess or in defect of the true value (obtained by taking a greater number of figures), or equal thereto. The range of error seems to be reduced to a

minimum. The worked-out exercises show that the mode of working is easy of apprehension and leads to correct results. A merit of the book, for frequent use, is that it is handy in form and very clearly printed.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The so-called "Bunsen-Pump"

PROF. ROSCOE, *President* of the Chemical Society of London, in enumerating the works of his friend Prof. Bunsen, says, in NATURE of the 28th ult. ("Scientific Worthies," vol. xxiii. p. 600):—

"To him we are also indebted for the apparatus for accelerating filtration, the 'Bunsen-pump,' together with all its appliances, now employed in every laboratory."

This assertion requires correction. The pump used by Prof. Bunsen for accelerating filtration was invented by me, and not by Prof. Bunsen, as would appear from the use of his name in connection with it. I described the construction of the "WATER-air-pump" distinctly and plainly in the *Journal* of the Chemical Society of London for January, 1865, under the title, "Researches on the Vacuum: I. The Instruments" (not the instrument, as some will read), and I sent a copy of this paper to Prof. Bunsen, inscribed with a suitable allusion to our former relationship as pupil and teacher, during the spring of 1865. Three years later Prof. Bunsen published his paper, "On the Washing of Precipitates," in which he described again my pump, *but unfortunately omitted to quote my paper of* 1865.

The following is a translation of Prof. Bunsen's own words:—

"To create the difference of pressure for filtration one cannot employ any of the air-pumps commonly used, especially not the mercury-air-pump, as the liquids to be filtered contain not unfrequently chlorine, sulphurous acid, sulphuretted hydrogen, and other substances, which would destroy the metallic portions of the apparatus. I therefore employ a water-air-pump constructed of glass on the principle of Sprengel's mercury-air-pump, which for all chemical purposes is, as I believe, preferable to every other apparatus for air rarefaction, where it suffices to push the rarefaction no further than to a pressure of mercury from 6 to 12 millimetres" (*Ann. Chem. Pharm.*, 1868, vol. cxlviii. p. 277).

The peculiar stress laid here on the uselessness of mercury-air-pumps, and on the fact that chlorine attacks mercury, combined with the omission of all reference to my paper, where both *water and glass* are mentioned, gave to Prof. Bunsen's description of the instrument a colour of originality which Prof. Roscoe (and with him many others) thinks right to support and to perpetuate by calling it the "Bunsen-pump."

As this misnomer has been already the subject of a disclaimer from Prof. Bunsen (NATURE, vol. vii. p. 241), of remonstrances both from myself (vol. vii. p. 241), from Prof. Frankland (vol. xiv. p. 74), and from others, I am sorry to see that Prof. Roscoe should continue to use this designation, which is intended to honour an "*employer*" of the instrument, which hurts the feelings of its inventor and deprives him of his only reward—the satisfaction of being credited with having placed a useful servant at the disposal of science and industry.

If any other inventor less eminent than Prof. Bunsen had made the omission which I have pointed out with much reluctance, no one would persist in giving his name to my child, nor (reversing the case) would anybody speak of a pump as "Sprengel's pump," if I had received from Prof. Bunsen the paper of 1865 and said in 1868, "I therefore employ a water-air-pump constructed of glass on the principle of Bunsen's mercury-air-pump." H. SPRENGEL

Savile Club, London, May 7

[I have read the foregoing note of expostulation from Dr. Sprengel, and I regret that I have hurt his susceptibilities. That Dr. Sprengel first enunciated the principle both of the water- and of the mercury-air-pump no one can doubt. But that Bunsen devised a water-pump suitable for filtration there can be as little doubt. Hence in speaking of a "filter pump"—as every chemist knew I was doing—as contradistinguished from an "air-pump," I conceive that I am justified in using the words "Bunsen-pump."—H. E. ROSCOE.]

Tide-Predicting Machines

THE recent discussions respecting tide-predicting machines have called to mind a very old invention of my own, which, although originally designed for a different purpose, seems to me capable of solving the required problems with all attainable accuracy.

I communicated to the British Association at Cambridge in 1845 "A description of a Machine for finding the Numerical roots of Equations and Tracing a variety of useful Curves." An abstract of that paper may be found at pages 3, 4, of the *Transactions* of the sections. About the same time I lithographed for private distribution a more detailed account of the proposed machine, illustrated by diagrams. It begins with the remark that "Persons engaged in testing theory by experiment have frequently derived great assistance from mechanical contrivances, which give rapid and near approximations without the trouble, in every separate case, of going through tedious multiplications and additions. The proposed machine would be capable of giving values of $\Sigma\{b \cos (n\theta + a)\}$, or of tracing the curve $\rho = \Sigma\{b \cos (n\theta + a)\}$."

At page 2 it is shown how it was proposed to trace the curve $\rho = a + b \cos (n\theta + a)$. It is then remarked that, in the same way, it would be possible to trace the curve $\rho = a + b \cos (n\theta + a) + b_1 \cos (n_1\theta + a_1) + b_2 \cos (n_2\theta + a_2)$, &c. Then follow a variety of suggestions for the practical use of the instrument, and at page 7 there are the following suggestions for the construction of a machine:—

"As toothed wheels cannot be employed to turn the circles (A_1), (A_2), &c., I have made use of a combination of the endless screw and toothed wheels so that the error of the wheels is almost destroyed. *H* (Fig.) represents a handle attached to an axis on which are mounted toothed wheels $t_1\ t_2\ t_3$ . . . which gear with the wheels $T_1\ T_2$, &c., mounted on separate axes, each having a portion of a very accurate screw. These act on the circumferences of the circles (A_1) (A_2), &c., and cause them to revolve uniformly, as in Ramsden's dividing engine, &c." The large diagram shows four of these (A) circles, each of which gives one term, $b \cos (n\theta + a)$, and these terms are summed by the help of a chain, such as is used to wind up watches, passing over pulleys carried by frames free to oscillate in parallel directions. I inclose copies of the lithographed description of the instrument.

May 9 F. BASHFORTH

Sound of the Aurora

IN NATURE, vol. xxiii. p. 484, one of your correspondents speaks of the sound of the aurora as "crackling," or as that of "the flickering of blazing fire," while another describes it as like the "rustling or switching of silk." On Monday, April 12 last, there was an electric storm here, and at 7 p.m. when I walked home (the blazing lightning leaving but *momentary intervals* of darkness), I heard all round me the constant crackling or rustling of blazing flames. Towards the north-west across a low arc near the horizon pale sheet lightning swayed quickly to and fro. There was no rain at the time, that came heavily afterwards. The sound of flames was close round me, and others had the same experience. No one I can find has ever seen lightning so completely fill the air or heard such strange sounds. F. C. CONSTABLE

Karachi, April 25

Meteorological Bibliography

I AM compiling a classified bibliography of meteorological science, and being desirous of rendering it as full as possible, I should feel much obliged if you would intimate to meteorologists that by sending copies of their papers to me they would do much towards helping on the work. The publication of this bibliography has already commenced in "The Scientific Roll."

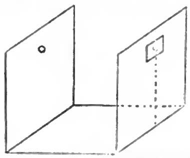
6, Kent Gardens, Ealing, W. A. RAMSAY

An Optical Illusion

THERE is an exquisite optical phenomenon of which I (and doubtless many others too) would be glad to see a really scientific

and physiological explanation. Cannot some one of your numerous scientific contributors favour us with one?

It is this: Take a slip of thin card about three inches long and one wide. Bend up the two end inches at right angles. Perforate one of these vertical ends with a pin-hole, and the other directly and concentrically opposite with a square hole



Observe.—The pin may be placed horizontally, or in any position, with the same result.

about one-eighth of an inch square. Place a small pin in this end, so that the head may be nearly in the middle of the square hole. Now apply this end to your eye and look through both holes at the sky, and you will see the pin apparently beyond the round hole and reversed in position! WILLIAM WILSON

Eirene, Chester, May 4

[This very pretty experiment seems novel. The explanation is, of course, excessively simple. What is seen is the *shadow* of the pin, thrown on the retina by the light diverging from the small hole. As the shadow is erect on the retina, it produces vision of an inverted pin. This appears to be situated about the distance of most distinct vision (ten inches or so), and therefore behind the hole.—ED.]

LAURENCE HARGRAVE, Sydney, should refer to the letter of the Rev. R. Abbay on the "Rayons de Crépuscule" in vol. xviii. p. 329, and the articles and correspondence therein referred to.

SCIENCE IN CHINA[1]

II.

IT is a generally received opinion that the Chinese language presents extraordinary difficulties, both in its acquisition by Europeans and in its use for the expression of the more exalted ideas of Western learning. The attempt to translate modern scientific or technical books into a language so ancient, so crude, and so unchanging is regarded by many who have not given careful attention to the matter as almost absurd. It is readily granted by them that such subjects as the doctrines of Christianity or affairs of a political nature might be expressed easily in the language of a people among whom religion and diplomacy have for ages been carried to a considerable state of advancement. But from the almost total absence of native scientific literature and pursuits there is necessarily a paucity of scientific terms, and this appears at first sight to form an almost impassable barrier to the use of Chinese for scientific purposes. A little investigation however will show that this opinion is without foundation; and that from the time the early Jesuit missionaries commenced their compilations up to the present day no serious difficulties have been experienced by foreign translators.

The question of nomenclature, however, is one that naturally has to be met at the outset. If it were necessary to use only such terms as are to be found in standard Chinese dictionaries, or if it were forbidden to give any new shades of meaning to existing characters the task of translation could never be accomplished. But it must be borne in mind that the Chinese, like other languages, is capable of growth. The increasing intercourse of China with Western nations is undoubtedly making vast additions to the number of words in current use. The Emperor, it is true, has the power of deciding the exact manner in which characters are to be written, and in various instances certain characters have been forbidden to be written in certain ways; but he is powerless to check the changes and additions that are now fast taking place in the language all over the Empire. Where it has become necessary to express a new idea, or to give a name to a new object in Chinese, there has always been found a way of managing the matter more or less satisfactorily; and hence some very clumsy specimens of nomenclature are gradually becoming current, especially among such natives as have much intercourse with foreigners. Of course all such new terms have to stand or fall on their own merits, and if radically wrong or misleading they are pretty certain eventually to be supplanted by better ones. This is merely what naturally happens in the growth of all languages, and although many inconveniences necessarily occur when terms have to be changed, yet there seems to be no help for it. It will be remembered that when the English language began to borrow largely from Greek and Latin, many scientific and technical terms were coined which have since fallen into disuse or been supplanted by others. So it must necessarily be in Chinese with regard to the words borrowed from the English or other languages.

It ought, however, to be possible for the pioneers of modern sciences and arts in China, by exercising great care and by working in harmony, to establish such a system of nomenclature that no very extensive alterations need be made in future years. A Chinaman of ordinary intelligence ought to be able to take up the translation of a work on such a subject as chemistry, for instance, and understand the nomenclature quite as well as a European of similar capacity, and, knowing nothing of chemistry, would understand the original when placed in his hands for the first time. Every new term being explained or defined only when first used, it would of course be useless for an ordinary Chinaman to begin in the middle of such a work and expect to understand everything he read. And yet not only Chinese but foreigners have been known to treat the translations published at the Arsenal in this way. Finding the nomenclature unintelligible to themselves or their Chinese friends, or their teachers or writers, they have condemned all such attempts to express the higher branches of Western learning in Chinese as useless, and have come to the conclusion that the study of European languages is the only way in which satisfactory progress will ever be made in China.

Before commencing the work of the Translation Department it was seen to be necessary to establish a system by which the nomenclature to be employed should be determined. After considerable discussion the following plan was agreed upon by those who organised the department:—

1. *Existing Nomenclature.*—Where it is probable a term exists in Chinese, though not to be found in dictionaries—

a. To search in the principal native works on the arts and sciences, as well as those by the Jesuit missionaries and recent Protestant missionaries;

b. To inquire of such Chinese merchants, manufacturers, mechanics, &c., &c., as would be likely to have the term in current use.

2. *Coining of New Terms.*—Where it becomes necessary to invent a new term, there is a choice of three methods:—

a. Make a new character, the sound of which can easily be known from the phonetic portion, or use an existing but uncommon character giving it a new meaning.

b. Invent a descriptive term, using as few characters as possible.

c. Phoneticise the foreign term, using the sounds of the

[1] By Mr. John Fryer, Chief Translator to the Chinese Arsenal. Continued from p. 11.

Mandarin dialect, and always endeavouring to employ the same character for the same sound as far as possible, giving preference to characters most used by previous translators or compilers.

All such invented terms to be regarded merely as provisional and to be discarded if previously existing ones are discovered or better ones can be obtained before the works are published.

3. *Construction of a General Vocabulary of Terms and List of Proper Names.*—During the translation of every book it is necessary that a list of all unusual terms or proper names employed should be carefully kept. These various lists should be gradually collected and formed into a complete volume for general use, as well as with a view to publication.

Unfortunately the above plan has not been thoroughly or consistently carried out, and hence there exists a certain amount of confusion in the works of the different translators already published, and which can only be partially rectified in future editions. This is greatly to be regretted, because the labour that would have been involved would have been trifling compared with the great advantages to be derived. It is to be hoped that the Chinese as well as the foreign members of the department will in time appreciate the necessity of using the same terms invariably throughout the whole series of publications. It is manifest that the practical utility of each one's work depends greatly upon the extent to which the above rules have been observed.

Next to nomenclature it may be well to consider the selection and arrangement of the various works compiled or translated at the Kiangnan Arsenal. The original idea was, as before stated, to prepare an encyclopædia that should bear some resemblance to the "Encyclopædia Britannica." It was soon found, however, that many of the treatises in the eighth edition of that valuable work were too elementary and too far behind the time. It became necessary, therefore, to translate from more modern and complete publications. Various high officials asked to have books translated for them on special subjects. Several treatises not considered sufficiently complete had to be supplemented by larger ones, and hence the idea of an encyclopædia has gradually been almost lost; while a miscellaneous collection of translations and compilations has been the result, and the range of subjects is comparatively limited. In most cases each translator or Chinese writer seems merely to have selected such subjects as suited him best, without regard to the symmetry or harmony of the entire collection. There are thus several important subjects, such as natural history, biography, &c., not yet noticed, while there are various treatises on others of comparatively little importance. As might be expected, military and naval science is one of the subjects that has received a large share of attention.

The general defect about most of the publications is that they are far too elaborate and profound, and consequently can only be understood by a few, while the masses can never master them. It was to remedy this defect that the *Chinese Scientific Magazine* was commenced, although it has no direct connection with this department. Recently some of the English "Science Primers" have been translated by Dr. Allen, and will no doubt help to supply the want. The "School and Textbook Series," however, will probably be the chief means by which a lower and an intermediate grade of books on scientific subjects will be furnished; and although the series owes its origin to the Missionary Conference held in Shanghai in 1877, it has the two European members of the Translation Department on its managing committee. To some extent this series, when completed, will therefore be supplementary to the publications from the Kiangnan Arsenal, and it is arranged that a part, at least, of the series shall be printed at that place.

Next as to the manner in which the work of translation or compilation is carried on. The foreign translator, having first mastered his subject, sits down with the Chinese writer and dictates to him sentence by sentence, consulting with him whenever a difficulty arises as to the way the ideas ought to be expressed in Chinese, or explaining to him any point that happens to be beyond his comprehension. The manuscript is then revised by the Chinese writer, and any errors in style, &c., are corrected by him. In a few cases the translations have been carefully gone over again with the foreign translator, but in most instances such an amount of trouble has been avoided by the native writers, who, as a rule, are able to detect errors of any importance themselves, and who, it must be acknowledged, take great pains to make the style as clear and the information as accurate as possible. A fair copy having been made, the work is placed in the hands of the foreman of the printing department, who causes it to be written out on sheets of thin transparent paper in the large bold book-characters of the "Sung" pattern, and pasted on blocks ready for the engraver. All illustrations, diagrams, &c., are drawn on the same paper by an experienced draughtsman, and cut at the same time and on the same kind of blocks as the characters with which they are interspersed, as in foreign books. In case of steel engravings, such as those accompanying the last edition of Herschel's "Outlines of Astronomy," translated by Mr. Wylie, the illustrations have been printed in England from the original plates. The various charts have been printed from copper plates engraved at the Arsenal.

It may seem strange that with such facilities for printing in Chinese by metal type as exist in Shanghai, and with a complete fount of such type as well as a good cylinder press on the premises, these books are nevertheless cut on wooden blocks and printed by hand, in the old-fashioned way that existed in China for so many ages before printing was known in Europe. The fact is, however, that as a matter of economy and convenience the old system is preferable. The blocks are all of the same size, about eight inches by twelve inches, and about half an inch thick. Each block represents two leaves or four pages of the book, being engraved on both sides. The blocks for a complete work can thus be stowed away in a very small compass. The cost of engraving a page of these wooden blocks is said to be but little more than the expense of setting up a page of Chinese type and preparing it for the press. An edition of one copy can be printed if no more are required, and thus the expense of keeping a large stock of printed books on hand, some of which might eventually have to be sold as waste paper when they grew out of date or revisions had to be made, as is the case among ourselves, is entirely avoided. Any errors or misprints that may be discovered can as a rule be corrected on the blocks with but very little trouble. A skilful printer can print by hand five thousand leaves of two pages each in a day, using no press or machinery whatever. He supplies his own tools and receives as wages about twenty-five dollar cents a day. The paper ordinarily used is white and of the best quality, although a yellowish kind is also made use of at a reduction of 20 per cent. on the selling price. The books are bound in the usual Chinese style and fastened with white silk thread. They present an appearance which satisfies the taste of the most fastidious native.

To those who regard the Chinese language as incapable of expressing modern Western learning, and who consider European languages to be the only medium by means of which the Chinese can become proficient in the Arts and Sciences, the establishment of the Translation Department necessarily appears to be a useless waste of time and money. To those again who grant the possibility of carrying on the work, but suppose that English is destined to become the universal language at no very distant

period, or even before the close of the present century, this attempt to supply Chinese literature with the results of modern discoveries in science and art must of course appear to be conferring a mere transient advantage.

But it is no difficult matter to see that the translation and publication of books in Chinese as it is carried on at the Kiangnan Arsenal and other places is the great means by which the intellectual stagnation of China is to be broken up. This work must necessarily go on and increase rapidly now that a current of thought has begun to set in. As long as foreigners have any knowledge to impart that is of real advantage to the Chinese, so long will the Chinese make efforts to obtain it; for the more the celestial mind drinks at this fountain the greater will become its thirst for further supplies.

The fact that this Translation Department has been established and kept up so long by the Government argues well for the future prospects of China, as it shows that whatever may be the national pride in her antiquated literature, or whatever may be her attitude towards the diplomatists of foreign powers, or the missionaries of foreign religions, she recognises the fact that knowledge is confined to no nation or country. She is therefore willing to be taught even by the "foreign barbarians" such useful things as she feels she is ignorant of. But she must do this of her own accord and in her own way, or not at all. It is a matter in which she is not to be dictated to, as in the case of treaties or missionary rights. She has freely availed herself of what she has considered beneficial, and has not been sparing in funds to enable knowledge to be disseminated throughout the Empire. This willingness to be taught and to pay for being taught is one of the most hopeful features that has occurred in her intercourse with foreign countries, and is deserving of the highest commendation.

The work at the Translation Department is at present only in its infancy; but enough has been done to establish a foundation upon which a large and important structure will eventually arise. Having been commenced and carried on only in obedience to a natural and instinctive desire for knowledge, it ought to go on harmoniously with the course of events, and prove a powerful lever in the regeneration of China. The large number of copies of works already sold at cost price without any attempt to bring them before the public notice evinces the appreciation of the masses of the people; for a Chinaman is very slow in parting with his dollars for what he does not value or admire, or derive benefit from in some way or other. It would be strange if the knowledge that has only been acquired by such vast expenditure of thought and labour by Western nations did not make its value felt among the Chinese; for by means of these books they can in some things place themselves on a level with foreigners without going through the difficulties attending discoverers and inventors.

It is gratifying to find that some of these translations have already found their way as text-books in the Peking University and in higher kinds of mission schools. For example, the work on Trigonometry has been used to advantage in Mr. Mateer's school at Tangchow, in the Shantung province.

Another cheering feature in connection with the Translation Department is, that it is not the only undertaking of the kind in China. Even before it had fairly commenced, Dr. Martin, the learned president of the Peking University, had begun to publish works on natural science and international law. Both he and his fellow-helpers have since published various works on scientific and diplomatic subjects, which have become very popular and have proved of great utility to the Government. Their translations are of a high standard, and are conducted in a style which renders them acceptable to literary men and officials of the highest grade. It is to be regretted that no detailed account of this important work that has been carried on in Peking for so many years appears to have yet been given to the public. Various other Protestant missionaries have done a great service to the country by their long list of published translations in Chinese. The names of at least half a dozen of them will be handed down to future generations as the foremost pioneers of the spread of Western arts and sciences in the "Flowery Land."

The establishment of Chinese legations at the courts of all the great treaty powers, and the creation of Chinese professorships at the Oxford, London, Paris, and Harvard Universities, are events which show the increasing importance of the study of the Chinese language. It is therefore not in vain to hope that in foreign countries, as well as in China, the work of supplying useful knowledge to the Chinese by means of their own language will eventually be carried on to an extent which may bear some reasonable proportion to the size and needs of the "Middle kingdom."

One of the latest and most promising of the schemes which have a similar object in view to the Translation Department is that previously alluded to, namely, the supplying a series of text-books chiefly for use in mission schools, but still of a character suitable to the wants of the nation at large. The practical working of this scheme was placed in the hands of a committee of six gentlemen, all of whom have already had considerable experience in this kind of labour. A series of fifty-five works has been determined on, which embraces several books of an elementary kind that will, no doubt, do well as introductions to the more elaborate treatises on similar subjects already in existence.

It is, however, to the future that we must look for the chief part of the practical utility of all this translation work. Such a vast nation as the Chinese is not to be started into motion and made to follow in the wake of Western civilisation all at once. Generation after generation will have to come and go before the complete transformation will be effected, and the intellectual as well as the physical resources of the country will be turned to the best account. Hereditary tendencies in a wrong direction are not to be eradicated without a long series of struggles. The system of ignoring everything but the "Four Books" and the "Five Classics" at the Government examinations, which are the passports to the highest offices in the State, is not destined to last for ever. By patiently working, on even the present generation of foreigners engaged in this laborious task of spreading intellectual light may hope to see much good resulting from their efforts. If they do not live to see Western learning occupying the position it ought to do in Government examinations they may yet see it holding a prominent place.

The work of translating and compiling scientific books is for the time being perhaps about as dull and unthankful a task as any foreigner could engage in, especially in such a secluded place as the Kiangnan Arsenal, and under the depressing influences of the climate of this part of China. Nothing but a strong sense of duty and a firm belief that this kind of labour is one of the most effective means, under the Divine guidance, for bringing about the intellectual and moral regeneration of this great country, has sufficed to render endurable the long and weary years and weary hours of close and continuous application which it has involved.

4. *List of Books and Statistics.*—The Translation Department, although established in the year 1868, did not commence the publication of books till the year 1871, when a treatise on Practical Geometry and another on Coal and Coal Mining made their appearance. Up to the present year the number of works published amounts to ninety-eight. These works are contained in 236 volumes, a Chinese volume generally consisting of 120 to 200 pages and representing perhaps on an average about

the same number of pages of an ordinary English work in octavo, with small pica type. Of course the style of the original as well as the style of translation may be such as to render this approximate number rather wide of the mark.

The number of copies of works sold up to the end of June, 1879, amounts to 31,111, representing 83,454 volumes. The number of maps and charts published amounts to twenty-seven sheets. Most of them are adaptations of the charts of the British Admiralty, and were printed from copper plates engraved at the Kiangnan Arsenal. The number already sold is 4,774 sheets.

The sum realised by the sale of books and charts may be estimated at about 17,500 dollars, or roughly 3,500*l*.

The numbers sold up to the present time, though considerable, are nothing compared with what might have been expected among such an extensive population. But with no regular means of communication, no postal or railway arrangements, no agencies, and no advertisements or other means of bringing them into general notice or distributing them, it is easy to understand why more have not already been disposed of.

The various periodicals, such as summaries of foreign news, political essays, &c., are not reckoned in the above numbers. From three to five hundred copies of these books are published and distributed gratuitously to various officials both in the vicinity of Shanghai and in distant provinces.

Forty-four works, representing about 142 volumes, have been translated, and are in various stages of preparation, but the publication is not yet commenced.

Thirteen works which are now in the course of translation and of which thirty-one volumes are already completed.

Forty-three books are to be published by the Committee of the "School and Text-book Series." Most of these works are nearly ready to be placed in the printer's hands.

Various treatises on scientific subjects have been published by Protestant missionaries; but about them it has been impossible to obtain statistics as to the numbers printed and sold.

The following list will give some idea of the number and class of scientific works that have been translated:—

Subjects.	Published.		Translated.		In course of translation.	
	works	vols.	works	vols.	works	vols.
Mathematics, Surveying, &c. ...	22	52	2	8	3	5
Engineering, &c.	7	17	3	6	1	2
Chemistry, &c.	5	19	1	1	1	4
Geography, &c....	8	12	—		—	
,, ,, ...	27 maps		—		2	9
Geology, Mining, &c.	5	20	—		—	
Astronomy, Navigation, &c.	9	27	3	4	—	
Physical Science	6	14	4	5	1	1
Medicine	2	8	1	6	2	10
Arts and Manufactures	13	15	9	26	—	
Naval and Military Science	15	41	9	26	2	2
Chronology, News, Periodicals, &c.	6	10	1	1	—	
Naval Architecture	—		3	13	1	1
History	—		5	18	—	
International Law	—		2	26	—	
Miscellaneous	—		2	2	—	
	98	235	45	142	13	34

THE WILLUGHBY SOCIETY[1]

IT was a happy thought to found an association under the name of Francis Willughby, having for its object the reprinting of scarce ornithological works, thus keeping the name of the writer of "Ornithologiæ Libri Tres" in remembrance and doing a service to the working ornithologist. It is nearly nine years over two centuries since Willughby died (July 3, 1672). About seven years younger than John Ray, he studied at Trinity College, Cambridge, under Ray; but though at first the pupil, he was soon the friend and afterwards the patron of our great English botanist. Belonging to a family of wealth and influence, Willughby soon married (1668), and settled at Middleton Hall, Warwickshire. How hard he must have worked the materials for his great work left at the time of his untimely death amply prove. His second son (the elder died) was created a peer by Queen Anne (Viscount Middleton). An annuity was left to Ray, who edited "The Ornithology," which was printed in London (1676) at the expense of Willughby's widow. Willughby has been called the "father of systematic zoology in this country." The new Willughby Society seems determined to follow in his footsteps.

The reprint in fac-simile of M. Desfontaines's "Mémoire" will be no doubt welcomed by the members, and it is only by members that these reprints can be obtained. Honoured by botanists in the beautiful genus Desfontainea, this account of the birds met by him at Barbary is very rare; and we agree with Prof. Newton that few papers are less accessible to ornithologists than those published by the late Sir A. Smith in the *South African Quarterly Journal.* We trust the Willughby Society will meet with the support it deserves from the members of *Ibis* and from bird-lovers in general.

[1] Desfontaines's "Mémoire sur quelques Nouvelles Espèces d'Oiseaux des Côtes de Barbarie" (1787). II. "Ornithological Papers," by Sir Andrew Smith (1830–34).

ZOOLOGY OF THE DUTCH ARCTIC EXPEDITION[1]

A SUPPLEMENTARY number of the *Niederländisches Archiv für Zoologie* just issued is composed of an instalment of five papers describing certain of the animals collected or dredged during the two Arctic voyages of the schooner *William Barents,* together with a list of all the places dredged at, and a map with these and the track marked on it. The ship visited the north of Spitzbergen and the west coast of Novaia Zemlia, and stretched northwards thence almost to Franz-Josef Land. All the dredgings, except two off the north coast of Spitzbergen, were made in the Barents Sea, between Novaia Zemlia and the north of Norway and Bear Island. Dr. R. Horst reports on the Annelids. He found no new species amongst the fifty-one obtained in the Barents Sea. Hjalmar Theel found in the Kara Sea, on the east side of Novaia Zemlia, ninety species. There can be little doubt that the fauna of the two seas, which join in several places, must be nearly identical, yet amongst the thirty-one species from the Barents Sea are fourteen not yet collected in the Kara Sea. The Annelid collection seems to have been rather a meagre one, and must not be taken as representative. The Pycnogonids are described by Dr. P. P. C. Hoek. Examples of these were obtained on fourteen out of the entire thirty dredgings made. They are of eight species, one of which is new. Amongst them is one species of the genus Colossendeis, numerous forms of which were obtained by the *Challenger* in southern latitudes, some attaining there gigantic proportions. The Lamellibranchiata are described by Dr. van Haren Noman, who appends to his paper an important memoir, illustrated by three plates, on the anatomy of the eyes, gills, and other parts of *Pecten Grœnlandicus* and other forms; Dr. A. A. W. Hubrecht contributes a list of the fishes; and Dr. F. A. Jentink a few notes on the field-mouse of Novaia Zemlia, *Curriculus torquatus,* which, unlike all of its allies, turns white in winter. The animal ranges over the whole of Arctic America, Europe, and Asia, and in late geological periods extended as far south as England, Germany, and the basin of the Loire.

[1] "Zoological Results of the two *William Barents* Arctic Expeditions in 1878 and 1879."

DR. HOLUB'S AFRICAN TRAVELS[1]

II.

DR. HOLUB'S third and longest expedition was commenced in March, 1875, and with an account of it the second volume opens. He now proposed to explore Southern Central Africa, and having acquired a great deal of experience during his two previous journeys, was justly in great hopes of success. The route this time selected was first to the Molapo River. As usual great herds of game were from time to time met with, wherever the bush cover was good; then on to his old quarters at Shoshong, where a few days for rest were spent; from Shoshong he journeyed to the great salt-lakes. Elands were now met with, and furnished many a hearty meal. The first salt lake was met on the morning of April the 18th. Away to the west it extended as far as the eye could see, and it took two hours to travel the length of its eastern coast. There was a uniform depth of barely two feet, and it presented a light grey surface edged with stiff arrow-grass and surrounded by dense bush-forest, whilst around about it, in the very thickest of the grass, were considerable numbers of miniature salt-pans; indeed every depression in the soil contained salt. The evaporation appeared to be most rapid. This salt-lake was called Tsitane, the same name being also given to the adjoining river. Here the first Baobab tree was seen; it was a fine specimen, some twenty-five feet in height and nearly fifty-two feet in circumference. Another larger and deeper lake was called by the natives Karri-karri. Here baobabs abounded. The third of the great salt-lakes, called Soa, is the largest; it extends westward beyond Lake N'gami; it is also very shallow, being only four feet in depth. Travelling on to the banks of the Nata and to Tamasetze with the object of getting to the Zambesi before the middle of the month, he encountered one of Mr. Anderson's servants called Saul. He was out on an ostrich hunt, and though an uncommonly bad shot, managed in the following manner to get more than his fair share of birds and eggs:—"I always," he told Dr. Holub, "take a man with me, and we look about till we discover a nest, and then we dig a hole pretty close to it in which we hide. The birds come to sit, and it doesn't want a very good shot to knock over an ostrich when it is just at hand. Well, having made sure of one bird, we stick up its skin on a pole near the nest, and except we are seen, and so scare the birds away, a second ostrich is soon decoyed, and I get another chance." Such "hunting" as this is very likely to destroy the flocks of ostriches in the country around the Klamaklenyana Springs. The country of the Madenassanas was now entered. These people would seem to be serfs to the Bamangwatos. They are a fierce race, tall, and strongly built, the men generally with repulsive countenances, though occasionally some of the women were even nice looking. Their skin is almost black, and their stiff woolly hair hangs down for more than an inch over their temples, while it is either quite short or is kept quite short over the rest of the skull. Many elephant-hunting parties were met with. One trader had in his two waggons not less than 7000 lbs. of ivory, procured mostly in the district between the Victoria Falls and the mouth of the Chobe. A very short *détour* off the beaten waggon-track revealed herds of buffaloes, striped gnus, Zulu hartebeests, and zebras, or showed evident tracks of these and lions. Great trees with trunks of sixty feet in height were also met with, and a great many orchids with *red* blossoms. What a pity that Dr. Holub did not bring home some of these! Passing over an account of a rather exciting lion-hunt, in which both lion and lioness got decidedly the better of it, the Jamasetze wood was left on July 20. The author was much struck by the peculiar way in which some of the leguminous trees shed their seeds, the heat of the sun causing the pods to burst with a loud explosion and to cast the seeds to a considerable distance all about. The air near this wood was full of myriads of tiny bees that crept into one's clothes, hair, and ears, making even one's nose tingle with great discomfort. About August the 10th the watershed of the Zambesi district was reached, and, gazing down into the valleys of the Chobe and the Zambesi, the author saw the realisation of some of the dreams of his youth. At Impalera, the Lower Chobe and the Zambesi rivers were calculated to have a depth of between thirty and forty feet, but the reaches and the rapids make all navigation impracticable.

Having obtained permission from the king, the Marutze kingdom was visited. Hippopotami and crocodiles were found abundant in the rivers, but all such creatures had to be for the moment overlooked because King Sepopo was waiting to receive the white man. At the banquet fish of many sorts seems to have been the principal food;

FIG. 4.—Ladle and Calabashes.

but at a supper also given, boiled eland flesh was served with a sauce made of meal, and the drink was impote (honey beer). The king demanded no present, though, such being usual, Holub presented him with a Snider breech-loader and 200 cartridges. A good deal of interesting details are given about the kingdom of Marutze, which now extends along both sides of the Zambesi, from Sekhose, to about 150 miles south of the confluence of the Kabompo and the Liba. It is a most productive portion of Africa, as well adapted for agriculture as for cattle breeding, abounds in game, and seems prolific in vegetable products, of which indiarubber is not the least important.

Not at once getting the king's permission to pursue his journey to the source of the Zambesi, Dr. Holub returns to Panda Ma Tenka, and then accompanied his friends Westbeech and Francis on a visit to the Victoria Falls, which were about fifty miles off, which are declared to be, so far as the author's experience goes, the most imposing

[1] "Seven Years in South Africa. Travels, Researches, and Hunting Adventures between the Diamond Fields and the Zambesi." By Dr. Emil Holub (translated by Ellen E. Frewer). With about 200 original illustrations and a map. In two volumes. (London: Sampson Low, Marston, Searle, and Rivington, 1881.) Continued from p. 38.

phenomena in the world. Staying there three days, after the amusement of a lion hunt and several adventures, they returned on September 24 to Panda Ma Tenka. With somewhat failing health our author once again turned his face to the sources of the Zambesi; but when he got to Sesheke the king told him he had been too long in coming, that it was too late to go now, and he had not kept the guides waiting for him. The king declared it would take him over four months to reach the Zambesi sources in the kingdom of the Iwanyoe. An elephant hunt on a grand scale took place about this time, but ended in a panic, during which the whole herd of elephants escaped; but a lion hunt was more successful. Dr. Holub says he heard that one of the days during another great elephant hunt a herd of over a hundred elephants had been seen, but although at least 10,000 bullets had been fired off, only four elephants had been killed. At last leave was given to the author to accompany some of the queens who had come from the Barotse country, and on December 1 he was off Three royal canoes were placed at his disposal, but he had to ask for a fourth, and even then his servants had to proceed on foot along the banks. The Barotse rapids were safely ascended, but at the rapids known as Mutshila Aumsinga one of the canoes, that which carried all his provisions, gunpowder, medicines, and natural history collections, was capsized, and this ended all his schemes of penetrating far into the country; and thus the preparations of seven previous years proved fruitless. The severe wetting and the extreme disappointment, brought on a dangerous attack of fever, and, growing worse and worse, there was finally no alternative but to return. After a long delay at Sesheke in hopes of recovery he was compelled, after some weeks, to revisit Panda Ma Tenka. An interesting account is given of the manners and customs of the Marutze tribes. They seem to believe in a Supreme Being in good and evil spirits, in the continued existence after death; they are fair agriculturists and good cattle breeders, having a fertile soil, a genial climate, and abundance of water; though the tsetse fly is met with, game abounds; Kaffir corn, maize, beans, cotton, and tobacco are cultivated; salt is expensive; beer from corn is usually drunk at meals; they have also a cider-like drink and the honey beer. The people are cleanly in their persons and keep their food material in well-washed wooden or earthenware bowls or in suitable baskets or calabashes. Some of these are very tastefully decorated, and in the accompanying figures (Fig. 4) one will be seen with animal designs. The medical knowledge of the Marutze would appear to be in advance of many of the South African tribes; they know the properties of a number of medicinal or poisonous plants; the treatment of fever, coughs, and wounds. Bleeding was a common operation among them, and was employed in cases of neuralgia or to reduce inflammation.

FIG. 5.—Rock caves and inscriptions of Bushmen.

After many troubles and trials Panda Ma Tenka was

left, and the return journey was made by the way of the Makalaka and West Matabele countries, and a hearty welcome was given to the traveller on his arriving at Shoshong by the Mackenzies. While here the news arrived that war had broken out in the Transvaal between the Boers and Sekokuni. The journey to the Diamond Fields was made by Limpopo, and shortly Kimberley was for the fourth time reached. Settling at Bullfontein, the doctor with indefatigable energy soon got into large practice, and during two years, surrounded by the various animals and birds he had collected in this journey, his establishment was quite a menagerie. One holiday he paid a visit to the Orange Free State. When he viewed the Rocky Caves used by the Bushmen, he was particularly attracted by the remarkable carvings on the rocks done by the Bushmen to adorn their primitive abodes. A sketch of some of these is represented in the adjoining woodcut (Fig. 5). The rock is chiefly a sandstone, and the drawings are frequently executed in coloured ochres.

After a considerable period spent at Bullfontein, at Grahamstown, at Port Elizabeth, and at Cape Town, he embarked on board the *Germania* for Europe in August, 1879, bringing with him large ethnological and natural history collections. While the author's travels have added something to our previous knowledge of the geography of the portions of Africa he traversed, his account of them is really pleasant reading, and will be found of special interest to the naturalist and sportsman.

ELECTRIC LIGHTING[1]

III.

DECIDEDLY the most successful application of the electric light in London is at the Cannon Street station of the South Eastern Railway. The Charing Cross station of that Company has been lit up by the Brush system, and the Bricklayers' Arms goods-yards and sheds by Mr. Crompton's system, so that the South Eastern Railway officials have an admirable competitive trial proceeding within easy reach of inspection. The Cannon Street station is lit up by the British Electric Light Company with Gramme machines and Brockie lamps.

The engine—one of Marshall's semi-portable type—is of 14 horse-power nominal, and has a double cylinder on a locomotive boiler. The power is transferred by countershafting to the dynamo-machines by a system specially designed for the purpose, which is shown in the following sketch (Fig. 2). Large heavy fly-wheel pulleys give a second motion to the fly-wheel, which secures great steadiness—an essential feature of electric lighting. The engine is controlled by Hartnell's automatic governor, which regulates the expansion-gear of the engine and secures great uniformity of action.

The dynamo-machines are of a new class of Gramme, of high electromotive force, and they generate currents powerful enough to work five lamps. The current produced is of 26 vebers strength, and works a circuit of about 8 ohms resistance, thus giving an electromotive force of 208 volts. There are two machines at work, working ten lamps—eight being inside the station and two outside. The dynamos are fed by smaller Grammes, as shown in Fig. 2.

The lamps are Brockie's, the mechanism of which is extremely simple, consisting only of one magnet with a clutch, which, by means of a branch circuit, periodically interrupted by the commutator, readjusts the arc by letting the clutch fall, which releases the carbons and brings them momentarily together, and then picks them up again very smartly, so as to separate them the required distance. This gives the lamp a blinking habit,

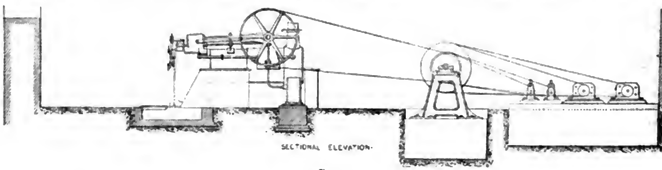


FIG. 2.

which at first is peculiar, but which one soon gets accustomed to and ignores. The diagram (Fig. 3) clearly illustrates how this is done. The magnets are fixed on shunts, two lamps being on two shunts and one lamp on the other. The shunts are of smaller wire than that of the main circuit, but they do not interfere with the main current, which passes through the carbons—in fact the shunts reduce the total resistance of the circuit. The lamps burn for four and a half hours, but it is intended to introduce a double set of carbons, which will of course duplicate this time.

Mr. Brockie has introduced quite a new principle into electric lighting, and certainly, to judge from the effect at Cannon Street, his success is unqualified. It remains to be seen how far this success is repeated at the General Post Office, at Victoria Street, Manchester, at Prince's Dock, Liverpool, and in the town of Liverpool itself. We certainly would like to see a good west-end street, say Piccadilly, Regent Street, or the Haymarket, lit up by this system.

Another system, not yet extensively employed, is Joel's improved incandescent electric lamp. In the latter part of 1878 considerable interest was excited in both scientific and commercial circles by the announcement that M. Werdermann had succeeded in the so-called division of the electric light by an invention based on the incandescent principle. His system was exhibited on an experimental scale only for some time, and then suddenly disappeared from public notice.

This incandescent principle has recently been revived, with many and ingenious improvements in the mechanism of the lamp, by Mr. Joel. An illustration of the hanging lamp is shown in Fig. 4. The light is reproduced, as was the case in M. Werdermann's system, by the heating to incandescence of the end of a small rod or pencil of carbon forming one electrode, which protrudes through a pair of contact jaws and abuts upon a fixed cylinder of copper forming the other electrode. The carbon pencil consumes at the rate of $2\frac{1}{2}$ to 3 inches per hour for lights of 100 candle-power and upwards, and is fed forward according to the consumption. The length of carbon in circuit between the contact jaws and the fixed electrode is about three-quarters of an inch, and this, by the passage of the

[1] Continued from p. 35.

current, is rendered highly incandescent, chiefly however at that part near the copper electrode, where the pencil becomes pointed, and therefore more intensely heated. There is also, in addition to this, a glow or flame-like appearance from the sides of the consuming carbon to the copper electrode, the light thus apparently taking an intermediate position between the purely incandescent system and that of the arc. The heated point of carbon becomes curled at the tip in a peculiar manner, as though it were viscous in shape, somewhat like a mushroom where it wastes away, and is replaced by the gradual forward motion of the pencil.

The fixed electrode, which may be entirely of copper or with a graphite insertion, remains intact without any appreciable wear.

The chief improvements in this lamp consist in the simplification and certainty of action of the mechanism in connection with the contact jaws for clamping the carbon pencil, by which means the lateral pressure of the jaws and the feeding of the carbon are attained by the combined action of one actuating weight, as shown in the diagram, Fig. 5. It will be seen from this that the lateral pressure is thus always proportional to the downward pressure, and may be varied to suit any conditions. The details devised for rendering the lamps in the same circuit independent of each other, and its general adaptability for interior and domestic lighting, constitute an important advance on anything which has gone before.

On referring to the sectional view of the lamp, Fig. 5, E is the fixed copper electrode upon which abuts the point

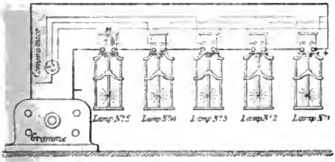


FIG. 3.

of the carbon *c*, which is rendered intensely incandescent by the passage of the current between the jaws and the copper cylinder. The jaws are shown at J clamping the carbon pencil. The actuating weight, W, which gives both the feeding motion to the pencil as it consumes, and the lateral pressure to the jaws, is suspended by continuous cords to the top of the lamp B, the cords then passing down through the weight and under one of the rollers at R, up again through a roller attached to the carbon holder, then back again through another roller at R, and ending at the weight. The rollers R are attached to a light tube, P, which passes down through a nipple, N, and terminates in a flange under the horizontal arms of the jaws and lifts them according to the leverage, thus producing the lateral pressure on the pencil. The top of this tube has also attached to it the armature, A, of an electro-magnet S, wound with fine wire and arranged in a shunted circuit in such a manner that as long as the normal condition of the light is maintained it is neutral; but if an arc should be accidentally formed between the carbon *c* and the copper E, the electro-magnet comes into action in opposition to the controlling weight, and frees the jaws from lateral pressure, thereby allowing the carbon pencil to descend freely and establish contact.

The carbon-holder is also arranged (Fig. 6) so that when the pencil is very nearly consumed the lamp is automatically short-circuited by the lever at L making contact with the arm carrying the copper electrode.

The stem or body of the lamp forms an important part of the whole, being formed of metal tubing in two semi-circular halves, each half forming part of the electrical circuit, the current traversing one side, of which the jaws form part, passing through the carbon pencil to the copper electrode, and returning by the other side of the lamp.

The two sides are kept closed mechanically (but insulated electrically from each other) by the latch and knob T, Fig. 5, which also automatically short-circuits the lamp when opened for the purpose of putting in fresh carbon, thus rendering it perfectly safe in handling. There are

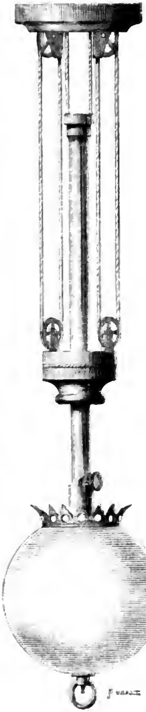


FIG. 4.

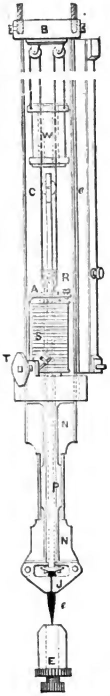


FIG. 5.

ingenious arrangements attached to this system for switching a lamp on or off, with resistances equivalent to that of the lamp; but upon these it is unnecessary to dwell, these adjuncts being common to many systems.

Prof. Adams stated at the Society of Arts that with this system an illuminating power of 715 candles per horse-power could be obtained.

If incandescent lighting is more expensive than the

arc system, which it necessarily is when the current has to traverse a number of small lights, it has the great advantage of possessing perfect steadiness, which an arc lamp can never rival, and for interior lighting this is of great importance. The cost of carbons constitutes an important item in the expenses attached to electric lighting as now employed, and if we consider that in some incandescent systems the consumption of material is for a considerable period nothing at all, we may still work economically even though using considerably more horse-power to obtain our results.

Incandescent lamps, however, as at present constructed, are limited to small lights and a certain steady strength of current, as any sudden increase is apt to break the thin carbon filament employed. In addition to this it is necessary to protect the incandescent carbon of such lamps from the influence or access of oxygen, as it would be rapidly consumed by even the slightest amount of oxygen present. Therefore it must be protected by inclosing it in a vacuum, and it is a matter of considerable difficulty to produce a sufficiently perfect vacuum to prevent some small quantity of free oxygen from coming into contact with the light-giving material. Incandescent lamps are very capricious. Difficulties arise from the extreme thinness and delicacy of the glass employed, leakage from defective sealing or fractures, the liability of the incandescent material to shake loose in its supports,

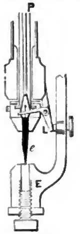


FIG. 6.

and the great care required in manipulation. The Joel lamp is free from these objections, and as in lamps that are purely incandescent, the heat is produced by the current only (the carbon not undergoing combustion by reason of the absence of oxygen), it follows that the incandescent portion cannot attain so high a temperature as when the carbon consumes, and therefore the light must necessarily be of less power than that in the lamp described. The offices of the Electric Light Agency in Queen Victoria Street are lighted by this system, and in the workshop two of these lamps take the place of fifteen gas jets with highly satisfactory results. The carbons employed are of 5 mm. diameter, and in length of about 1 m. The lamp burns for seven or fourteen hours, according to the dimensions of the carbon.

The Swan lamp is the only purely incandescent lamp that has met with any success in England. The Maxim light, the most successful in America, has not reached here yet. Dr. Draper's house in New York is lit by it, and he is able to manipulate his lamps with all the ease and comfort of gas-fittings. Sir William Armstrong, at Craigside, near Newcastle, has utilised a brook to run a dynamo-machine by means of a turbine, and he is able to maintain thirty-seven Swan-lights in his house. Mr. Spottiswoode occasionally gratifies his friends by illuminating his rooms with Swan-lights, and the rooms of the Royal Society were so lit at their last *soirée*. But such lamps remain luxuries, and nothing more.

Wherever the electric light has been introduced for internal illumination it has met with considerable favour. It not only lowers the temperature of a gas-lit room within reasonable bounds, but it clears the atmosphere of vitiations, and men work more cheerfully and better. In fact the extra amount of work got out of men is said in some instances to pay for the change. Moreover, since it renders the illumination comparable with that of daylight, it enables the aged and the weak-sighted to read and work without spectacles.

Electric lighting has however passed the experimental, it has now reached the practical stage.

HOW TO PREVENT DROWNING

I WISH to show how drowning might, under ordinary circumstances, be avoided even in the case of persons otherwise wholly ignorant of what is called the art of swimming. The numerous frightful casualties render every working suggestion of importance, and that which I here offer I venture to think is entirely available.

When one of the inferior animals takes the water, falls, or is thrown in, it instantly begins to walk as it does when out of the water. But when a man who cannot "swim" falls into the water, he makes a few spasmodic struggles, throws up his arms, and drowns. The brute, on the other hand, treads water, remains on the surface, and is virtually insubmergible. In order then to escape drowning it is only necessary to do as the brute does, and that is to tread or walk the water. The brute has no advantage in regard of his relative weight, in respect of the water, over man, and yet the man perishes while the brute lives. Nevertheless any man, any woman, any child who can walk on the land may also walk in the water just as readily as the animal does, if only he will, and that without any prior instruction or drilling whatever. Throw a dog into the water and he treads or walks the water instantly, and there is no imaginable reason why a human being under like circumstances should not do as the dog does.

The brute indeed walks in the water instinctively, whereas the man has to be told. The ignorance of so simple a possibility, namely the possibility of treading water, strikes me as one of the most singular things in the history of man, and speaks very little indeed for his intelligence. He is, in fact, as ignorant on the subject as is the newborn babe. Perhaps something is to be ascribed to the vague meaning which is attached to the word swim. When a man swims it means one thing, when a dog swims it means another and quite a different act. The dog is wholly incapable of swimming as a man swims, but nothing is more certain than that a man is capable of swimming, and on the instant, too as a dog swims, without any previous training or instruction, and that by so doing without fear or hesitancy, he will be just as safe in the water as the dog is.

The brute in the water continues to go on all fours, and the man who wishes to save his life and cannot otherwise swim, must do so too, striking alternately, one two, one two, but without hurry or precipitation, with hand and foot, exactly as the brute does. Whether he be provided with paw or hoof, the brute swims with the greatest ease and buoyancy. The human being, if he will, can do so too, with the further immense advantage of having a paddle-formed hand, and of being able to rest himself when tired, by floating, a thing of which the animal has no conception. Bridget Money, a poor Irish emigrant, saved her own life and her three children's lives, when the steamer conveying them took fire on Lake Erie, by floating herself, and making them float, which simply consists in lying quite still, with the mouth shut and the head thrown well back in the water. The dog, the horse, the cow, the swine, the deer, and even the cat, all take to the water on occasion, and sustain themselves perfectly

without any prior experience whatever. Nothing is less difficult, whether for man or brute, than to tread water, even for the first time. I have done so often, using the feet alone or the hands alone, or the whole four, many times, with perhaps one of my children on my back. Once I recollect being carried a good way out to sea by the receding tide at Boulogne, but regained the shore without difficulty. A drop of water once passed through the rima of the glottis, and on another occasion I experienced such sudden indisposition that if I had been unable to float, it must, I think, have gone hard with me.

Men and animals are able to sustain themselves for long distances in the water, and would do so much oftener were they not incapacitated, in regard of the former at least, by sheer terror, as well as complete ignorance of their real powers. Webb's wonderful endurance will never be forgotten. But there are other instances only less remarkable. Some years since, the second mate of a ship fell overboard while in the act of risting a sail. It was blowing fresh; the time was night, and the place some miles out in the stormy German Ocean. The hardy fellow nevertheless managed to gain the English coast. Brock, with a dozen other pilots, was plying for fares by Yarmouth; and as the main-sheet was belayed, a sudden puff of wind upset the boat, when presently all perished except Brock himself, who, from four in the afternoon of an October evening to one the next morning, swam thirteen miles before he was able to hail a vessel at anchor in the offing. Animals themselves are capable of swimming immense distances, although unable to rest by the way. A dog recently swam thirty miles in America in order to rejoin his master. A mule and a dog washed overboard during a gale in the Bay of Biscay have been known to make their way to shore. A dog swam ashore with a letter in his mouth at the Cape of Good Hope. The crew of the ship to which the dog belonged all perished, which they need not have done had they only ventured to tread water as the dog did. As a certain ship was labouring heavily in the trough of the sea, it was found needful, in order to lighten the vessel, to throw some troop-horses overboard which had been taken in at Corunna. The poor things, my informant, a staff-surgeon, told me, when they found themselves abandoned, faced round and swam for miles after the vessel. A man on the east coast of Lincolnshire saved quite a number of lives by swimming out on horseback to vessels in distress. He commonly rode an old grey mare, but when the mare was not to hand he took the first horse that offered.

The loss of life from shipwreck, boating, bathing, skating, fishing, and accidental immersion is so disastrously great, that every feasible procedure calculated to avert it ought to be had recourse to. People will not consent to wear life-preservers, but if they only knew that in their own limbs, properly used, they possessed the most efficient of life-preservers, they would most likely avail themselves of them. In every school, every house, there ought to be a slate tank of sufficient depth, with a trickle of water at one end and a syphon at the other, in order to keep the contents pure. A pail or two of hot water would at any time render the contents sufficiently warm. In such a tank every child from the time it could walk ought to be made to tread water daily. Every adult, when the opportunity presents itself, should do so. The printed injunction should be pasted up on all boat-houses, on every boat, at every bathing place, and in every school. "Tread water when you find yourself out of your depth" is all that need be said, unless indeed we add, "Float when you are tired." Every one, of whatever age or sex, or however encumbered with clothing, might tread water with at least as much facility, even in a breaking sea, as a four-footed animal does. The position of a person who treads water is, in other respects, very much safer and better than is the sprawling attitude which we assume in ordinary swimming. And then the beauty of it is that we can tread water without any preliminary teaching, whereas "to swim" involves time and pains, entails considerable fatigue, and is very seldom adequately acquired after all.

The Indians on the Missouri River, when they have occasion to traverse that impetuous stream, invariably tread water just as the dog treads it. The natives of Joanna, an island on the coast of Madagascar, young persons of both sexes, walk the water carrying fruit and vegetables to ships becalmed, or it may be lying-to, in the offing miles away. Some Croomen whose canoe upset before my eyes in the seaway on the coast of Africa walked the water, to the safe-keeping of their lives, with the utmost facility; and I witnessed negro children on other occasions doing so at a very tender age. At Madras, watching their opportunity, messengers, with letters secured in an oilskin cap, plunge into the boiling surf, and make their way, treading the water, to the vessels outside, through a sea in which an ordinary European boat will not live. At the Cape of Good Hope men used to proceed to the vessels in the offing through the mountain billows, treading the water as they went with the utmost security. And yet here, on our own shores, and amid smooth waters, men, women, and children perish like flies annually, when a little properly-directed effort—treading the water as I have said—would haply suffice to rescue them every one.

Belfast HENRY MACCORMAC

NOTES

WE learn from the *Times* that at the meeting of the Royal Society on Thursday last the vacancies in the list of foreign members were filled by the election of Gabriel Auguste Daubrée of Paris, Jean Charles Marignac of Geneva, Carl Nägeli of Munich, and Carl Weierstrass of Berlin.

SIR JOHN LUBBOCK has been nominated to succeed Prof. Allman as president of the Linnean Society, and Mr. G. J. Romanes for the post of zoological secretary, vacant by Mr. Alston's lamented death.

IN the current number of the *Revue Scientifique* there is an article by M. de Lacaze-Duthiers descriptive of an interesting enterprise on which he is engaged, viz. the construction of a zoological laboratory at Port Vendres. Backed by the recommendation of the Academy of Sciences, he obtained a liberal offer from the municipal authorities of the place, which among other considerations determined him in the selection of the site. Altogether he is provided with 32,000 francs as a capital sum, 750 francs per annum as a fixed income, with the gift of building ground and a boat. It will thus be seen that the municipal authorities deserve all credit for the substantial encouragement which they have extended to the undertaking. In a few months the laboratory will be completed, and is then to be thrown open to workers of all nationalities. As its situation on the coast of the Mediterranean is an admirable one for the procuring of fauna, the institution is in every way favourably circumstanced, and we cordially wish it all success.

THE English Transit of Venus Commission having expressed a desire for an understanding with the French Commission, so as to secure a uniform method of observation, M. d'Abbadie and M. Tisserand are coming to London to compare notes with the English Commission.

A LETTER from M. Mascart, director of the French Central Meteorological Bureau, read at last week's meeting of the Paris Academy of Sciences, stated that the French Government intend to establish an observatory for terrestrial magnetism at Cape Horn. The expedition will set out in the same vessel as will

take the astronomers who are to observe the approaching transit of Venus. This is intended as the contribution of France to the international scheme of polar observations, to which England has as yet made no sign of lending her aid.

MESSRS. SIEMENS AND HALSKE gave a public trial last week of their new electric railway, which runs between Lichterfelde and the Cadettenhaus, about six miles from Berlin. The trial is stated to have been most successful. It was in a simple tram-car, with an electric battery totally concealed between the wheels, in connection, through the rails it ran on, with the principal battery at the station. The rails are three feet three inches apart, and exactly resemble those of an ordinary railroad, only the gauge being narrower. The greatest speed obtained on a distance of about one and a half mile was eighteen English miles an hour. Dr. Siemens has proved that if necessary a far greater speed could be obtained, but this is not allowed by the German police authorities. It will not be allowed to proceed at more than nine miles per hour. The railway was opened to the public on Monday.

M. POUCHET with two assistants are about to proceed to Vadsö, on the east coast of Finmark, to collect natural history specimens for the Paris Museum of Natural History.

AN interesting paper on recent earthquakes in Japan has lately been published by Prof. John Milne, vice-president of the Seismological Society of Japan. To get an arrangement which will cause a clock to stop at exactly the same tremor during an earthquake as another similar clock is stopped at, to get a complete time record of the tremors at any place, and to find accurately the direction of the transmission of earth vibrations, these are the questions which Mr. Milne and his friends have been trying to surmount with the help of a grant of money from the British Association and the help of the Japanese Telegraph Department. A result of the work hitherto done is that there is a chronic centre of disturbance within a radius of a few miles from Yokohama, and Mr. Milne felicitates the inhabitants of that seaport on the advantages which their position gives them for seismological observation. The Society has also issued useful forms in which to record earthquake observations, which might supply hints to European observers.

AT the Victoria (Philosophical) Institute, on Monday, a paper on "The Rainfall and Climate of India" was read by Sir Joseph Fayrer, K.C.S.I., F.R.S. He reviewed the causes and effects of those climatic changes which obtain in that country at the present time, and many of which once operated in Palestine and Egypt, not to mention England and other parts of the world, and threw light on questions involving the denudation theory, the variation of river deposits, and other matters affecting the uniformitarian theory of geology. Sir Joseph Fayrer spoke at some length in regard to the climate of India, and showed that, if what science had taught us in regard to the effects of cultivation, the preservation of the forests, drainage, &c., were carefully attended to by the powers that be, the importance of the results could not be estimated, as they involved the health and prosperity of that great country.

IN connection with the subject of "Fascination," Dr. Otto of Schemnitz, Hungary, writes us of a case which came under his notice. In 1859, when the use of firearms was under stringent regulations in Hungary, peasants often killed hares on the Danubian island Creppel in the following way:—Two peasants would drive in a cart over the reaped fields. On spying a hare (say at two to three hundred paces) they proceeded to drive round it some five or six times in succession. One peasant carrying a long stick at length sprang out, at the moment the cart was behind the hare (the cart continuing its course), and coming up to the animal slowly, killed it without difficulty. It was not uncommon to kill thus as many as six or seven hares in one morning.

THE following instance of animal intelligence is sent to us by Dr. John Rae, F.R.S., who states that the Mr. William Sinclair mentioned is respectable and trustworthy. The anecdote is taken from the *Orkney Herald* of May 11:—"A well-authenticated and extraordinary case of the sagacity of the Shetland pony has just come under our notice. A year or two ago Mr. William Sinclair, pupil teacher, Holm, imported one of these little animals from Shetland on which to ride to and from school, his residence being at a considerable distance from the school buildings. Up to that time the animal had been unshod, but some time afterwards Mr. Sinclair had it shod by Mr. Pratt, the parish blacksmith. The other day Mr. Pratt, whose smithy is a long distance from Mr. Sinclair's house, saw the pony, without halter or anything upon it, walking up to where he was working. Thinking the animal had strayed from home, he drove it off, throwing stones after the beast to make it run homewards. This had the desired effect for a short time; but Mr. Pratt had only got fairly at work once more in the smithy when the pony's head again made its appearance at the door. On proceeding a second time outside, to drive the pony away, Mr. Pratt, with a blacksmith's instinct, took a look at the pony's feet, when he observed that one of its shoes had been lost. Having made a shoe he put it on, and then waited to see what the animal would do. For a moment it looked at the blacksmith as if asking whether he was done, then pawed once or twice to see if the newly-shod foot was comfortable, and finally gave a pleased neigh, erected its head, and started homewards at a brisk trot. The owner was also exceedingly surprised to find the animal at home completely shod the same evening, and it was only on calling at the smithy some days afterwards that he learned the full extent of his pony's sagacity."

M. J. PLATEAU has issued a second supplement to the "Bibliographie Analytique des Principaux Phènomenés Subjectifs de la Vision," comprising the years 1878-79. It is reprinted in a separate form from the *Memoirs* of the Belgian Academy.

WE have received the first three parts of the *Zeitschrift für Instrumentenkunde*, a monthly journal intended to bring together all novelties in scientific apparatus. It is edited by Dr. George Schwiskus, assisted by a large staff, and published by Julius Springer of Berlin. It is amply illustrated and its utility is obvious.

M. CARNOT, the grandson of the celebrated War Minister of the First Republic, has taken an important step in his capacity of Minister of French Public Works. He sent to the Lower House a *projet de loi* asking for a credit of 280,000*l*. in order to introduce at once into all the French lighthouses magneto-electric generators and acoustical signals with steam blower. This proposal is sure to be accepted with enthusiasm, and executed with the utmost rapidity.

THE General Council of the Seine have granted the credits asked by the Préfet for establishing in the Morgue a refrigerating machine by MM. Mignon and Rouard. Ammonia is the substance which has been considered as the most powerful and cheapest by a special commission appointed by the Council of Hygiene. Laboratories will be annexed to the establishment. The work will begin as soon as Parliament has voted a small sum for contributing to the expense.

M. BARTHÉLEMY ST. HILAIRE, French Minister of Foreign Affairs, has recommended to M. Cochéry a suggestion of M. W. de Fonvielle's advising the appointment of a commission on the state of international law relating to ocean telegraphic cables, and the means of improving it. M. St. Hilaire states that in

case the Congress of Electricians comes to any conclusion relating to this most important object, he is ready to send a circular to the several Governments on the opening of an international conference on the matter. Thi official correspondence will be published in full in the next number of *L'Électricité*.

WE take the following from the *Colonies and India*:—"To say that a train had been stopped by caterpillars would sound like a Yankee yarn, yet such a thing (according to the *Rangitikei Advocate*) actually took place on the local railway a few days ago. In the neighbourhood of Turakina, New Zealand, an army of caterpillars, hundreds of thousands strong, was marching across the line, bound for a new field of oats, when the train came along. Thousands of the creeping vermin were crushed by the wheels of the engine, and suddenly the train came to a dead stop. On examination it was found that the wheels of the engine had become so greasy that they kept on revolving without advancing—they could not grip the rails. The guard and the engine-driver procured sand and strewed it on the rails, and the train made a fresh start, but it was found that during the stoppage caterpillars in thousands had crawled all over the engine, and over all the carriages inside and out."

A SHOCK of earthquake is reported from Möttling (Carniola) on April 26, at 4.55 p.m., direction from north to south. At Tuffers (Styria) a smart shock was felt on May 6, at 7.41 p.m., duration three seconds, direction north-east to south-west.

ON Tuesday last week, the Princess Christian of Schleswig-Holstein presented the prizes and certificates adjudged to candidates in a competitive examination on "Domestic Sanitation," following a course of lectures delivered on the subject by Dr. B. W. Richardson. With regard to the course of lectures he had given at the request of the Ladies' Sanitary Association, Dr. Richardson stated that nearly 300 pupils attended, of whom seventy-five competed for the prizes offered by Mr. Edwin Chadwick and others. Of the papers sent in, he could say that all the writers showed a sound knowledge of four subjects, viz. the relative values of the substances used as foods, the circulation of the blood, the process of breathing, with the conditions which produce a pure and healthy dwelling, and the management of a sick room. Dr. Richardson announced that, by desire of the Ladies' Sanitary Association, he should deliver another course of lectures, beginning in October next, on the nervous system. This would raise questions concerning education and other interesting and, at present, debatable matters.

IN the *Revue Scientifique* for May 14 is the conclusion of a long paper on the Physiological Immunities enjoyed by the Jewish race, in which the nature of these immunities is examined and the probable reasons for them given.

THE additions to the Zoological Society's Gardens during the past week include an Indian Fruit Bat (*Pteropus medius*) from India, presented by Mr. Edwin H. Maskell; a Wood Brocket (*Cariacus nemorivagus*) from South America, presented by Capt. Mackenzie, s.s. *Severn*; an Egyptian Gazelle (*Gazella dorcas*) from Egypt, presented by Mrs. J. J. Jones; a Common Hare (*Lepus europæus*), British, presented by Mr. Wormald, F.Z.S.; two Hawfinches (*Coccothraustes vulgaris*), British, presented by Dr. Bree; three Viperine Snakes (*Tropidonotus viperinus*) from North Africa, presented by Mr. J. C. Church; a Common Adder (*Vipera berus*), British, presented by Mr. G. H. King; a Three-striped Paradoxure (*Paradoxurus trivirgatus*) from India, a Javan Adjutant (*Leptoptilus javanicus*) from Java, received in exchange; six Rose-coloured Pastors (*Pastor roseus*) from India, two Mandarin Ducks (*Aix galericulata*) from China, purchased; a Blue and Yellow Macaw (*Ara ararauna*) from South America, deposited; a Geoffroy's Dove (*Peristera geoffroii*), three Red-crested Whistling Ducks (*Fuligula rufina*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—Mira Ceti, which was at its minimum on March 20, according to Prof. Schönfeld's formula in his second catalogue of variables, will attain a maximum by the same on July 8, and may therefore be observed as it approaches that phase. The next maximum takes place on April 1, 1882, and will not be observable.

χ Cygni, by the recent observations of Prof. Julius Schmidt, may also be expected to reach its maximum about July 10, perhaps a few days later: the last maximum occurred on May 30, 1880, when the star was 6m.; it has occasionally attained 4m. at maximum. The perturbations in this case appear to be considerable.

The position of the variable usually designated *Nova* 1848 may be identified by means of Prof. Schmidt's observations of neighbouring stars. In *Astron. Nach.*, No. 1708, he gives the following places for 1855·0:—

Mag.		R.A. h. m. s.		Decl. ° ′ ″	
11	...	16 50 53·3	...	−12 43 57	
13	...	— 51 18·1	...	12 42 0	
13·12	...	— 51 22·5	...	12 40 3	*Nova* 1848
11	...	— 51 44·8	...	12 47 8	
11	...	— 51 51·9	...	12 31 57	
10·11	...	16 53 9·2	...	−12 47 2	

Further, the variable follows the star, Lalande's star 30,853 a ninth magnitude, 14·8s., and is north of it 18′ 21″.

Prof. Winnecke's star of the twelfth magnitude, in close proximity to the place of Kepler's *Nova* 1604, deduced from the observations of Fabricius, and apparently in the position of a star marked 10m. by Chacornac, but not since observed of that brightness, well deserves watching, and it would be interesting to possess a carefully-formed map of all stars visible in the vicinity of Kepler's celebrated star, with the aid of one of our most powerful telescopes—similar to that prepared by D'Arrest with the Copenhagen refractor for the vicinity of Tycho's *Nova* 1572 in Cassiopea. Prof. Winnecke's star precedes the 9m., No. 16872 in Oeltzen's Argelander 33·2s., and is 2′ north of it.

THE SATELLITES OF SATURN.—Observations of these satellites are still followed up at the Observatory of Toulouse, and M. Baillaud has communicated a series made about the last opposition of the planet to the Paris Academy of Sciences. Amongst them are a number of observations of *Mimas*, consisting mainly of elongations, but with several attempts to fix the moments when the satellite was on the tangent to the extremity of the ring. M. Baillaud does not appear to regard the latter observation with favour, on account of the difficulty attending it, but proposes to gain further experience of the degree of precision of which it admits. The later observations of *Mimas* are as follows:—

1880		h. m. s.	1880		h. m. s.
Nov. 1 ...	W. ...	6 59 40	Nov. 25 ...	N.E. ...	10 23 50
13 ...	W. ...	12 58 17	Dec. 18 ...	W. ...	10 1 7
23 ...	E. ...	10 33 13	19 ...	W. ...	8 42 33

The times are mean times at Toulouse, 3m. 29·9 . west of the Observatory of Paris. The observation of November 25 relates to the passage of the satellite by the tangent to the extremity of the ring.

Dr. M. W. Meyer of Geneva publishes elements of *Enceladus*, *Tethys*, *Dione* and *Rhea*, obtained on a new method, from observations made with the 10-inch Geneva refractor. By the way he terms the second of these satellites *Thetis*, not the only time that Sir John Herschel's proposed designation has been mistaken of late. *Thetis*, as is well known, is appropriated for one of the minor planets discovered by Dr. R. Luther.

SWIFT'S COMET (1881 *a*).—M. Bigourdan has calculated the following elements of this comet, from the Dun Echt observation on May 2 and observations made at Paris on May 5 and 7:—

Perihelion passage 1881, May 21·0613, Paris M.T.

	° ′ ″	
Longitude of perihelion	297 54 43	M. Eq.
,, ,, ascending node	119 24 5	1881·0
Inclination	81 40 56	
Log. distance in perihelion	9·75568	

Motion—direct.

Though observations will not be longer practicable in these latitudes the comet may perhaps be observed in the southern

hemisphere, it being understood that telegrams have been sent to the Cape and to Australia (by Lord Crawford) with this object. According to the above orbit, on July 9 the comet will have one-fifth of the intensity of light on the night of discovery. There is no close resemblance of elements to those of any comet previously calculated.

THE "ASTRONOMISCHE NACHRICHTEN."—It is announced that after the termination of the current volume, by authority of the Prussian Government a new arrangement for the management of this journal will take effect. It will be edited by Prof. A. Krueger, the director of the Observatory at Kiel, in co-operation with the president of the "Astronomische Gesellschaft," of which association it will become a recognised organ.

BIOLOGICAL NOTES

LIMULUS POLYPHEMUS.—A paper on the anatomy, histology, and embryology of *Limulus polyphemus*, by A. S. Packard, jun., M.D. (*Anniversary Mem.* Boston Soc. Nat. Hist. 1880), may be regarded as a continuation of the author's former series on the development and affinities of the king-crab. He discusses fully the question of the affinities of that puzzling animal, and combating the position of those zoologists who connect Limulus with the Arachnida, he sums up the facts which point to the crustacean nature of Limulus as follows:—(1) The nature of the branchiæ, those of Limulus being developed in numerous plates overlapping each other on the second abdominal limbs (those of the Entrypterida being, according to H. Woodward, attached side by side like the teeth of a rake), while the mode of respiration is truly crustacean; (2) the resemblance of the cephalothorax of Limulus to that of Apus; (3) the general resemblance of the gnathopods to the feet of Nauplius or larva of the cirrhipedia and copepoda; (4) the digestive tract is homologous throughout with that of Crustacea, particularly the Decapoda, there being no urinary tubes as in Tracheata; (5) the heart is on the crustacean type as much as on the tracheate type, and the internal reproductive organs (ovaries and testes) open externally, at the base of and in the limbs, much as in Crustacea. The paper is illustrated by seven plates showing the circulation of Limulus, sections of the adult and of embryos, and details of the structure of the eyes with comparison of these with those of Trilobites, with which group the author, as in his first memoir, allies the Merostomata.

THE NUMMULITIC ECHINIDS OF EGYPT.—A monograph of the Echinids contained in the Nummulitic strata of Egypt, by M. P. de Loriol, is published in the *Mem.* Soc. Phys. et d'Hist. Nat. de Genève (tom. xxvii. 1880, 1ère pt.). The specimens described by M. de Loriol were obtained mostly near Cairo and Thebes. The fauna of the Nummulitic strata of Egypt has been found by him as far as yet explored to contain forty-two species of Echinids, or about the same number as that of the Nummulitic strata of India; that of the Canton of Schwytz has only thirty-four, the Eocene fauna of the Antilles only eighteen; but the Eocene fauna of the Pyrenees has as many as ninety-three. In the present memoir, which is illustrated by ten plates, twenty new species are described. The author does not concur in Prof. Jeffrey Bell's reasons for the formation of his new genus Paleolampas, considering that there are not sufficient grounds for separating it from Echinolampas. Only four of the forty-two species composing the Egyptian Nummulitic fauna are regular Echinids, all the rest are irregular. Of the whole number all but eight are peculiar to Egypt. Of the eight exceptions four occur in the lower part of the Nummulitic formation at San Giovanni Harione, in the Vicentin, three in that of the Pyrenees, whilst the remaining one, *Hemispatangus depressus*, has been found in the Crimea in the same beds as *Echinolampas subcylindricus*, which also occurs at San Giovanni Harione.

SPONGES OF LAKE BAIKAL.—In a recent note to the St. Petersburg Academy, Dr. Dybowski says sponges occur in Lake Baikal wherever the bottom is rocky or large blocks of stone or wood are lying about. Close to the border of the lake, at a depth of 2 to 6 metres, they have a sod or cushion-like form, clinging to the stones, blocks, and (more largely) to decaying wood. In a depth of 6 to 25 metres they become tree- or shrub-like, with a height rarely exceeding 60 ctm.; while from 25 to 100 m. depth the sod or cushion-like form recurs, and only that is met with. The colour of the sponges is generally more or less dark grass-green, sometimes olive-green or brown. But those got from depths of 60 to 100 m., or found under stones, are nearly quite white.

MICROSCOPICAL EXAMINATION OF FARINA.—In examining any given kind of farina with the microscope to find whether a less nutritive farina has been mixed with it, it has been common to confine attention to the starch granules (which one may easily be mistaken about): Dr. Cattaneo holds (*Re. Ist. Lomb. Rend.* vol. xiv. fasc. v.) that greater importance should be attached to the character of the bran-particles, some of which are never wanting even in the most carefully-bolted flour. These (as he shows) differ in a marked way according to species.

THE HYPOPHYSIS IN ASCIDIANS.—While the hypophysis, or pituitary gland, found in the cranial cavity of adult vertebrates seems to be a rudimentary body without function, it is, in its earlier development, furnished, like all active glands, with an excretory passage into the alimentary canal. On the instance of M. van Beneden, M. Julin has lately studied an enigmatical organ in ascidians, a glandular apparatus under the brain (discovered by Hankow), which, it was thought, might be homologous with the pituitary gland of vertebrates. M. Julin examined the gland, the so-called anterior tubercle or vibratile organ, and various connected organs, in four species of ascidians at Leewik, on the Norwegian coast, and his researches (lately described to the Belgian Academy) appear to confirm M. van Beneden's conjecture. M. Julin is unable to regard the vibratile organ as an olfactory organ (the ordinary view); it receives no nerve-branch, and no olfactory cells can be found in its vibratile cylindrical epithelium. It is (he considers) merely the enlarged mouth of the excretory canal of the gland below the brain, leading into the buccal region, while the gland itself represents, in permanent state and functional activity, the embryonic hypophysis which becomes rudimentary in vertebrates. The *rôle* of the gland remains in obscurity. (Anatomical details will be found in the Academy's *Bulletin*, No. 2.)

PHYSICAL NOTES

AN extremely ingenious explanation of the peculiar green phosphorescence observed by Crookes in his researches on high vacua has recently been given by Mr. J. J. Thomson of Cambridge. This phosphorescence appears on the inner surfaces of the exhausted glass tubes whenever they are exposed to the so-called molecular bombardment of particles projected from the negative electrode. Mr. Thomson points out firstly that, as predicted by Clerk-Maxwell and verified by Rowland, a moving electrified particle acts as a current of electricity and possesses an (electro-magnetic) vector-potential. Now where such an electrified particle strikes a glass surface and rebounds, its change of velocity is accompanied by a change of vector-potential, and the glass against which it impinges and rebounds will be subjected to a rapid change in electromotive force. But by Clerk-Maxwell's electro-magnetic theory of light this is precisely what happens when a ray of light falls upon it. And therefore it phosphoresces as it would under the impact of an actual ray of light. It would be interesting to inquire whether all phosphorescent and fluorescent phenomena are capable of an analogous explanation in accordance with Clerk-Maxwell's theory.

MR. E. H. COOK proposes (*Phil. Mag.*) the term *sonorescence* as suitable to apply to the phenomena discovered by Graham Bell and investigated by Mercadier, Tyndall, and others, of the direct conversion of intermittent radiations into sound. The new name is obviously suggested by analogy with fluorescence and calorescence, but does not seem quite a happy one. Stokes gave the name of *fluorescence* to the phenomenon of the change of non-luminous ultra-violet rays into luminous ones. Akin gave the name of *calcescence* to the phenomenon of the change of non-luminous heat-rays into luminous ones (as in the lime-light), but the term has been superseded by Tyndall's term *calorescence*, which is etymologically unfortunate, seeing that the Latin verb is *calesco*, not *caloresco*. By strict analogy the term *sonorescence* should mean the conversion of sound into luminous rays, not the reverse change, to which Mr. Cook applies it.

THE researches of Edlund, Joubert, and others have left no doubt that the voltaic arc possesses an electromotive force of its own acting in a direction opposite to that of the current which sustains the arc. The principal work of maintaining the arc appears indeed to be spent in overcoming this opposing force, and is not occasioned by the resistance of the arc itself, which is small. M. Alfred Niaudet has lately announced the observation

of an important fact in connection with this subject, namely, that when the arc begins to emit the well-known *hissing sound* there is an abrupt change in the opposing electromotive force, which is greater while the arc is silent than when it is hissing.

MM. NACCARI AND PAGLIANI have lately determined the vapour tensions of a number of liquids in the laboratory of the University of Turin. Their method consisted of a modification of that of Regnault, reduction of pressure being effected by an aspirating pump. The tensions of toluene, propylic and iso butylic alcohol, and of several of the ethers of the fatty acids were determined at different temperatures with great exactitude and their empirical formulæ calculated.

FROM a study of the electromotive force of inconstant couples MM. Naccari and Guglielmo conclude that in couples containing one fluid the electromotive force is influenced by the nature of that pole to which the hydrogen goes, and that the change in the strength of the current varies always in the opposite sense to that of the electromotive force, the sense depending upon the manner in which the liberated oxygen enters into secondary chemical actions.

AT the Observatory of Campidoglio, Prof. Respighi has been lately conducting a series of experiments for the determination of gravity. The data are not as yet fully reduced, but the author has described his method (*Atti della R. Acc. dei Lincei*, vol. v. fasc. 5), which consists in the use of a pendulm with a lead ball about 9½ kg. in weight, and a steel wire 0·6 mm. in diameter; a sharp iron point at the extremity, dips in mercury each oscillation, so as to give passage to the current of a chronograph. Five different lengths of pendulum were nsed, between 7·90 m. and 5·16 m.; and with all these lengths the pendulum, on account of its weight, the fineness of the wire, and the convenient mode of suspension, proved independent of the rotatory motion of the earth, presenting Foncault's well-known phenomenon (an essential condition, in the anthor's opinion, but not verified in Borda's or Bessel's apparatus). The number and duration of the oscillations were registered by the chronograph with greater exactness than is attainable by the method of coincidences.

AN arrangement for rendering Volta's pile constant and depolarised is described by Count Mocenigo in a recent number of the *Rivista Scientifico-Industriale*. Twelve couples with their elements are fixed on a horizontal axis; a trough of acidulated water having twelve compartments is brought up by a lever motion, so as to cover a good third of the surface of the pile, and a rotatory movement is communicated to the axis.

THE velocity of sonnd in chlorine has been determined lately by Prof. Tito Martini (*Riv. Sci. Ind.*, No. 6), no physicist having previously, to his knowledge, done so. His method was suggested by an experiment of Tyndall. A glass tube 40 ctm. long and 2 ctm. internal diameter, and fixed in vertical position, was connected below, by means of a gutta-percha tube, to another glass tube holding sulphuric acid, and capable of being raised or lowered so as to vary the level of the liquid entering the fixed tube, in order to obtain the column of gas which would strengthen a certain tone. The fixed tube was graduated in centimetres and millimetres. Having first verified the accuracy of the method by experiments with carbonic acid and protoxide of nitrogen, the author proceeded to chlorine, and obtained 206·4 m. as mean value of the velocity of sound in it for zero temperature.

THE mode of decomposition of water by discharge of Leyden jars through platinum electrodes has been studied by Dr. Streintz (Vienna Acad. *Anz.*). Riess attributed this phenomena to heating of the electrodes. Using a quadrant-electrometer, &c., Dr. Streintz found that with very small electrodes giving passage to a series of discharge-currents in one direction, then left to themselves, a remarkable reversal of electromotive force occurred, but only when the discharges did not exceed a certain number. The author was led to examine the change of electromotive force by short galvanic currents, which also produce, in a few minutes, a reversal in the electric behaviour of the electrode covered with H_2; and he explains this by saying that platinnm containing no free, but only occluded, hydrogen is electromotively negative to pure platinum. The further observation that a fully-polarised cell, one of whose electrodes was covered by a very brief galvanic current with H_2 the other with O_2, did not show a reversal of the difference of potential, led to the conclusion that the decomposition through battery discharges is to be regarded as the prodnct of a galvanic polarisation and a connected (thermal ?) development of oxyhydrogen gas on the two electrodes.

IN a recent note to the Vienna Academy Prof. Reitlinger and Dr. Wächter distinguish three varieties of Lichtenberg figures: (1) the positive radiating figure (*Strahlenfigur*); (2) the positive disk-figure; (3) the negative disk-figure. The (2) was lately added by Herr Holtz. The conditions of production in each case are investigated. The positive radiating figure is produced (according to the authors) by dust particles detached and carried off from the electrode; the negative disk-figure, on the other hand, by gas-discharges. In the former case the particles, while they communicate their positive electricity to the resin, describe radial paths rendered visible and yellow by the dusting process. The reason why one never gets a negative (red) radiating figure, or even branch, is that the electro-negative discharge from a metal or other conductor in air is neither capable of effecting an electric disaggregation of the electrode, nor a carrying away of dust-particles.

To obtain an enlargement (on a screen) variable at will, at any distance, M. Crova (*Journ. de Phys.*, April) places between the object and the screen (whieh are fixed) a projection-apparatus formed of two lenses, one convergent (piane-convex), the other divergent (plane-concave), of the same focal distance, and capable of being moved apart by means of a rack and pinion arrangement.

GEOGRAPHICAL NOTES

MR. JAMES GLAISHER writes from the office of the Palestine Exploration Fund, announcing the discovery of a "Hittite" City.—"A great battle," he states, "figured in Sir G. Wilkinson's 'Ancient Egyptians,' was fought between Rameses II. and the Hittites near their sacred city of Kadash, which is shown as a city with a double moat, crossed by bridges beside a broad stream running into a lake. The lake has been generally identified with the Baheiret Homs, through which the Orontes passes south of Homs, but the site of the city, as important in Hittite records as the northern capital of Carchemish, remained to be discovered. We now learn from a despatch received from Lieut. Conder, the officer in charge of our new expedition, that he has identified the lost site with the ruins known as the Tell Neby Mendeh. They lie on the left bank of the Orontes, four English miles south of the lake. The modern name belongs to a sacred shrine on the highest part of the hill on which the ruins lie, and the name of Kadesh still survives, so that here is another instance of the vitality of the old names which linger in the minds of the people long after they have forgotten the Roman, Greek, or Crusaders' names. Not only the name is preserved, but the ancient moat of the city itself. Lieut. Conder writes:—'Looking down from the summit of the Tell we appeared to see the very double moat of the Egyptian picture, for while the stream of the Orontes is dammed up so as to form a small lake fifty yards across on the south-east of the site, a fresh brook flows in the west and north to join the river, and an outer line of moat is formed by earthen banks, which flank a sort of aqueduct parallel with the main stream.'"

THE French Government is taking advantage of the occupation of a part of Tnnis to extend their ordnance survey to regions hitherto untrodden by ordinary travellers. Col. Perrier, the member of the Institute who is at the head of the French Survey, has been ordered for this service.

THE death is annonnced of Gessi Pasha, the friend and eoadjutor of Col. Gordon in the Sudan. He died on the evening of April 30, in the French hospital at Suez, after protracted sufferings eaused by the terrible privations he endured in the months of November and December last, when he was shut in by an impassable barrier of weed in the Bahr-Gazelle River, Upper Egypt, as already recorded. Capt. Gessi conducted some valuable exploring work on the Nile under Col. Gordon, and in 1876 succeeded in circumnavigating Albert Nyanza, adding greatly to our knowledge of that lake.

IN the *Revue Scientifique* of May 14 M. G. Rolland has a long article on the Sand Dunes of the Sahara, in which he adduces data to show that these dunes shift but very little, that although they move towards the south-east, it is very slowly, and that little difference is made upon them in the course of a generation.

"CAMEOS from the Silver-Land," by Mr. E. W. White, F.Z.S., will shortly be issued in two volumes by Mr. Van Voorst. It relates to the author's experience in the Argentine Republic, and will be specially full on the natural history of the country.

WE regret to learn the death of Admiral La Roncière le Noury, president of the Geographical Society of Paris, who died on Saturday after a protracted illness. He was born in 1813. In 1856 he went to the Arctic Ocean in the *Reine Hortense* on a scientific exploration professedly conducted by Prince Napoleon, who was on board. On the death of M. Chasseloup Laubat the Admiral was elected president of the Paris Society of Geography, in which office he continued without opposition up to the last election. The Admiral took great interest in scientific geography, as well as in zoology and botany.

DR. GERHARD ROHLFS, who has been travelling in Abyssinia, has returned to Berlin.

THE HYPOPHYSAL GLAND IN ASCIDIANS

SINCE the publication of Kowalewsky's remarkable discovery of the course of development in Ascidians, and its confirmation, in all the leading features, by Kupffer and others, any morphological work on the Tunicata is naturally regarded with great interest on account of the possibility of its throwing light on the difficult problem of the relationship of that group to the Vertebrata.

Embryological investigations have clearly demonstrated that the fully-formed larval Ascidian (in most genera, at least) possesses an axis occupying the centre of the tail, and comparable with the vertebrate notochord; that the dorsal region of the body contains a neural canal—of epiblastic origin, and formed by the rising up, arching over, and coalescence of "laminæ dorsales"—expanding anteriorly as a vesicle, in the walls of which certain sense-organs are developed, and being continued posteriorly as a fine canal running along the tail on the dorsal surface of the notochord. The ventral region of the body is occupied by the alimentary canal, lying below the nerve vesicle, and, in its most posterior prolongation, below the anterior extremity of the notochord, which in this locality separates the neural and visceral canals. These developmental researches have also shown that in the adult Ascidian the branchial aperture must be regarded as homologous with the vertebrate mouth, and the branchial sac with the pharynx.

An excellent paper by M. Charles Julin [1] in the last number of the *Archives de Biologie* (tome ii. fascicule i., 1881), of which a preliminary account appeared lately in the *Bulletin* of the Académie Royale de Belgique (3me ser., t. I, No. 2, Fevr. 1881), adds to this interesting list of homologous organs by showing strong grounds for the belief that the little-understood "neural gland" in the Ascidians represents the glandular portion of the hypophysis cerebri, or pituitary body of Vertebrates.

M. Julin gives a minute account of the structure and relations of the peripharyngeal bands, the dorsal lamina, the nerve ganglion, and that enigmatical organ generally known as the olfactory tubercle; the most important section of his paper, however, is that dealing with the neural gland. This structure was first discovered by Hancock, and more recently its glandular nature was demonstrated by Ussow, who called it the olfactory gland, and stated that it was connected with the olfactory tubercle by a narrow canal, an observation since confirmed by Nassonoff. Julin contends that the so-called olfactory tubercle is not a sense-organ at all, but merely the curiously complicated opening into the pharynx of the duct of his "hypophysis." He states that he has been unable to find any nervous connection whatever between the tubercle and the ganglion, and that the nerve which has frequently been observed and described as supplying the supposed sense-organ really passes behind it without communicating, and that therefore he cannot confirm the innervation described and figured by Ussow. The histological structure of the tubercle is also opposed to the probability of its sensory function, as no modified cells are present, the whole surface being covered by normal ciliated columnar epithelium.

The reasons which M. Julin advances in support of the homology of the neural gland with the pituitary body are its structure, its position on the ventral surface of the ganglion, and its relation with the pharynx. The glandular nature of this body was first shown by Ussow, and its minute structure has been investigated by Julin. It consists of branching glandular tubules surrounded by connective tissue richly supplied with blood-sinuses, while the excretory duct in its posterior part has a complete dorsal wall only, as ventrally it communicates freely with the ends of the tubules, just as is the case with the duct during the development of the pituitary body.

Julin points out that in the Ascidians the duct, in running anteriorly towards the olfactory tubercle, is in direct relation with the ventral surface of the nerve ganglion, no layer of connective tissue intervening; and this he states is also the case in Vertebrates.

The position of the neural gland, or "hypophysary gland," as Julin proposes it should be called, is constant. Wherever the nerve-ganglion may be,—and it varies considerably in its position in different species,—the gland is always situated on its ventral surface.

The excretory duct arising from the dorsal surface of the gland, runs anteriorly, directly below the nerve-ganglion, to the olfactory or hypophysary tubercle, where it communicates with the pharynx, probably within the region formed by the epiblast involved in the oral invagination.

It is evident that Julin's observations throw the gravest doubts on the always somewhat questionable olfactory nature of the dorsal tubercle. A ciliated pit having no apparent nervous relations, and connected by a duct with a body having a well-marked glandular structure, has no claim to be regarded as a sense organ. Its function, and that of the gland, remain a mystery; Julin states that he is unable to throw any light upon this question. From the large size of the gland and the constant presence and usually extraordinary complication of the tubercle one would imagine that they performed an important function in the economy of the Ascidian; but what that function is, and why the duct of a gland should have so elaborate an opening into the pharynx, are at present totally unknown.

Julin gives us no information as to the development of these organs. In 1871 Kowalewsky [1] described, in the course of the development of *Ascidia mammillata*, the formation of an aperture connecting the anterior end of the nerve vesicle with the region of the epiblast which was being invaginated to form the oral funnel, and he declared that this aperture of communication between the neural and visceral canals persisted in the adult as the ciliated tubercle. Kupffer,[2] in the following year, while referring to Kowalewsky's statement, declared that he had been unable to discover any such aperture in the larva of *Ascidia mentula*. If Kowalewsky's observation is confirmed, and if the canal is found to remain as the duct of the neural gland, the course of its development would seem to differ considerably from that of the hypophysis cerebri as described by Mihalkovics, Balfour, and Kölliker, which are the views approved of by Julin and confirmed from his own observations.

In conclusion, the arguments in favour of the homology of the Ascidian's neural gland with the glandular portion of the pituitary body are very strong. The structure, position, and relations of the two organs are, in a certain stage of development, identical—admitting, of course, that the branchial sac is a modified pharynx, and that the nerve-ganglion is homologous with the vertebrate brain—and the only point required for the proof of the hypothesis is the demonstration that the neural gland and its duct are epiblastic in formation, and that their development corresponds with that of the pituitary body.

W. A. HERDMAN

[1] "Recherches sur l'Organisation des Ascidies simples—sur l'hypophyse et quelques organes qui s'y rattachent dans les genres *Corella*, *Phallusia*, et *Ascidia*. Par Charles Julin, Assistant du Cours d'Embryologie à l'Université de Liège.

[1] "Weitere Studien über die Entwicklung der einfachen Ascidien" (*Arch. f. microsc. Anat.*, vol. vii.).

[2] "Zur Entwickelung der einfachen Ascidien" (*Arch. f. microsc. Anat.*, vol. viii. 1872).

STORING OF ELECTRICITY

SECONDARY batteries to store up currents of electricity in the form of chemical work promise to play so important a part in the ultimate adoption of the electric light, that improvements in their construction are of peculiar interest. The latest innovation is due to M. Faure, who has modified with great success the secondary battery of Gaston Planté by covering the surfaces of the lead plates with a coating of minium, thereby increasing their capacity manifold. This device possesses the additional advantage that it obviates the necessity of "forming" the cells by the tedious process of charging and discharging them for many days, as in Planté's batteries. Two sheets of lead are separately coated with minium and are rolled together in a spiral, being kept apart by a layer of felt, and are then placed in a

vessel containing dilute acid. When a current is passed into this cell the minium on one plate is reduced to metallic lead that on the other is oxidised to the state of peroxide. These actions are reversed while the charged cell is discharging itself. According to M. Reynier one of these cells made large enough to weigh 75 kilograms may store up energy sufficient to furnish afterwards one-horse power of work for an hour.

A correspondent of the *Times* of Monday gave an interesting account of an experiment he witnessed in Paris of storing electrical energy by the method adopted by M. Faure.

"A Faure battery, or *pile secondaire*," he states, "was charged with the electric fluid direct from the ordinary Grove battery and in my presence. It may be more economically done from a Gramme or Siemens machine. The receptacle consisted of four Faure batteries, each about five inches diameter and ten inches high, forming a cylindrical leaden vessel, and containing alternate sheets of metallic lead and minium wrapped in felt and rolled into a spiral wetted with acidulated water, and the whole placed in a square wooden box measuring about one cubic foot and weighing some seventy-five pounds. This was protected by a loose wooden cover, through which the electrodes (in lead) protruded, and were flattened down for convenience of transport. This box of 'electric energy' was handed to me by M. Faure at my request, with the object of submitting it for examination and measurement to our eminent electrician, Sir William Thomson, F.R.S., at the University of Glasgow. I had the box by me all through the journey from Paris on Tuesday night (last week), including a five hours' delay at Calais. I arrived at Charing Cross at 11 a.m. on Wednesday, after running the gauntlet of customs and police authorities, who suspiciously looked askance and seemed to doubt my statement that my box only held 'condensed lightning,' and contained no infernal machine or new explosive destined to illustrate some diabolical socialistic tragedy. From time to time on the journey I tested the force of the discharge and found it to have well maintained its energy. From London to Glasgow required only another ten hours, and finally, in about seventy-two hours from the time of charging in Paris, I had the satisfaction of presenting to Sir William Thomson M. Faure's rare offering of a 'box of electricity,' intact and potent, holding by measurement within that small space of one cubic foot a power equivalent to nearly one million of foot pounds! This wonderful box is now deposited in the laboratory of the Glasgow University, under the vigilant eye of its director, and being submitted to a series of tests and measurements, the results of some of which made Sir William exclaim, 'Why, it's a little witch.'" With reference to this Sir Wm. Thomson writes to us under date May 17:—"I had the marvellous box under trial for seventy-two hours before I left Glasgow yesterday, giving it successive charges, and discharging to various degrees, measuring approximately the whole quantity sent in during the charge, and taken out in the discharge. Thus I shall be able to calculate the amount of energy spent, and the amount recovered under various conditions. Mr. J. T. Bottomley continues the trials in my absence. A considerable time must pass before I have results to publish."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The report of the Botanic Garden Syndicate, as it now stands, recommends the admission of members of the Senate into the Garden on Sunday afternoons from three to six during the present summer, and as an experiment only; three friends may be introduced at the same time, their names being written in a book. Only one entrance is required to be opened, and the curator or his deputy and one policeman are to be present. The number of signatures of residents in favour of this change is very large. Prof. Babington objects, and so do the heads of Queen's, Pembroke, and St. Catherine's Colleges, Professors Cowell and Westcott, and several resident clergymen.

At Trinity College W. R. Sorley (second year) has been elected to a Foundation scholarship for Moral Science, and D'Arcy Thompson (first year) to a Scholarship for Natural Science; E. D. Ritchie (Winchester) and W. B. Ransom (Cheltenham) to Exhibitions of 40*l.* for Natural Science. At King's College S. F. Harmer has been awarded the Vintner Exhibition for Natural Science, and A. P. Laurie (Edinburgh Academy) an Exhibition of 50*l.* for two years.

Mr. Lea is lecturing, in Dr. Foster's advanced course, on the Physiology of Vision.

Mr. Hicks is taking an examination class in Elementary Botany at Sidney College.

Dr. Vines' course of botany this term is one of Morphology, chiefly cryptogamic, with practical work.

The first M.B. Examination commences on June 13, the second on June 7, the third on May 10; the M.C. Examination on June 13.

The open mathematical lectures this term are those of Mr. Dale (Trinity) on Heat, and Mr. Taylor on Higher Plane Curves, Mr. Besant (St. John's), on Sound and Vibrations, Mr. Webb (Emmanuel) on the Potential and Green's Theorem, and Mr. Temperley on Finite Differences.

The first part of the Natural Sciences Tripos begins on June 6.

IN the report of the last Local Examinations (December, 1880) it is stated that the juniors answered satisfactorily in Botany, while the descriptions of specimens by the seniors and their answers in Vegetable Physiology were very weak. In Zoology the seniors did better relatively than the juniors, but practical work was largely deficient. In Geology the answering was bad, and the practical knowledge of specimens extremely meagre.

LONDON.—At the presentation day last week at the University of London, when the certificates of degrees and honours won by the successful students at the late examinations were distributed, three ladies received certificates of matriculation, and four degrees of B.A. Earl Granville said that this year they had lost by death Sir Philip Egerton, a man of great cultivation, who had always shown the greatest interest in the work of the University. There were other losses which they regretted, but which carried some consolation with them, as being highly to the credit of the University—as, for instance, the departure of Dr. Greenfield, who had for so long been identified with the Brown Institution, to Edinburgh University. They were perhaps aware that in 1852 Mr. Brown had left a sum exceeding 20,000*l.* for the creation of an Institution for the investigation and cure of diseases peculiar to animals useful to man, the donor expressing a desire that the University of London should appoint a committee of their body or of medical men outside to scientifically carry out his views. Ten years ago that institution was established, and during that period few or no cases of interest to it had been discussed in which it had not taken a leading part. These investigations had, he believed, been carried on in a manner which promised the greatest possible advantage, not only with regard to the diseases of animals, but also to those of man. During the past year 3870 animals had been cured, and as an example of the great kindness with which the patients were treated he would relate the following anecdote:—A distinguished member of the Senate was driving along the road in which the institution was situated when suddenly his hack cab came to a dead stop. He asked the driver whether his animal was lame or ill, but the driver answered, "No sir. I never can get him past this place since he had his corn cured here; he likes it so much that he always wants to stop." Results had shown that the University was justified in extending the limits of its operations to the Brown Institution. After careful consideration it had been determined to extend the examinations into the science and art of teaching, for which purpose a scheme had been prepared, which would shortly be carried out. As a member of a Government which adopted as its first principle economy of public funds he was glad to be able to give an instance that this did not always degenerate into niggardly stinginess. Their application to the Treasury for the establishment of a practical museum of natural history to enable them satisfactorily to carry on examinations on their own premises had been most liberally met, and he hoped that in a very short time such a department would be opened.

THE annual distribution of prizes to the successful students at the London School of Medicine for Women took place on Wednesday last week. The report stated that up to the present eighty-six pupils had been received, of whom forty-four are now attending. Nine of these were studying for the University of London, four were amateurs, and the remaining thirty-one were training for examination by the College of Physicians in Ireland. Altogther twenty-five ladies had now been declared qualified to practise. The report of the treasurer, the Right Hon. James Stansfeld, M.P., stated that the expenditure had been 2018*l.*, of which one-half had been provided by the students' fees. The subscriptions had been 626*l.* 17*s.* 6*d.*, as against 723*l.* 15*s.* 6*d.*

last year; and the donations 33*l.* 2*s.*, as against 18*l.* There had been several large legacies realised, amongst them one of 4050*l.* from Mrs. George Oakes.

MANCHESTER.—We understand that a sum of 1500*l.* has been offered by a benefactor to the Council of Owens College for five fellowships of 100*l.* a year, each renewable for a second or third year, the conditions being that they shall be awarded on evidence given by the candidates of their past work in literature or science, and on their satisfying the electors as to their subsequent devotion to original work. The scheme is as yet only under consideration. We likewise understand that Mr. Waterhouse is preparing plans for completing a portion of the buildings required for Owens College, including museums for natural history, geology, and mineralogy, and for the lecture-rooms and laboratories required for the professors of the above subjects.

On Saturday next (May 21) Prof. Boyd Dawkins, F.R.S., will begin the seventh series of Field Lectures in Geology, at Miller's Dale Station, Derbyshire. That and the two following Saturdays will be devoted to the examination of the Carboniferous rocks of the Pennine Chain. On Saturday, June 9, the class will visit the British Museum (natural history) under the guidance of Dr. Woodward, F.R.S., for the study of the mammalia associated with Pleistocene Man. On June 10 the brickfields at Crayford and Erith, in Kent, will be visited under the guidance of Mr. F. C. Spurrell; and on the 11th the subject of the Antiquity of Man will be finished by an examination of the collections of prehistoric archæology in the British Museum (Bloomsbury).

THE Queen has directed letters patent to be passed under the Great Seal granting and declaring that the degrees of Bachelor and Master of Arts and Bachelor and Doctor of Medicine, of Laws, of Science, and of Music, granted or conferred by the University of Adelaide, South Australia, on any person, male or female, shall be recognised as academic distinctions and rewards of merit, and be entitled to rank, precedence, and consideration in the United Kingdom and in the colonies and possessions of the Crown throughout the world, as fully as if the said degrees had been granted by any university of the United Kingdom.

SCIENTIFIC SERIALS

Journal of the Royal Microscopical Society for April, 1881, vol. i. ser. ii. part 2, contains—Prof. P. Martin Duncan, on a Radiolarian and some Microspongidæ from considerable depths in the Atlantic Ocean (plate 3).—Dr. Lionel S. Beale, the President's address.—Prof. E. Abbe, on the conditions of orthoscopic and pseudoscopic effects on the binocular microscope.—A. D. Michael, on a species of Acarus believed to be unrecorded (plate 4).—Prof. E. Abbe, on the estimation of aperture in the microscope. The summary of current researches, pp. 217-364.—Proceedings of the Society. (In the summary of current researches appears a memoir by Mr. Crisp, "On Aperture, Microscopical Vision, and the Value of Wide-Angled Immersion Objectives," in which the whole subject is very exhaustively and clearly put.)

Annalen der Physik und Chemie, No. 4.—Experimental investigation of the connection between refraction and absorption of light, by E. Ketteler.—On the ratio of intensity of the two sodium lines, by W. Dietrich.—On the condensation of gases on surfaces in their relation to pressure and temperature, by H. Kayser.—Influence of pressure on the surface-tension of liquids, by A. Kundt.—Variations of the vapour-density of some esters with pressure and temperature, by P. Schoop.—On differences of tension between liquids touching each other, with reference to concentration, by E. Kittler.—On electric ring-figures and their alteration of form by the magnet, by E. Reitlinger and F. Wachter.—On the divergence of Ampère's theory of magnetism from the theory of electromagnetic forces, by J. Stefan.—On some remarks of Herr C. Neumann on electrodynamics, by R. Clausius.—The law of Clausius and the motion of the earth in space, by E. Budde.—On the extent of the electric expansion in glass and caoutchouc, by Dr. J. Korteweg and V. A. Julius.—The glass plate battery, by Th. Erhard.—Some remarkable properties of flames, by W. Holtz.

American Journal of Science, April.—Monograph by Prof. Marsh on the Odontornithes, or toothed birds of North America, by G. B. Grinnell.—On some elements in orographic displacement, by W. J. McGee.—On the indices of refraction of certain compound ethers, by J. H. Long.—On the Whitfield County, Georgia, meteoric iron, by W. E. Hidden.—The basin of the Gulf of Mexico, by J. E. Hilgard.—On the geology of Florida, by E. A. Smith.—The magnetic survey of Missouri, by F. E. Nipher.—American sulpho-selenides of mercury, by G. J. Brush.—Analysis of Onofrite from Utah, by W. J. Comstock.—Effect of great cold on magnetism, by J. Trowbridge.—Channel fillings in Upper Devonian shales, by H. S. Williams.—A new order of Jurassic reptiles (Cœluria), by O. C. Marsh.—Discovery of a fossil bird in the Jurassic of Wyoming, by the same.—American pterodactyls, by the same.

Journal of the Franklin Institute, March.—Experiments with the Perkins machinery of the *Anthracite*, by Mr. Isherwood.—The wearing power of steel rails in relation to their chemical composition and physical properties, by Dr. Dudley.—Note on steam cylinders, by Prof. Marks.—Novel mode of originating an index wheel, by Dr. Grimshaw.—The polarisation of sound and the nature of vibrations in extended media, by Prof. Robinson.—Gyroscope model for class-illustration, by Dr. Rand.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xiv. fasc. vii.—Grafts of the vine, by Count Trevisan.—On the determination of maximum moments, &c. (continued), by Prof. Clericetti.—On two rare helminths of reptiles, by Prof. Pavesi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 28.—"The influence of Stress and Strain on the Action of Physical Forces." By Herbert Tomlinson, B.A. Communicated by Prof. W. Grylls Adams, M.A., F.R.S. Part I.—Elasticity. "Young's Modulus."

The values of "Young's modulus" were determined for several metals by a method devised by Sir W. Thomson.

A large number of experiments with different loads were made, and after a great many unsuccessful attempts to account for certain discrepancies which could not be explained away as errors of observation, the following facts were elicited:—

1. After a wire has suffered permanent extension, the temporary elongation which can be produced by any load becomes less as the interval between the period of permanent extension and that of applying the load becomes greater.

2. This increase of elasticity is greater in proportion for large loads than for small ones.

3. The increase of elasticity takes place whether the wire be allowed to remain loaded or unloaded between the period of permanent extension and that of the testing for the elasticity.

4. The *rate of increase* of elasticity varies considerably with different metals; with some the maximum elasticity is apparently attained in a few minutes, and with others not till some days have elapsed, iron and steel being in this last respect very remarkable.

5. The elasticity can also be increased by heavily loading and unloading several times, the rate of increase diminishing with each loading and unloading.

6. A departure from "Hooke's law" more or less decided always attends recent permanent extension, even when the weights employed to test the elasticity do not exceed one-tenth of the breaking weight.

7. This departure is diminished very noticeably in the case of iron, and much less so in the case of other metals, by allowing the wire to rest for some time either loaded or unloaded; it is also diminished by repeated loading and unloading.

The effect of permanent extension on the value of "Young's modulus" was tried according to the direct method for iron and copper, and indirectly for most of the metals.

From both the direct and indirect methods results were obtained which showed:—

1. That, in all metals, provided the wire has not been kept heavily loaded for some time before testing, permanent extension produces decrease of elasticity, if the strain be not carried beyond a certain limit.

2. That, if the extension be carried beyond the above-mentioned limit, further permanent increase of length causes increase of elasticity.

3. That, in the case of iron, heavy loading for some time so increases the elasticity that, even when the extension would have caused diminution of elasticity without such continued loading, the latter will, if sufficient time be allowed, change this diminution into an increase; in the case of copper this is not so.

The effect of suddenly chilling steel heated to a high tempera

ture was found to be similar to that of excessive permanent extension of iron.

Several experiments were made to test the effect of permanent torsion and permanent extension on the modulus of rigidity.

From these experiments it was concluded :—

1. That the loss of rigidity produced by twisting or stretching a wire beyond the limits of elasticity is partly diminished by rest.

2. That the loss is more sensible with large arcs of vibration than with small ones.

3. That the influence of rest is more apparent in the case of large vibrations than in that of small ones.

4. That continual vibrating through large arcs has a similar effect on the rigidity to that produced on the longitudinal elasticity by heavily loading and unloading. And—

5. That in the case of hard steel the effect of vibrating through a large arc for several minutes makes temporarily the rigidity as determined from such vibrations greater than that determined from smaller vibrations.

The influence of an electric current and of magnetism on the torsional rigidity of metals was also investigated, and the following results arrived at :—

1. The torsional rigidity of copper and iron is temporarily decreased by the passage of a powerful electric current, but is very little, if at all appreciably, altered by currents of moderate intensity.

2. The torsional rigidity of iron is temporarily diminished to a small but perceptible extent by a high magnetising force.

3. The effects mentioned in 1 and 2 are independent of any changes produced by the current in the temperature of the wire.

Finally, certain critical points are alluded to, there being at least two such for each metal, at which sudden changes take place in the ratio of the permanent extension produced by any load and the load itself.

May 5.—"On the Structure and Development of the Skull in Sturgeons (*Acipenser ruthenus* and *A. sturio*)," by W. K. Parker, F.R.S.

I must refer the reader to Prof. Salensky's[1] invaluable work on the development of the sterlet (Kasan, 1878), unfortunately published in *Russian*, and to the second volume of Mr. Balfour's new work, for an account of the earliest stages of the *Acipenserine* embryo.

Even in larvæ one-third of an inch in length, the cartilage was becoming consolidated, and I was able to work out, by sections and dissections, the structure of the cranium and visceral arches; the one specimen which was seven-twelfths of an inch in length, and which was made into a large number of extremely thin sections, left nothing to be desired.

The development of the skull of the sturgeon is very similar to what we find in the sharks and skates ("Selachians"), but the suspension of both the mandibular and the hyoid arches by one pier, derived from the hyoid (the *hyostylic* skull), which is seen in the Selachians on one hand and in the Holostean Ganoids and Teleosteans on the other, attains its fullest development in the "Acipenseridæ," or Chondrosteous Ganoids; for in them the "symplectic" is a separate cartilage, and not a mere osseous centre as in *Lepidosteus* and the Teleostei.

Here I find a very noticeable fact, namely, that whilst in the salmon the metamorphosis of the simple primary arches of the face can be followed step by step, in the sturgeon the peculiar modification of the arches shows itself *during chondrification;* the hyoid arch, from the first, is inordinately large.

Notwithstanding the huge size of the sub-divided hyoid pier, its head only articulates in the larva with the auditory capsule; later on the basal cartilage reaches it, as in the Selachians.

But the arches that retain their normal size lend no colour to the theory that the visceral arches are related by their dorsal ends to the paired cartilages that invest the notochord, a state of things like that seen in the ribs and in the superficial cartilaginous hoops that surround the huge pharynx of the lamprey.

Mr. Balfour has demonstratively shown that in the branchial region, when the pleuro-peritoneal cavity has been sub-divided by the hypoblastic outgrowths of the pharynx, the aortic arches lie *inside* the small temporary "head cavities," or remnants of the once continuous sub-division of the body wall into an inner layer, the "splanchno-pleure," and an outer layer, the "somato-pleure."

But the aortic arches mount up, on each side, *outside* the proper branchial arches, which become grooved to receive them; these arches must therefore be considered as developments of the temporarily separate "splanchno-pleure"; they cannot be classed with the *costal* arches, which are developed in the permanently distinct "somato-pleure."

My dissections and sections, both of this type and of the Selachians, show, without leaving room for doubt, that all the visceral, or, properly speaking, *branchial* arches, mandibular, hyoid, and post-hyoid (branchial proper) are developed in the outer walls of the large respiratory pharynx, quite independently of the base of the skull and the fore part of the spinal column.

I have at last ceased to contend for true branchial or visceral arches in front of the mouth, and also to look upon the mouth and the openings around or in front of it as more than mere *involutions* of the epiblast; the *first cleft* is that between the mandible and the hyoid arch, the first arch is the mandibular.

With regard to the skull, it is now very evident that the "trabeculæ cranii," even in their furthest growth forwards from the end of the cephalic notochord, are merely *foregrowths* from the moieties of the investing mass (the parachordals), the true axis of the cranial skeleton ending under the fold of the mid-brain. The "cornua trabeculæ," and the "intertrabecular" part or tract, are *fresh shoots*, so to speak of cartilage that are specially developed to finish the cranial box and the internasal framework. I fear that my long-cherished *pre-oral visceral arches* will now have to go down and take their place among these *secondary* or *adaptive* growths.

I may remark, in concluding this very imperfect "abstract," that the sturgeon is a very important type for the morphologist to get clear light upon.

In the Selachians the huge pterygoid foregrowth of the mandibular arch aborts the apex of its pier, whose *function* is supplied by the hyo-mandibular; fragments only are developed in its upper part.

In the sharks from *one* to *three* mere "rays" are developed in front of the small upper remnant of the first cleft ("spiracle"); in skates there is, as a rule, a small separate piece, the true apex of the arch, its "pedicle." In one kind, however, the torpedo, four such fragments appear on each side, as shown by Gegenbaur. In the sturgeon there is a most remarkable plate in the common metapterygoid region, its form is rhomboidal; it is composed of a number of well-compacted pieces of cartilage, a middle series of *azygous* plates, and a somewhat irregular arrangement of plates right and left of these. This remarkable structure only exists in the Acipenseroids; it is not found in *Polyodon*.

In the Selachians the "placoid" plates or spines are not brought under the influence of the chondrocranium, which has neither parosteal plates applied as splints to it nor ectosteal plates grafted upon it.

In *Acipenser* there are both parostoses applied to the oral apparatus, and ectosteal centres in the post-mandibular arches; morever, along the side of the skull, in old individuals, plates of bone appear as splints or parostoses, that are manifestly the forerunners of the deeper plates that in the higher Ganoids and the Teleostei form the proper ectosteal bony centres of the more or less ossified cranial box.

The Ganoid scutes of the sturgeon are so far dominated by the huge chondocranium, that *by courtesy* they may be called frontal, parietal, opercular, and the like; of course such scutes are not the accurate homologues of the bones so-named in the Teleostei, which at the most can only correspond to the inner layer of the scute of such a fish as the sturgeon.

The sturgeons as a group cannot be said to lie directly between any one family of the Selachians and any one family of the Bony Ganoids, yet on the whole that is their position; the Bony Ganoids on the whole approach the Teleostei, especially such forms as *Lepidosteus* and *Amia*, which have lost their "spiracle," and in other things are less than typical, as Ganoids.

Larval sturgeons are, in appearance, miniature sharks; for a few weeks they have a similar mouth, and their lips and throat are beset with true teeth that are moulted before calcification has fairly set in. Their first gills are very long and exposed, but not nearly so long, or for such a time uncovered, as in the embryos of sharks and skates.

Mathematical Society, May 12.—S. Roberts, F.R.S., president, in the chair.—Prof. C. Niven, F.R.S., was admitted into the Society, and the following were elected members :—I. Rosenthal, B.A. Dublin, C. A. van Velzer, F. Franklin, Ph.D., and Miss Christine Ladd, the last three of the Johns Hopkins

[1] My smallest specimens were the gift of Prof. W. Salensky, the larger of the late Mr. William Lloyd.

University, Baltimore.—The following communications were made:—On Ptolemy's theorem, by Mr. Merrifield, F.R.S.—The summation of certain hypergeometric series, by the Rev. T. R. Terry.—Quaternion proof of Mr. S. Roberts's theorem of four co-intersecting spheres, by Mr. J. J. Walker.—Some solutions of the "15-girl" problem, by Mr. Carpmael.—Note on the co-ordinates of a tangent line to the curve of intersection of two quadrics, by Mr. W. R. W. Roberts.—Shorter communications were made by the President, Prof. Cayley, F.R.S., Mr. Hart, and Mr. J. J. Walker.

Entomological Society, May 4.—H. T. Stainton, F.R.S., president, in the chair.—Two new Members and one Subscriber were elected.—Mr. R. Trimen made some observations on the sexes of *Pieris saba*, *Diadema mima*, and *Papilio cenea*, and exhibited specimens in illustration. He also remarked on *Tinea gigantella* having been bred from the hoof of a dead horse, and on the uncertainty which still exists as to whether the larva of this species ever feeds on living horn or not.—The Secretary read a letter from the Colonial Office respecting the occurrence of *Phylloxera vastatrix* on vines in Victoria.—Mr. A. G. Butler communicated "Descriptions of New Genera and Species of Heterocarous *Lepidoptera* from Japan—*Noctuites*."

PARIS

Academy of Sciences, May 9.—M. Wurtz in the chair.—The following papers were read:—Reply to some criticisms of the note of February 21, on the parallax of the sun, by M. Faye. He invites his English critics to correct his ten numbers according to their best information, and expects they will reach nearly the same result.—On nitrate of diazobenzol, by MM. Berthelot and Vieille. This solid crystalline body ($C_{12}H_4N_2NO_6H$) detonates with extreme violence when heated above 90° (and is thus much more sensitive to heat than fulminate of mercury). It also detonates when struck with a hammer or rubbed. It is now much used in making colouring matters.—On a new derivative of nicotine obtained by the action of selenium on this substance, by MM. Cahours and Étard. The collidine obtained is one of the propylpyridines corresponding to the isomeric position, still unknown, of nicotianic acid. Theory anticipates six collidines of this species. Selenium is found to be capable of removing nitrogen from an organic substance.—On the divisors of functions of periods of primitive roots of unity, by Prof. Sylvester.—On the densities of liquefied oxygen, hydrogen, and nitrogen in presence of a liquid without chemical action on these simple substances, by MM. Cailletet and Hautefeuille. The mixture was chiefly with carbonic acid. The density varied considerably with temperature and pressure. The coefficients of dilatation are so little different that the densities must be sensibly in the same ratio at 0° and at − 23°. The atomic volumes calculated are 17 for O, 30·3 for H, and 31·8 for N (dividing each of the atomic weights by the density at − 23°, viz. O, 0·89, H, 0·033, and N, 0·44). Gaseous O, H, and N diverge very unequally from Mariotte's law, at the pressures employed (275 and 300 atm.), and there is not then a simple relation between the atomic weight and the density; but on change of state by lowering of temperature in presence of a gas easily liquefiable, the atomic volume is found to reveal a positive relation between density and equivalent weight.—M. de Gasparin was elected Correspondent in Rural Economy, in room of the late M. Kuhlmann.—On displacement of a figure of invariable form in its plane, by M. Dewulf.—On the work-product of secondary batteries, by M. Regnier.—M. Mascart stated that Admiral Cloué, Minister of the Marine, would probably organise an expedition to islands near Cape Horn, taking part in the international scheme of simultaneous observations on terrestrial magnetism, &c.—On seeds of two species of Chinese vines discovered in 1872 by Abbé David in the province of Chen-si, by M. Du Caillaud.—M Vinot submitted a telescope made on a suggestion of M. Caussin. The image formed by one telescope is looked at with another of the same or different power.—Observations of Saturn's satellites at Toulouse in 1879 and 1880, by M. Bailland.—Observations, elements, and ephemerides of the comet *a* 1881 (discovered by Mr. Lewis Swift on April 31), by M. Bigourdan.—On a system of differential equations, by M. Halphen.—On trilinear forms, by M. le Paige. On some actinometric measurements made in the Alps in 1880, by M. Puiseux. The total radiation (that diffused by ground and sky as well as that direct from the sun) was found to be increased 0·10 at an altitude of 800 m. and 0·21 at 2100 m. At greater heights (3380 and 3251 m.) the numbers were much higher, but less easy of interpretation, because of snow and mist; the reduced figures were 1·25 and 1·24 (showing good agreement with the others). Phanerogamic plants are found up to 3900 m., and must accomplish all their phases in the three summer months at a temperature below that of a polar summer. Doubtless the intensity of radiation compensates.—Action of light on phosphorescent substances, by M Clémandot. He notes the confirmation, by M. Yung of Geneva, of his views that the phenomenon is physical, and the vibratory influence strongest in the blue ray. M. Becquerel called attention to his own researches thirty years ago.—Action of light on bromide of silver, by M. Noel. *Ceteris paribus*, silver bromide retains longer the molecular modification impressed on it by the chemical spectrum, the greater its sensibility, and when this first modification disappears it seems to have recovered its initial sensibility.—Action of carbonic acid on baryta and strontium, by M. Raoult.—On the products of action of perchloride of phosphorus on acroleine, by M. van Romburgh.—On the nature of the troubles produced by cortical lesions of the brain, by M. Couty. He rejects the theory of localisations, both on anatomical and physiological grounds.—On the poisonous action of the juice of manioc, by M. de Lacerda. It is not great, and it seems to affect the central nervous system.—On the *rôle* of marine currents in geographical distribution of amphibian mammalia, particularly Otaria, by M. Trouessart. These animals seem to have radiated from Antarctic regions. Their course to the North Pacific, &c., corresponds remarkably with that of certain currents.—Movements of juices and various plant-organs referred to a single cause; variations of hydrostatic tension, by M. Barthélemy.

VIENNA

Imperial Academy of Sciences, May 12.—V. Burg in the chair.—The following papers were read:—C. Claus, on tomora and tomorella.—Prof. L. Ditscheiner, on searching for spots of interruption at insufficiently insulated circuits.—E. Sathey, on the phenomena of exsiccation and imbibition on the involucra of Cymareæ.—Dr. R. Maly, on yolk-pigments.—E. Weiss, on the comet discovered by Lewis Swift (Rochester, U.S.) on May 2, &c.—E. Weiss, on a new method of computation of the apparent anomaly in orbits of great excentricity.—Dr. Zd. Skraup, on cinchonidine and homocinchonidine.

Imperial Institute of Geology, May 3.—The following papers were read:—Prof. Cornel. Doelter, on the geological state of the Cape Verde Isles.—M. Vacek, exhibition of the geological map of Trieste.—Dr. L. Szaynocha, on the occurrence of petroleum at Sloboda Rungurska.—Dr. E. Hussak, on the inclusion of resinous matters in the pycrite porphyry of Steyrdorf.

CONTENTS PAGE

THURSDAY, MAY 26, 1881

THE EVOLUTION OF THE CRYPTOGAMS

L'Évolution du Règne Végétatale. Les Cryptogames. Par MM. Saporta et Marion. Bibliothèque Scientifique Internationale, xxxi. (1881.)

THIS exceedingly valuable contribution to the history of the evolution of the vegetable kingdom, to be followed by a second volume dealing with the Phanerogams, is fully illustrated by 85 figures.

It is almost superfluous to remark on the exceptional qualifications with which the authors enter upon the task, for they have already produced some of the ablest works, particularly upon fossil plants. Although much of the material they have had to deal with has not been more than usually satisfactory, their work has been singularly free from the arrogance of other writers on the subject of fossil plants, who pretend to a clairvoyance enabling them to determine unhesitatingly even fragments of leaves of extinct trees when every organ necessary for botanical determination is absent. When, as in the great majority of cases, subsequent discoveries prove these gentlemen to be wrong, we hear nothing from them, but when their guesses prove right, they are exceedingly jubilant.

At the outset the authors lay some stress on the fact that a less complex organisation does not necessarily imply relative antiquity. Circumstances exceptionally favourable to certain series of plants have forced their development to a state never afterwards surpassed, but which, on the contrary, has retrogressed by the elimination of hastily-developed or prematurely-adopted types. New series or new branches given off by the same series have constantly replaced, in all ages, the series or the branches which have died out or declined, and the vegetable kingdom, taken as a whole, has constantly progressed. The book, moreover, is not written for those who totally disbelieve in the principles of evolution, for no proof that it contains will convince them; but those who wish to understand the successive modifications that have led to the comparatively recent group of Angiosperms, will find it full of interest.

Commencing with the Protista, the authors lead us through the Protophytes to the Lower Metaphytes, which together constitute the artifieial group of Cryptogams. These represent an elder branch of the vegetable kingdom, and lead, by perfectly natural transitions, to the Phanerogams, the younger branch, of which the latter offshoots appeared only at long posterior dates.

The origin of all plants is in protoplasm, and those of the Protista which are amorphous, yet possess the essential attributes of life, may well be thought to reproduce the probable characteristics of the earliest primordial plants. On the southern shores of France creatures several centimetres in length are dredged from depths of five to ten fathoms, whose substance is entirely penetrated with fine particles of the sea bottom. They would pass unnoticed did they not shift their position with extreme slowness and extend short prolongations. Placed in a glass of sea-water they attach themselves to the sides, and free themselves gradually of sand, when a slightly yellow hyaline jelly, absolutely deprived of nucleary elements, is disclosed. They are allied to the Protamœba, Protobathybius, and Pelobius, and from these starting points all the progressive stages of development are traced.

In certain organisms among the Protista the protoplasmic mass secretes a rigid envelope, and when, further, a portion of the protoplasm becomes converted into another substance, "chlorophyll," all the characters of vegetable life are realised. In the interior of these cells the protoplasm remains truly amœbous, and acts and is acted upon in precisely the same manner as in animal amœbæ, but this special substance chlorophyl gives rise to a whole series of new physiological functions, and its presence alone marks off animal from vegetable life. The only distinction that can be drawn between the two kingdoms is thus entirely due at the commencement to a transformation of part of the elementary protoplasm.

Leaving the Protista, the authors treat at some length the embryogeny and methods of reproduction of Protophytes, especially the Algæ, tracing these through the primitive and single-celled diatoms and desmids, with soft or hard envelopes containing protoplasm charged with chlorophyl, to the higher forms in which special organs are developed, as the Florideæ, Fucaceæ, &c., and the Characeæ. Setting aside the Fungi and Lichens as groups whose development has been arrested by parasitic habit, the authors proceed to consider the manner in which aquatic vegetation became first adapted to terrestrial life.

While the more highly organised and complex Algæ have preserved those aquatic habits necessitated by physiological functions, numerous species of Nostochineæ, Palmelleæ, and Vaucheria have quitted the water from time to time to vegetate in humid places on land. These furnish the earliest indication of adaptability to aërial life, and it is curious to find that this proceeds from lower forms of Algæ but slightly differentiated from each other morphologically, and not from the more completely evolutionised types. Some, with flat cellular fronds, such as Ulva, crept, it is supposed, face to the ground and became ancestors of the Hepaticæ. Others, more confervoid, produced a thallus whose growth, necessarily apical, became complex by simple vegetative multiplication. Foliary appendicles were given off, and a sort of plantlet with rootlets, stem, and leaves, all strictly cellular, came into existence, capable, like the Mosses at the present day, of agamous reproduction. In the earliest stage of growth of the Equisetaceæ, of Ferns, and of Ophioglosseæ, we see a similar primordial cellular plant, called a Prothallus, develop from the spore, and resembling in every respect the lower Algæ. This prothallus bears the sexual organs, and it may here be noticed that it is impossible to insist too strongly on the influence exercised by the act of reproduction on the differentiation of primordial plants.

In lowly developed types the act of reproduction arrests what may be termed their nutritive life. This act may be "precocious" or "tardy," the variations in the time of sexuality exerting a dominant influence on the morphologic differentiation of life. In the well-known case of the Axolotl the embryos of the same birth may either have well developed or only rudimentary sexual glands. In the former case the fry reproduce precociously before losing their branchiæ, while those which reproduce more

tardily become transformed into "Amblystomes," their morphologic differentiation being unchecked by the act of reproduction. Similarly among primitive terrestrial Algæ, those in which sexuality was deferred until late had a longer period of purely vegetative life, and therefore not only felt more strongly the influence of new conditions, but had a longer time in which to adapt themselves and thus become diversified in type. The resultants of this elaboration are represented now by the Mosses and Hepaticæ. In the Mosses the spore gives birth to a confervoid thallus called "protonema," a reversion apparently to the primitive ancestral Alga. This elementary thallus, not being arrested by the appearance of sexual organs, is susceptible to subsequent differentiation; foliary buds are given off in places from its ramifications, the multiplication of cells at these points becomes regular, and little by little small laminæ assume the form of leaflets on a stem supported by radicles. These radicles are capable of producing new plants, and mosses propagate so energetically that extensive carpets may be formed without the aid of reproductive organs, and in some species fruiting plants are rarely met with. This great vegetative power seems entirely due to the absence or rarity of sexual function. The reproductive organs when present are however of the greatest morphological importance. These are distinguished as "antheridia," or male, and "archegone," or female. At maturity antherozoïds escape from the antheridia and impregnate the archegone. The "oospore" contained in the archegone produces a new cellular plant, which develops more or less within the archegone in which it is born, and finally becomes the organ called "fruit" in the Mosses. This so-called fruit is in reality a distinct plantlet, called a "sporogone," which by asexual generation or simple multiplication gives birth to the spores, and these spores, falling in damp places, again give rise to new thalli or moss plants. This alternate generation is unknown among Algæ. We have thus in the Mosses a new point of departure, the more important generation, being analogous to Algæ and tardily sexual, take on very complete morphological characters; the other generation, agamous, subordinate, and incapable of disengagement from the archegone in which it is formed, yet fundamentally an independent plant. The Hepaticæ are similar in growth, and both together present a stationary group which have elaborated a special kind of organic differentiation, but in a direction limited by biologic conditions. Derived from cellular thalli with "tardy sexuality," evolution has acted exclusively on the first generation; while the second, of newer origin and free from heredity, would have been susceptible of far more complete differentiation. The truth of this hypothesis becomes apparent when Ferns, Equisetaceæ, and Ophioglosseæ are studied.

The origin of these three groups is similar to that of the Mosses and Hepaticæ. Their spores give birth to a cellular thallus or "prothallus," which "precociously" produces numerous archegones and antheridia. The same process takes place as in the case of Mosses, except that the resulting "sporogone" is vigorous and speedily effaces the ephemeral life of the sexual plant. It promptly frees itself and takes root, its tissues become extremely diversified, and fibres and vessels, histological elements previously unknown, appear, and plants known as ferns, horsetails, &c., result. On the leaves of this highly-developed sporogone the sporangia are born which produce the spores, whose germination gives birth to the sexual prothallus. The precocious and abundant development of sexual organs almost immediately arrests the differentiation of the prothallus, and the primordial aërial Alga becomes completely subordinate. On the other hand the sporogone which succeeds became more and more developed and commenced a series which step by step has led finally to the most highly organised and most recent group of plants, the Angiosperms. The evolution which has given us those plants, which seem to an inattentive observer to form nearly the entire vegetation of the earth, is in the authors' opinion the result of a circumstance, doubtless almost insignificant in its commencement, and of which the first effects were to arrest by a precocious sexuality the organic differentiation of some of the primordial terrestrial plants. While everything seemed to unite to favour the evolution of those types with permanent thalli, and which produced Mosses and the Hepaticæ, other thalli of lower development found in the very causes which limited their differentiation, the starting point of a new vegetative system, that of the sporogone, the preponderance of which soon became manifest. In the Rhizocarps we see this species of development in a more advanced stage than in the Ferns. The sporogone has become more and more preponderating, and the prothallus scarcely disengages itself from the envelopes of the spore.

Ferns occasionally exhibit a tendency to a separation of the sexes, for the prothallus may be either male or female, but in the Rhizocarps diœcity is more nearly realised, for the spores themselves are of two sexes—microspores and macrospores. The germination of the microspores consists simply in the production of tubes scarcely divided into cells, in one of which the antherozoïds are produced. In the macrospore, though a rudimentary prothallus is at first more or less apparent, this is quickly concealed by the extension of the sporogone developed within one of the archegones. With the disappearance of the rudimentary prothallus almost the last trace of the primordial cellular Alga disappears.

The prothallus is thus seen to be so reduced in the Rhizocarps that it seems almost as if the sporogone were disengaged directly from the macrospore. This sporogone follows otherwise the same histological development as in Ferns, but gives birth morphologically to a further departure. Certain fronds become differentiated into "sporocarps," a kind of fruit comprising both micro- and macrosporangia, and which in *Marsilia* attain remarkable complexity. This is the highest point of evolution seen in existing Cryptogams, for the Lycopods are rather a parallel development than an actual advance beyond the Rhizocarps. They are divided into Isospores, or true Lycopods, in which the sporogones bear but one kind of spore, producing monœcious prothalli only; and the Heterospores (Selaginellæ and Isoetes), in which the sporogone bears both microspores and macrospores.

In the microspores of heterosporous Lycopods a single cellule represents the male thallus, and appears a useless appendicle to the antherozoïd-producing cellules. The macrospore germinates into two cellular masses, corresponding to the female thallus, which, although never

entirely disengaged from the envelopes of the spore, still produces true archegones destined to receive the impregnation of the antherozoïds.

In these, as in all the Metaphytes of which we have been speaking, the spores become detached before germination. While this caducity always characterises the microspore, the macrospore separates less readily from the sporogone, and the method in which the sexes in primordial plants became separated is doubtless indicated by this tendency. The microspore always represents the male and the macrospore the female thallus, the physiological functions which they have to effect being very different. Activity characterises the male element, which always seeks the female element, necessarily more complex, voluminous, and charged with plastic substances. It is easy to conceive the possibility of the existence of a stage, a little above the existing heterosporous Lycopods, in which the microspores alone become detached before germination, and seek the macrospores while still attached to the fronds of the sporogone, which would then germinate on the plant and receive impregnation before their fall. It is true that we can say nothing definitely as yet respecting the extinct allies of the Lycopodiaceæ, which may have possessed this character, but the course of evolution requires this stage to have existed, and it is recognisable in the Gymnosperms and Angiosperms.

In these, the culminating development of the vegetable kingdom, the sporogone masks completely the primordial vegetative system, of which however there still remain traces. The sporogone, which has become differentiated into the most varied and complex plants with organs of the utmost delicacy and efficacy, invariably produces spores of two sorts. The microspores (or pollen grains) quit their sporangia (anthers) before germination, to fecundate the female spore, but impregnation no longer depends on the action of vibratile corpuscles, leaving an antheridium. The entire ancient life of the male prothallus with its cellular tissue and its antheridia is represented by a tube piercing the exospore or external membrane of the pollen grain and coming into contact with the female element. The male protoplasm is no longer in corpuscles, but in order to impregnate, directly traverses by endosmose the membrane of the pollinic tube. The gradations by which this reduction of the male prothallus has taken place are not preserved in any existing plant.

The manner in which the development of the female macrospore has been arrested is even more remarkable. A special macrosporangium or "ovule" is born in Phanerogams, on branches of the sporogone in which the leaves are transformed into what is called a flower, an organ not differing morphologically from the sporangium-bearing spikes of Cryptogams. That no complete interruption or hiatus really exists between these different types of vegetation is demonstrated by a study of the macrosporangia of Gymnosperms.

In these the macrospore or embryonic sac contained in the macrosporangium (ovule) germinates on the spot and gives birth to a true prothallus or primordial cellular vegetative system, which fills the entire ovule. On this inclosed prothallus of the Gymnosperms (Conifers and Cycads), called an "endosperm," archegones appear (the "corpuscules"), which are fecundated by the last rudiment of the male prothallus (the pollinic tube). This is accomplished while the macrosporangium is still attached to the sporogone, and results in the production of an embryo in place of the oospore of the archegone. This rudiment of the new sporogone is already well developed when the macrosporangium or seed becomes detached. The sporogone only apparently succeeds directly to another sporogone, for actually the primordial vegetative system has preserved its sexual function; concealed and reduced as it is, it has still presided over the earliest developments of the agamous phase of the plant.

In certain Gymnosperms (*Salisburia*), and as if to better demonstrate the successive stages which have led from the Cryptogams to Phanerogams, the pollinic tube has inaugurated its movement, and the seed, apparently ripened, falls from the tree before the formation of any corpuscles or archegones. These are scarcely developed in the ovule, before the penetration of the male organ operates fecundation and gives birth to the phenomena which result in the formation of the embryo.

In the Angiosperms these processes are further reduced and the macrosporangium still more concealed by the production of an ovary. In tracing the homology of the complex and delicate processes involved in the reproduction of Angiosperms the climax of plant-evolution is reached.

Enough has been said to show the scope and value of the work which Saporta and Marion have laboriously produced. That part which attempts to bridge the gap, hitherto perhaps the most complete break in the natural system, is of such great importance that I have almost quoted the authors' own words. The interpretations and ideas set forth may perhaps be insufficient to carry complete conviction, but it will be seen that the remainder of this work, which treats principally of palæontology, confirms the theories derived from study of existing plants.

J. STARKIE GARDNER

(To be continued.)

PROF. ROBERTSON SMITH ON THE OLD TESTAMENT

The Old Testament in the Jewish Church. By Prof. W. Robertson Smith. (Edinburgh: A. and C. Black, 1881.)

THE only result of the "baiting" to which Prof. Robertson Smith has been subjected seems to have been the exact reverse of what his assailants intended. Forbidden to lecture upon Hebrew philology at Aberdeen, he has been invited to Edinburgh and Glasgow, there to detail to crowded audiences the method and conclusions of biblical criticism.

No one could be more fitted for the task he has undertaken than Prof. Robertson Smith. Clear-headed, acute, and learned, he had been a devoted student of natural science before he suddenly turned his attention to the Semitic languages and Old Testament criticism. The scientific habit of mind he had acquired was carried by him into his new studies, and it was inevitable that he should attach himself to that modern school of philologists and historians which by the application of the scientific method has revolutionised the study both of language and of history. He believed that the prin-

ciples of evidence and reasoning which held good of the language and history of Greece or Rome must hold equally good of the language and history of the ancient Jews.

The lectures he has now published under the heading of "The Old Testament in the Jewish Church," put in a popular and intelligible form the chief conclusions arrived at by modern critics in regard to the Pentateuch and its position in Jewish history, together with the evidence upon which they rest. The reader is led on from one point in the argument to another with admirable skill and clearness; nothing essential is omitted, while at the same time the whole chain of reasoning may be followed without difficulty by those who do not know a Hebrew letter and have never read a line of critical theology. Prof. Robertson Smith claims that there is no opposition between the results of critical inquiry and the fullest belief in the divine character of the Biblical record; on the contrary, these results, if frankly admitted, will be found to be confirmatory of the orthodox faith. Indeed the Professor's most relentless opponents ought to be gratified by the hard blows he deals at "rationalism," whatever that may mean.

With the theological aspect of the question we have of course nothing to do. But we must congratulate the Professor upon having found such large and sympathetic audiences to listen to an exposition of the mode in which the scientific principles of inductive inquiry have been applied to early history. The chief object of his contention is that the Levitical Law has taken its true place in the development of the Jewish nation; instead of coming at the beginning of the nation's existence, and so making the whole of its subsequent history unintelligible, it has been shown to have come at the end. Unknown to the most pious of the judges and kings, unknown equally to the prophets before the Exile, it naturally makes its appearance when Judah had ceased to be an independent state, when the free spontaneity of prophetic utterance was passing away, and when the priestly rulers of the returned exiles had no longer to fear the contamination of foreign idolatry or the erection of rival altars. The Levitical Law, according to Prof. Smith, follows the labours of the prophets; it does not precede them.

This result he claims to have obtained by questioning the Jewish records in accordance with the principles of scientific evidence. The credibility of a historical fact rests upon the authority of the documents or oral traditions that vouch for it, and naturally diminishes in proportion to the length of time between its supposed occurrence and the date of the earliest document in which it is described. The age and character, therefore, of a historical document must be closely tested and ascertained. The means for doing this are threefold: historical, literary, and philological. We must discover whether the historical conditions presupposed by the document agree with its assumed age, whether it bears marks of compilation and redaction, or has come to us straight from the hand of a single author, and whether the language in which it is written is as old as it professes to be. But when the age and character of the document have been thus determined, the scientific historian has still much to do. If its claims to antiquity can be substantiated we have still to ask whether the facts it narrates are the statements of a contemporary, or only the far-off echoes of a bygone tale. If, on the other hand, its claims are disallowed, we have yet to discover how much or how little of its assertions may be believed; what rests on first-hand evidence, and what is merely late tradition or the coloured narrative of the writer himself. And even when all this has been done, our work is not quite over. The facts we have extracted from our authorities must be pieced together and shown to follow in a natural and continuous stream of development. For in history as in nature the scientific method reveals to us the law of continuity and development, and whatever offends against this law cannot be admitted in a scientific reconstruction of the past. The school of historians to which Prof. Robertson Smith belongs believe they have proved that the traditional view of the Pentateuch and the Levitical legislation does offend against this law, and they would change and modify it accordingly. And in thus changing and modifying it they claim the support of history, of literature, and of philology.

OUR BOOK SHELF

Manuals of Elementary Science—Electricity. By Prof. Fleeming Jenkin, F.R.S. (London: Society for Promoting Christian Knowledge, 1881.)

THIS little work, of little more than a hundred pages, is a remarkable *tour de force*, since it contains in briefest language almost everything that can be taught, without using mathematical symbols, of the modern notions on electricity. It therefore well deserves to stand as a companion volume beside that remarkable primer of "Matter and Motion" of the late Prof. Clerk-Maxwell. The strong point of the present elementary work on electricity is the way in which it points out the connection between electrical (and magnetic) phenomena and the phenomena of other branches of physics as regulated by the law of the Conservation of Energy. So early as the sixth paragraph the fundamental idea of electric potential is introduced, a course which is surely to be commended, inasmuch as there is no more inherent difficulty in the mind of the beginner in conceiving of electricity as able to do work by moving from one position to another than of conceiving it as able to exert a force at a distance, while there can be no question that the former of these two conceptions is the more fruitful for expressing electric actions and reactions. The inherent connection between induction and charge is carefully insisted upon, and the beginner is told in simple language how the equal and opposite stresses between the two elements of an induction-pair, separated by an insulating medium, represent a store of energy whose seat is in reality in the intervening medium. Where so much pains has been taken to spare the beginner from having anything to unlearn, it is a pity that in the very first sentence our antiquated friends the "two imponderable fluids called positive and negative electricity" crop up. We also think it is a mistake to refer to a magneto-electric generator as a magneto-electric "engine" (as is done on page 107). The chapter on Electro-chemistry is admirable in every way. The following paragraph, on the perception of electricity, deserves to be quoted entire:—

"Our atmosphere is not only electrified, but presents such variety in the intensity and distribution of its electrification, that *a sense enabling us directly to perceive electricity* would frequently disclose a scene as varied as a gorgeous sunset. This sense would reveal the surface of solid bodies delineated by varying electrical density. Dielectrics would be transparent to the new sense, and conductors would be opaque, having their projecting edges, corners, and points marked with startling distinct-

ness. The effect of contact in producing or maintaining difference of potentials would be perceived by a difference in electric brilliancy, and this difference would vary with each re-arrangement of the objects. Every movement of our body, each touch of our hand, and the very friction of our clothes, would cause a play of effects analogous to those of light and shadow on the eye, while more highly electrified matter would bring into prominence by induction electrical differences between surrounding bodies. This speculation, however fanciful, helps us to conceive the omnipresence of electricity; and since the mechanical conditions required to excite sensation are fulfilled in the electrical relations between bodies at different potentials, there does not seem any very great boldness in suggesting that some living things may have *an electrostatic sense* so far developed as to be of use to them" (page 51).

Altogether this little work forms a very suitable introduction to its author's much more advanced and well-known "Textbook of Electricity and Magnetism."

The Natural History of the Cranes. A Monograph by the late Edward Blyth. Greatly enlarged and reprinted with numerous illustrations by W. B. Tegetmeier. (Published for the Author, 1881.)

THIS is an excellent monograph of an exceedingly interesting group of birds. On the arrival in 1873 of a pair of the beautiful white-naped cranes of Japan in London they were drawn by Mr. T. W. Wood for the *Field* newspaper, and the late Edward Blyth took the opportunity of publishing in the columns of that paper a monograph of all the then known species of crane. At the suggestion of Prof. A. Newton, Mr. Tegetmeier has republished these notes, inserting however much new matter that either want of space had prevented Blyth from incorporating, or that had come to hand since Blyth's death. Thus we have Wolley's graphic account of the nesting of the common crane in Lapland, Dr. Cullen's account of the nesting of the Demoiselle in Bulgaria, and even Col. Prjevalsky's account of a new species found at Koko-nor. Sixteen species, two belonging to the genus Balearica and fourteen to the genus Grus, are described. Mr. Wood's figures of *Grus leucauchen* are reproduced. There is a facsimile of the coloured figure of *Grus nigricollis* from Col. Prjevalsky's "Birds of Mongolia"; a spirited sketch by Prof. W. H. Flower of flocks of *Grus virgo* on the banks of the Nile; some copies of studies of cranes from Mr. Cutler's beautifully-illustrated work on Japanese ornament (charming studies); and a few woodcuts of anatomical details.

Cranes of one or more species are found everywhere, with the exception of South America, the Malayan and Papuan Archipelagos, and the scattered islands of the Pacific. The common European species, celebrated in all times for its migrations—

"So steers the prudent crane
Her annual voyage, borne on winds; the air
Floats as they pass, fann'd with unnumber'd plumes"—

was at one time very numerous in the fenny districts of England; so possibly Milton knew the bird. The name is quite wrongly applied to the heron in Scotland and Ireland, while in America and Australia the white egret herons are also called cranes. Old Æsop's fable of the stork being captured in the evil companionship of the cranes, and being condemned to death for thus even associating with notorious plunderers of grain, indicates that he well enough knew the two kinds of birds; far better indeed, as Blyth truly remarks, than did that renowned master of mediæval painters, who commits the curious zoological mistake of introducing cranes instead of storks in his world-known cartoon of the Miraculous Draught of Fishes.

In common with many other gregarious birds, cranes always place sentinels as a lookout, while the rest of the flock will trustfully repose; and they likewise leave them on the watch while on their marauding expeditions to crops of grain.

Zoological Atlas (Including Comparative Anatomy) With practical directions and explanatory text for the use of students. 231 coloured figures and diagrams. By D. McAlpine. Vertebrata. (W. and A. K. Johnston, 1881).

THE object of this work is to help the student in the examination and dissection of the leading types of animal life. The author quotes Dr. Macalister's words, "That in a practical science such as zoology it is only by the examination of specimens that any knowledge of the science worth acquiring can be obtained, and the function of a book is to assist in practical study." Bearing this in mind, he has here tried to assist the student by giving descriptions and drawings of one selected specimen from each group of the vertebrates. The skate and cod have been chosen to represent the cartilaginous and bony fishes respectively; the salamander to represent the tailed amphibia; the tortoise to represent the reptiles; and the pigeon and rabbit to represent the birds and mammals. The various systems are well represented, with the exception of the muscular system, which perhaps has been wisely overlooked. There can be no doubt but that this Atlas will form an important addition to the working student's books. It should remove many elementary difficulties from his path.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Dr. Carnelley's Experiment with Mercuric Chloride

I WAS a little surprised to notice from a paragraph in Prof. McLeod's letter in NATURE, vol. xxiv. p. 28, that he had been unable to repeat Dr. Carnelley's experiment with mercuric chloride. Immediately after the publication of my former letter, it was remarked to me, that although I had shown hot ice to be an impossible commodity, perhaps Dr. Carnelley's assertion of the existence of solid mercuric chloride above its boiling point might still hold. I therefore repeated this experiment, and after overcoming a few preliminary difficulties, obtained a result similar to that with ice. The difficulties were these:—After solidifying a cylinder of mercuric chloride round the thermometer (to which it adhered at first), on heating, the mercuric chloride soon became detached and fell from the thermometer. It had therefore to be sustained in position round the thermometer, by a stout iron or copper wire. Another difficulty arose from the fact that the mercuric chloride soon became deeply pitted and fissured, so much so, that the thermometer was sometimes seen through holes a quarter of an inch deep. This pitting went on till the mercuric chloride cylinder, though not much reduced in diameter, became a mere network, the thermometer being visible in many places. The erosion seemed to take place more quickly next the bulb; making the holes in the cylinder widest at the interior. Another difficulty lay in the high temperature causing, as Prof. McLeod noticed, the rupture of the thermometer thread; but by using a very good thermometer, and keeping it as nearly vertical as was convenient, this was entirely obviated. A large condenser is not required, and I only used a piece of combustion tubing fully an inch in diameter and about twenty inches long, the thermometer with the cylinder of mercuric chloride being inserted at one end, and a tube connected with a Sprengel pump at the other. The results obtained are as follows:—Melting point of mercuric chloride, 271° (uncorr.); boiling point, 291° (uncorr.). The pressure was now reduced to 400 mm., and the tube heated until the temperature was constant, the pressure again reduced, another reading taken, and so on until a vacuum was reached, or the cylinder had become too porous to give correct readings.

By the use of Gimmingham's form of pump the exhaustion can be increased in a very short time, and the readings all obtained from one cylinder an inch in diameter. Three series of readings were taken agreeing very well with each other. At first there were discrepancies, owing to the porosity of the cylinder not being noticed, but these disappeared when care was taken. The temperatures were not corrected, as the results were not intended for publication, as I expected some other worker to repeat the experiment, but that not being the case I give the numbers as they are, premising that the temperatures, if corrected, would be 6° or 7° higher for the upper and 4° to 6° for the lower ones. The following are the numbers obtained :—

Pressure in millimetres.	Temperature of volatilisation.	Pressure in millimetres.	Temperature of volatilisation.
400	270	40	233
300	267	30	228
200	263	20	223
100	253	10	214
80	248	5	205
60	242	0	185

A determination done before those given above gave—

Pressure in millimetres.	Temperature of volatilisation.
68	245
10	222
5	210

But I do not place the same confidence on these numbers, as they were obtained in ignorance of the porosity of the solid; but they confirm the others. It appears from the above that mercuric chloride is no exception to the general law which makes the volatilising point rise or fall with the pressure. The low latent and specific heats of mercuric chloride make it not nearly so suitable an exponent of the truth of Regnault's conclusions as water; the latter allowing of a whole hour's continued experiment. I think after these experiments the idea of being able to raise solids in vacuo to temperatures above their ordinary volatilising or boiling points may be dismissed as inadmissible, except it may be in some rare case of allotropy. J. B. HANNAY

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The Conservation of Electricity

BY the kind permission of Messrs. Macmillan and Co. I am allowed to quote the following paragraph from the preface to my "Elementary Lessons in Electricity and Magnetism," shortly to be published by them in their School Class Books Series, and now in the press. The preface is dated "March, 1881."

"The theory of Electricity adopted throughout is that Electricity, whatever its nature, is *one*, not *two:* that Electricity, whatever it may prove to be, is not *matter*, and is not *energy:* that it resembles both matter and energy in one respect, however, in that it can neither be created nor destroyed. The doctrine of the *Conservation of Matter*, established a century ago by Lavoisier, teaches us that we can neither destroy nor create matter, though we can alter its distribution and its forms and combinations in innumerable ways. The doctrine of the *Conservation of Energy*, which has been built up by Helmholtz, Thomson, Joule, and Mayer, during the last half century, teaches us that we can neither create nor destroy energy, though we may change it from one form to another, causing it to appear as the energy of moving bodies, as the energy of heat, or as the static energy of a body which has been lifted against gravity or some other attracting force into a position whence it can run down, and where it has the potentiality of doing work. So also the doctrine of the *Conservation of Electricity*, which is now growing into shape,[1] but here first enunciated under this name, teaches us that we can neither create nor destroy electricity, though we may alter its distribution—may make *more* to appear at one place and *less* at another—may change it from the condition of rest to that of motion, or may cause it to spin round in whirlpools or vortices which themselves can attract or repel other vortices. According to this view all our electrical machines and batteries are merely instruments for altering the distribution of electricity by moving some of it from one place to another, or for causing electricity, when heaped up in one place, to do work in returning to its former level distribution. Throughout these Lessons the attempt has been made to state the facts of the science in language consonant with this view, but rather to lead the young student to this as the result of his study than to insist upon it dogmatically at the outset."

[1] This is undoubtedly the outcome of the ideas of Maxwell and of Faraday as to the nature of electricity. It has nowhere been more excellently or pithily put into shape than in a discourse delivered by Dr. Oliver J. Lodge before the London Institution, "On the Relation between Light and Electricity," December 16, 1880 (NATURE, vol. xxiii. p 302).

The above paragraph is published at the present time because, since the date when my manuscript was sent to the publishers, a memoir has been presented to the Académie des Sciences bearing the title, "Sur le Principe de la Conservation de l'Électricité, ou seconde Principe de la Théorie des Phénomènes Électriques." Of this memoir, which is by M. G. Lippmann, only a brief extract has as yet been published in the *Comptes rendus* of the sitting of May 2, when it was read. In that short extract the general doctrine of the conservation of electricity is laid down with considerable clearness, and an elegant analytical expression of it is given in the briefest form, the author promising some examples of its application to the prediction of new and important phenomena. The publication of the complete memoir of M. Lippmann will no doubt be awaited with interest.

As my manuscript was placed in the hands of Messrs. Macmillan and Co. on the very day when the above extract was written, the phraseology used by M. Lippmann must have been adopted by him in entire independence of me. Since some weeks must elapse before my "Elementary Lessons" will be in the hands of the public, I wish to avoid, meantime, all chance of misunderstanding by taking the earliest opportunity, firstly, of making this acknowledgment, which is due to M. Lippmann, and secondly, of establishing my right to use the language of my preface as to the explicit enunciation of *the doctrine of the Conservation of Electricity*. SILVANUS P. THOMPSON

University College, Bristol, May 19

The Florence Herbarium

I BEG to forward to you the inclosed protest of the botanists of Florence against the proposed removal of the Herbarium and adjoining Botanical Garden at Florence to a new locality in that city.

It is well known to all botanists who have visited that city that, taking into account the importance of the herbarium, the admirable building in which this and the other collections are lodged, and the annexed botanical garden, the establishment at Florence deserves to rank amongst the first in the world, and is indeed scarcely second to any except that at Kew. It has an especial interest in the eyes of Englishmen, owing to the fact that it includes the invaluable collections of the late Mr. Barker Webb, which include, besides the type specimens of the Canary Island flora and of his other works, those still more important of Labillardière, of Rinz and Pavon, and of Desfontaines, whose herbaria all passed into his hands.

Although well acquainted with the Florence Museum, and disposed to believe that it would be difficult to find another locality equally well adapted for the purpose, I was unwilling to express any opinion on the subject without full information as to the new arrangements proposed in substitution for those now so excellent.

Within the last month my friends Sir Joseph Hooker and Dr. Asa Gray have visited Florence, and have carefully examined the present building and its appurtenances, and also the sites to which it is proposed to remove the herbarium and botanic garden. I learn that they have expressed an unqualified opinion that the proposed new building is altogether unsuited for the purpose, and would too probably tend to the injury and ultimate loss of the herbarium, while the site of the proposed botanic garden is also an unfavourable one.

Sir Joseph Hooker has written a full statement of his views to Prof. Carnel, recently appointed Director of the Botanical Museum, who has not, I believe, as yet published his opinion on the subject.

Under these circumstances I venture to hope that you will publish the accompanying document, with a view to prevent the accomplishment of a design so injurious to natural science. Those who wish to associate their names with the protest are invited to send them to M. E. Sommier, Lung'Arno, Corsini, Florence. JOHN BALL

10, Southwell Gardens, London, S.W., May 20

Florence, 5 Mars, 1881

A propos du déplacement projeté des collections botaniques du Musée d'Histoire Naturelle de Florence.

Lorsqu'au mois de Mai de l'année 1874 un grand nombre de botanistes de toutes les parties du monde se trouvèrent réunis à Florence, dans les salles des Collections botaniques fondées par feu le professeur Parlatore, M. le professeur Alphonse de Candolle fit observer "qu'une des choses les plus remarquables s'imposant à l'attention des membres du Congrès, était le Musée botanique, avec ses salles amples et commodes, où avaient lieu les séances du Congrès." ("Actes du Congrès Botanique international, tenu à Florence au mois de Mai 1874," pag. 220.)

Personne, alors, n'eût soupçonné que ce qui avait été jugé digne d'admiration par les hommes les plus compétents, serait déclaré mauvais et condamné à un bouleversement radical sept ans à peine après que ces paroles mémorables avaient été prononcées. Pour justifier l'abandon du Musée actuel, on prétexte l'inconvénient qu'il y a pour les étudiants fréquentant les cours de l'Institut des Études supérieures, place St. Marc, d'avoir à se rendre, pour une autre partie de ces cours, au Musée de Via Romana; et, afin de centraliser les édifices affectés aux études, on ne pense à rien moins qu'à opérer le déménagement des collections botaniques et à abandonner le jardin du Musée, avec toutes ses serres et annexes. En revanche, on parle de rendre à son ancien usage le modeste Jardin des Simples, situé à proximité du nouvel emplacement destiné aux herbiers. Mais a-t-on examiné si ce transport est réalisable, avantageux, et si le nouveau local de la place St. Marc, destiné à la Botanique, est adapté ou non à recevoir les herbiers et les autres collections?

Or ce local n'est autre que le bâtiment des anciennes écuries des Grands Ducs de Toscane, occupées plus tard par la cavalerie italienne. Pendant une série non interrompue de près de trois cents années, ces écuries ont logé des chevaux en très-grand nombre, et c'est là que l'on se propose de colloquer des collections de plantes, d'un prix inestimable, et si faciles à se détériorer sous l'influence de l'humidité! Il est vrai qu'en sacrifiant d'énormes sommes pour reconstruire l'édifice à peu près de fond en comble, on ferait peut-être disparaître les traces du long usage auquel il a servi; mais il est permis de se demander si, même dans ce cas, on obtiendrait jamais des salles comparables à celles du Musée actuel, soit en beauté, soit en salubrité, soit en solidité.

Ce projet étant soutenu et sur le point d'être mis à exécution par des personnes respectables, mais étrangères à la Science et par conséquent incompétentes, nous Soussignés, amis de la Botanique résidents à Florence, croyons de notre devoir, dans l'intérêt des collections, de protester contre ce déplacement, et, afin de donner plus de poids à notre protestation, nous invitons les Botanistes qui se sont trouvés à Florence lors du Congrès de 1874, ainsi que tous ceux qui connaissent les salles actuellement affectées aux collections de plantes, à joindre leurs voix à la nôtre pour empêcher qu'on ne mette à exécution un projet que nous croyons hautement préjudiciable à nos plus chères études.

Nous prions en conséquence les Botanistes italiens et étrangers de vouloir bien employer leur influence afin que le projet en question soit abandonné, et que les sommes, dès à-présent destinées à une œuvre inopportune et risquée, soient de préférence employées à augmenter le matériel scientifique du Musée actuel, par l'achat de collections de plantes vivantes et desséchées (surtout de plantes cryptogames) et d'ouvrages manquant à notre bibliothèque botanique et à acquérir les armoires et étagères, nécessaires pour placer et mettre en ordre une immense quantité de paquets d'herbier, actuellement sans emploi et inutiles aux études, ainsi qu'à adapter les serres du Jardin botanique aux exigences modernes, en commençant par y faire les réparations reconnues de première nécessité.

(*Signé*)	A. B. ARCHIBALD	E. MARCUCCI
	D. BARGELLINI	UGOLINO MARTELLI
	ODOARDO BECCARI	VINCENZO RICASOLI
	ANTONIO BIONDI	RICCARDO RICCI
	EMANUELE G. FENZI	NICCOLÒ RIDOLFI
	ENRICO GROVES	S. SOMMIER
	EMILIO LEVIER	P. DE TCHIHATCHEF

Barometer Pumps

COMMUNICATIONS from Mr. Sprengel have been published, in which he has defended his claim to be the inventor of the mercury barometer-pump. As long as he confined himself to this claim I had no right to interfere, but by his letter in your previous number (vol. xxiv. p. 53) he claims to be the inventor or father of all kinds of barometer-pumps. His right to this claim I dispute; for in May, 1847, I obtained a patent for improvements in sugar-refining, one of which is the conversion of a vacuum-pan into a large barometer by placing under a common vacuum-pan a long pipe in a perpendicular position, which acts as a pump whereby the sugar is taken out of the pan by its own weight in the long pipe, and thereby the vacuum in the pan is not destroyed, and the process of sugar-boiling is carried on continuously. The syrup to be boiled is added in the pan above, while the boiled sugar is taken out below through the barometer-pump. The specification of my patent was published in patent journals in London in 1847, and it is possible that Mr. Sprengel took the idea of his mercury barometer-pump from my sugar barometer-pump. But at all events Mr. Sprengel was not the first inventor of a barometer-pump. I claim that honour.

JAMES JOHNSTONE

Experiment Rooms, No. 1, James Square, Edinburgh, May 21

The Hutton Collection of Fossil Plants

IT has only within the last few days come to my knowledge (indeed only to-day authoritatively) that the Hutton Collection of Fossil Plants, at present deposited in the Museum of the Natural History Society of Northumberland and Durham, at Newcastle, had been named by the curator, Mr. Richard Howse, prior to the compiling by myself of a Catalogue of the Collection, published in 1878 by the North of England Institute of Mining and Mechanical Engineers. The labels on the specimens, referred to in the Catalogue, were therefore Mr. Howse's, and not, as I until now imagined, either William Hutton's original ones, or mere copies of them.

Moreover an unsigned MS. list of the specimens in the Collection, agreeing with the labels, with which I was furnished by the Mining Institute, and which was used freely by me in drawing up the Catalogue, must now be regarded as the result of much time and labour spent by Mr. Howse in identifying and naming the whole of the Hutton Collection.

I trust you will allow me space in your paper to acknowledge now what I should have made a point of acknowledging in the preface to the "Catalogue," had I been made acquainted with the facts of the case at the time. G. A. LEBOUR

College of Physical Science, Newcastle-upon-Tyne, May 18

"How to Prevent Drowning"

MR. MACCORMAC's valuable article induces me to call attention to a prevalent error.

Almost every treatise on swimming tells the beginner that every one can float without exertion. Even Mr. MacCormac seems to imply that "lying quite still with the mouth shut and the head thrown well back in the water" is enough to insure any one against sinking. Now this may possibly be true for most men, but certainly not for all. I am a practised swimmer, fond of the water, and have often tried. Going through all the orthodox motions of the deep breath, the folded arms, and the head thrown back, I go down instantly. This is in fresh water; in salt I believe I can just float, but have seldom had a good opportunity of trying. The fact is that men are very different in buoyancy. I have seen a man float motionless with head and shoulders out of the water. Others may be even denser than I am. Most men believe themselves capable of coolness and presence of mind. They should remember that these will neither supersede the art of swimming nor alter the laws of gravity.

St. John's College, Cambridge E. HILL

The Effects of Pressure on the Germination and Growth of Plants

THE following experiments may be of interest to vegetable physiologists:—On April 7, at 11 p.m., two sets of mustard-seeds—five in each set—were sown on pieces of moist cotton-wool, arranged as follows:—One piece was placed in a small bottle, which was then secured to the curved extremity of a glass tube, into the long arm of which mercury was poured till a height of forty-five inches was reached above the level of the metal in the shorter arm. The second piece, with its seeds, was placed in an exactly similar bottle, the neck of which was then made to dip beneath mercury, the bottle, of course, like the one soldered on to the tube, being inverted. This bottle was then placed beside the first.

The two sets of seeds were thus in exactly similar conditions, except for the increased atmospheric pressure and the compression of the atmosphere in the one case as compared with the other. The following was the course of development:—By 9 a.m. of the 9th three of the seeds under the 2½ atmospheres of pressure had protruded their radicles, and this protrusion by 12 p.m. of the same day had become considerable, while as yet there was no indication of commencing germination in any of the seeds of the second set. By 10 a.m. of the 10th these latter had just begun to germinate, the radicles of the seeds under high pressure being at the time a fourth and a third of an inch long.

Henceforward, however, the rapidity of development was reversed. The seeds, under ordinary pressure, grew rapidly, and their cotyledons became of a deep green colour; while the development of those under the high pressure became permanently arrested and the cotyledons of one that had entirely escaped from the seed-coats remained as etiolated as though they had been grown in absolute darkness.

They were allowed to remain untouched for eight days, when, as there was no change, the bottle was removed from the tube and simply allowed to stand inverted in the place it had formerly occupied. The two—out of the five—seeds which had hitherto remained unchanged now rapidly germinated, and grew into vigorous green young plants.

Does a greatly increased atmospheric pressure or a greatly compressed air prevent the development of chlorophyll, and while it stimulates germination does it prevent growth?

Liverpool, April 27 WILLIAM CARTER

[This is an interesting observation, and seems to suggest a new and comparatively unworked field of investigation—the effect of different amounts of atmospheric pressure on plant-life. With regard to the decomposition in the presence of chlorophyll and under the influence of sunlight, of carbon dioxide, it is remarked by Dehérain ("Cours de Chimie agricole," pp. 25, 26) that the conditions are analogous to those affecting the combustion of phosphorus. This is not luminous in pure oxygen at ordinary pressure, but becomes so immediately the oxygen is diluted with nitrogen or hydrogen, and still more when the pressure is much diminished. Boussingault has shown that leaves will not decompose pure CO_2 at the ordinary atmospheric pressure; but a small cherry-laurel leaf placed in the pure gas decomposed a cubic centimetre of it at a pressure of ·17m. (*Compt. rend.*, 1865, t. lx. p. 872.)]

The Magnetic Survey of Missouri

IT may interest some of your readers to know that, although our State Legislature absolutely refused to do anything to aid in the Magnetic Survey of Missouri, refusing by a "crushing" vote even to authorise county officers to have a true meridian established, the work will still go on. A gentleman of St. Louis, whose name is withheld at his own request, has assumed the entire expense, and we shall now begin a more minute examination of the Missouri, Grand, and Osage valleys. We shall hereafter travel by wagon, and shall do the work where it is most needed in order to disclose the real directions of the isogonic lines.

F. E. NIPHER

An Optical Illusion

THE illusion described by Mr. Wilson and commented on in an editorial note is anything but a novel one. An apparatus for the experiment was purchased by the Birmingham and Midland Institute, along with a quantity of optical apparatus, from Mr. Robert Addams, in, I think, 1857. Within the last few years I have noticed that the experiment is described and explained in Priestley's "Light and Vision." I am writing from home, or would give the exact reference. C. J. WOODWARD

Cambridge, May 23

I SHOULD like to know whether the following is a general experience, or only a peculiarity of my own vision?

If I stand with a source of light—a lamp or a window—at one side of my head, so that the light falls strongly on one eye only, and look, successively or simultaneously, at the images of a piece of white paper as seen by my two eyes, the image seen by the eye next the light is greenish white, and that seen by the eye farthest from it is light buff

If instead of white paper I use the gilt edges of a book, the image seen by the eye next the light is of a beautiful golden green; the other is of a brassy yellow, almost orange.

This phenomenon does not appear to depend on any effect of dazzling, for the experiment succeeds perfectly with very moderate degrees of illumination. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, May 23

The Speaking Tube Anticipated?

HAS the following appeared anywhere in this connection as yet, or not? If not, please allow it to appear in NATURE with this qualification only, that the italics are mine.

Describing the "speaking trumpets or pipes which ran, we are told, along the whole length of the Wall," Bruce says ("The Roman Wall," by the Rev. John Collingwood Bruce, p 76), that Drayton long ago sang of them as follows in his "Polyolbion":—

> "Townes stood upon my length, where garrisons were laid
> Their limits to defend: and for my greater aid
> With turrets I was built, where sentinels plac'd
> To watch upon the Pict: to me my makers grac'd
> *With hollow pipes of brasse, along me still they went,*
> *By which they in one fort still to another sent*
> *By speaking in the same to tell them what to doe,*
> *And soe from sea to sea could I be whispered through.*"

Ashton-under-Lyme, May 17 W. CURRAN

J. C. SHENSTONE.—A case of Phyllody of the calyx. "*Ranunculaceæ* particularly liable to this change" (Master's "Teratology," p. 246; recorded in *Anemone nemorosa, ibid.* p. 252).

ORIGIN OF THE ENGLISH MILE[1]

IT is known that the mile of 1609 metres long passed among English geographers and navigators as being the length of the terrestrial arc of 1′; in other words they made the degree equal to 60 of these miles. In reality it contains 69·5; there is thus an error of about one-sixth. This error, if it existed long among our neighbours, which I do not know, must have caused many a shipwreck. It has had another very remarkable result; it nipped in the bud the discovery of the law of universal attraction. The first time that Newton's great idea presented itself to his mind the proof failed him, because he made use of the common English mile to calculate the radius of the earth. He renounced the idea for a long time, and only took the calculation up again when he learned the results of Picard's measurement of a degree in France. Whence comes this defective estimate? Certainly it does not proceed from any effective measurement, for the worst degree measurements, among those which have been really made, and not fictitious measurements, like that of Posidonius, are far from presenting errors of such magnitude. English geographers then must have committed some mistake in taking their mile from ancient documents.

So long as navigation was limited to the waters of the Mediterranean, and to coasting along the western shores of Europe, it was scarcely necessary to trouble about the value of this element; but from the time that the discoveries of the Spaniards and Portuguese opened out a much vaster field, sailors were compelled to make some inquiry into the matter. I suppose that the English navigators applied to their geographers, and that these found nothing better to consult than Ptolemy, the great, the only authority in these matters. But Ptolemy himself refers to Eratosthenes; he says that he verified the measurements of the latter and found the same result, viz. 500 stadia for the terrestrial degree. I have thus been led to examine the measurement of Eratosthenes. According to the documents which historians have preserved, Eratosthenes measured the great arc of meridian which separates the parallels of Syené and Alexandria, and finally found 700 stadia to the degree. This is how he worked:—He observed at Alexandria, certainly by means of a gnomon, the zenith distance of the sun at

[1] Paper read at the Paris Academy of Sciences by M. Faye (*Comptes rendus*, xcii. No. 17).

midday in the summer equinox, and thus found $7^\circ 12'$. It is added that at Syené the bottom of the wells was fully lighted by the sun on that day, so that Eratosthenes concluded zero for the zenith distance of that body. I believe rather that the Greek astronomer caused an observation to be made at Syené with a gnomon, an instrument then very common in Egypt, and that that distance resulted from an effective observation, as well as in the case of Alexandria. We shall see that this conjecture is perfectly justified. We know that the observations made on the dark shadow of a gnomon bear a constant error equal to the semi-diameter of the sun, or, to speak more accurately, that they give the zenith distance of the upper edge of that body. The ancients do not seem to have remarked this; and in fact, as they deduced from their observations only the obliquity of the ecliptic or the epoch of the solstice, it did not concern them, for by combining the observations of the summer with that of the winter solstice, the error in question disappeared from the difference. But it is exactly the same here, since we have not to do with absolute latitude, but with the difference of latitude of two places at which the centre of the sun is found at midday on the same side of the vertical. Thus the amplitude $7^\circ 12'$ concluded by Eratosthenes is correct; it has moreover the advantage of not being sensiby affected by refraction.

Here is a first verification. On opening the *Connaissance des Temps* we find—

	°	′
For the latitude of Alexandria	31	12
,, ,, Syené	24	5
Difference	7	7

instead of $7^\circ 12'$. The difference, whatever may be the cause, is very small.

Here is a second and more delicate verification. The latitude of the point in Alexandria, where Eratosthenes observed, could not differ much from that which we have given. By adopting that and $7^\circ 12'$ for the zenith distance of the upper edge of the sun at the winter solstice we find $31^\circ 12' - (7^\circ 12' + 16') = 23^\circ 44'$ for the obliquity of the ecliptic. Syené gives $24^\circ 5' - 16' = 23^\circ 49'$. Is it possible that in the year 250 B.C. the obliquity of the ecliptic was from $23^\circ 44'$ to $23^\circ 49'$? From 1750 A.D. to 250 B.C. is 2000 years. At the rate of $48''$ diminution per century the obliquity would be

$$23^\circ 28' 18'' + 48'' \times 20 = 23^\circ 44'.$$

The observation of Eratosthenes at Alexandria is then authentic, and moreover very precise. That of Syené presents an error of only $5'$.

There remains the geodetic operation. Egypt was the only country of antiquity which rejoiced in a survey. The valley of the Nile was very populous at that epoch, as far as Syené, and no doubt the survey extended thus far. Eratosthenes must have had every facility for procuring the necessary documents. He must have taken into account the difference of longitude of $2^\circ 59'$ which exists between the two cities, without having had to determine it directly. I regard, then, the distance of 5000 stadia, in round numbers, as being quite as accurate as the other parts of his operation, and as applying to the arc of meridian comprised between the parallels of the two cities.

We finally conclude from this 694·4 stadia for the degree. The Greek astronomer gave, in round numbers, 700 stadia. What was this stadium?

To reply to this question I calculate the arc of meridian from Alexandria to the parallel of Syené, with the actual element of the terrestrial ellipsoid. It is 797,760 metres. At the rate of 5000 stadia we find 159·55 metres for the stadium. At the rate of 600 feet for the stadium, the foot adopted by Eratosthenes would be 0·266 metres. This was then the ancient Egyptian foot, which we now reckon at 0·27 metre; and in fact it was with this foot that the survey of Egypt must have been made. By this reckoning the 5000 stadia give—

$$5000 \times 600 \times 0{\cdot}27 = 810{,}000 \text{ metres,}$$

showing a difference of 12,240 metres, partly owing to that of the points of departure, partly to the error which we perhaps make in the length of the Egyptian foot in carrying it to 0·27 metre. Thus the measurement made in Egypt, more than 2100 years ago, by an able Greek astronomer, is as good as authentic. All the existing causes of uncertainty do not alter it more than one-sixth. It is certainly not from this quarter that the error can come for which we seek.

Nor is it in the measurement of Ptolemy, for he tells us he went through the same operations and found the same results; only he gives 500 stadia to the degree instead of 700. This difference is evidently due to the fact that Ptolemy, who lived 400 years after Eratosthenes, under another domination, did not make use of the same foot. In fact he employed the stadium of 600 Phileterian feet, and as this foot is about 0·36 metre, while the ancient Egyptian foot was only 0·27 metre, he had to reduce the 700 stadia of his predecessor to $700 \times \frac{27}{36} = 525$, or 500 in round numbers.

These estimates are confirmed, finally, by the Arabian astronomers, who measured, in 827 A.D., an arc of 1° in the plains of Mesopotamia. They found fifty-six miles, and concluded that they had thus verified the number of Ptolemy. The Arab mile being 2100 metres, the arc measured is found to be 117,600 metres, which corresponds to a stadium of 235 metres. This is very nearly the Phileterian stadium of 216 metres, except the error of the measurements seven times more sensible on so small an axis, and the uncertainty of our existing estimate of the Arabian mile in the time of the Kalif Almamoun.

To resume: the estimate of Ptolemy is only a sort of conversion of the excellent measurement of Eratosthenes in units of another epoch and of different length. It would thus lose a little of its first precision; but, such as we find it in Ptolemy, the English geographers were fully justified in taking it for the basis of a valuation of the arc of $1'$ and of offering it to the navigators of their country. Only, and it is here the mistake lies, they believed that the great Greek astronomer of Alexandria must have made use of the Greek foot. This is one and a half hundredths larger than the English foot. If the English geographers of the sixteenth century had strained this valuation ever so little, and had carried it to $\frac{5}{100}$ths, they would have found 630 English feet for the stadium, which they believed to be 600 Greek feet, and these 630 feet or 210 yards, multiplied by 500, would give them 105,000 yards for the degree, and exactly 1760 yards for the mile. The English mile, then, has evidently been deduced from the measure of Ptolemy; its error of one-sixth is solely due to the fact that the Greek foot has been confounded with the Phileterian foot.

LAURENTIAN GNEISS OF IRELAND

IN 1863 Dr. T. Sterry Hunt pointed out the resemblance of some specimens of rocks and minerals from Donegal which he had examined to those of the Laurentian series of North America. These rocks and minerals have been described by Dr. Haughton and Mr. R. H. Scott, who have pointed out that the "typical Donegal granite" is really a metamorphic bedded rock, containing in some places bands of crystalline limestone or marble. Outside the granite district are the newer series of schists, quartzites, and limestones, which occupy the whole of the Promontory of Innishowen, and were identified by the late Prof. Harkness with the Lower Silurian metamorphic series of the Highlands of Scotland. These two groups are shown on Griffith's Geological Map of Ireland, and it

will be seen on an inspection of this map that the quartzite series is represented as terminating obliquely against the margin of the granite or gneiss. This obliquity has never (as far as I can discover) been explained. The prevalent opinion seems to have been that the newer series has been converted into the older by increased metamorphic action. For some time past I never studied Griffith's map without the impression that the obliquity was due to unconformity of stratification, and on the determination of this point plainly rested the question whether the granitoid gneiss was, or was not, of Laurentian (or "Archæan") age.

Having had the advantage of a visit to some of the sections in the North Highlands of Scotland, in company with my colleague, Mr. R. G. Symes, under the able guidance of Prof. Geikie, last summer, I was in a position much more favourable for undertaking the investigation of this interesting question than would otherwise have been the case; and in the recent visit to Donegal I was accompanied by Mr. Symes and Mr. Wilkinson, of the Irish Survey, who rendered material aid in this preliminary survey.

The knowledge thus obtained has been of essential service, and I am happy to be able to state that we have succeeded in identifying the Donegal gneissic series, both as regards its mineral characters and its unconformable relations to the Lower Silurian series with the Laurentian beds of Sutherland and Ross. The relations of the two series in Donegal are similar to those which are to be observed in the Laxford and Rhiconich districts, where the Cambrian sandstones and conglomerates are absent, and where, in consequence, the Lower Silurian quartzites and limestones rest directly on the old gneiss. These conditions can be clearly made out in the neighbourhood of Lough Salt, near Glen, where successive beds of quartzite, limestone, diorite, and schist of the Lower Silurian series terminate abruptly at the margin of the gneissic series. We satisfied ourselves that this truncation of the Silurian beds is not due to faulting, as there is no appearance of disturbance or fracturing amongst the strata on either side. Similar—though less clear—indications were observable all along the eastern or southern margin of the gneissose district. Nor was the unconformity confined to the Silurian series, as we found that the beds of this formation came into contact with those of different geological horizons amongst the gneissic series at different places; there occurs, in fact, a double unconformity.

When examining the gneissic series we were often struck by the resemblance presented by the beds to those of Sutherlandshire, particularly amongst the lower portions. The massive foliated rock formed of red orthoclase, greyish oligoclase, green and black mica, and quartz, traversed by pegmatite veins, is identical in character with that from Rhiconich and Laxford; while the upper beds are interstratified with hornblendic and micaceous schists like those near Scourie. The occurrence of thin beds of white and grey marble, with sphene, idocrase, &c., in the Laurentian gneiss, seems peculiar to the Irish rocks, and brings them into close relationship with those of Canada.

A new basis has thus been formed for the whole superstructure of the Irish geological formations as deeply seated as that of any other country, and there can be little doubt that as the Laurentian beds have thus been recognised on the clearest evidence in Donegal, they may be recognised also in parts of Sligo, Mayo, and Galway, where the evidence is not so clear.

As I hope to have an opportunity of more fully stating the case at the forthcoming meeting of the British Association at York, it will be unnecessary here to enter on further details. I will only add that in speaking of the gneissic series as "Laurentian" I only wish it to be understood that the beds are contemporaneous with those underlying the Cambrian and Lower Silurian series of the Scottish Highlands. Whether they are really the representatives in time of the Laurentian beds of Canada is immaterial for my present purpose. For my own part I consider the preponderance of the evidence to be in favour of the view that they are in the main representative. EDWARD HULL

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JOSEPH BARNARD DAVIS

AFTER a short illness Dr. J. Barnard Davis died last week at his residence at Hanley, Staffordshire, being about eighty years of age. In the summer of 1820, while still a student, he made a voyage to the Arctic regions in the capacity of surgeon to a whaling ship. In 1823 he became a licentiate of the Society of Apothecaries; twenty years later he passed the College of Surgeons, and in 1862 took the M.D. degree of the University of St. Andrew's. In 1868 he was elected into the Royal Society. Soon after obtaining his first qualification he settled down in the Potteries, and but for what he describes, in the preface to his "Thesaurus Craniorum," as "an accidental conversation with a friend," might have remained through life leading the useful but uneventful life of thousands of general practitioners in the country, unknown beyond his immediate sphere of work. That accidental conversation however lighted up some smouldering embers of an interest which long before had been kindled by the lectures of Lawrence on the Natural History of Man, and led to the researches which resulted in the publication (in conjunction with the late Dr. Thurnam) of the "Crania Britannica," or delineations and descriptions of the skulls of the aboriginal and early inhabitants of the British Islands, illustrated with sixty-seven beautifully-executed lithographic plates, completed in 1856. Besides this Dr. Davis published many memoirs on anthropological subjects, including one "On Synostotic Crania among Aboriginal Races of Man," one on "The Osteology of the Tasmanians," one on "The Peculiar Crania of the Inhabitants of Certain Groups of Islands in the Western Pacific," and one published in the *Philosophical Transactions* for 1868, "On the Weight of the Brain in different Races of Man."

But it was by his famous collection, rather than by his writings, that Dr. Barnard Davis was best known, and the time, labour, and money which he spent in gathering it together is his greatest service to science. During a long period of time, in which the national and other public collections were losing the golden opportunities afforded by the extension of English adventure and commerce to all parts of the world, and allowing races to die out or their characteristics to become obliterated by intermixture with others, Dr. Davis let no chance of procuring specimens pass by, and was unwearied in his correspondence with travellers, collectors, and residents in foreign lands. He thus amassed together within a few rooms of a small house in Staffordshire a collection of crania and skeletons, nearly all with carefully-recorded histories, far exceeding in size that in all the public museums of the country put together, and only surpassed in very recent years by any of the Continental collections. In 1867 he published a catalogue called "Thesaurus Craniorum," which not only contains a description and many figures of the specimens, with 25,000 measurements, but which is also a perfect storehouse of information, owing to the literary references with which it abounds. The publication of this catalogue made the collection so well known that it naturally led to its increase, and in 1875 it became necessary to publish a supplement on the same plan, in which the history of the literature of the subject was continued to date. The catalogue and supplement contain descriptions of more than 1700 specimens, mostly crania.

Warned by failing health and increasing years of the

desirability of making arrangements for the future preservation of the collection, Dr. Davis entered into negotiations about a year ago with the College of Surgeons of England, by which body it was purchased, and in whose museum it has now been arranged in such a manner as to be accessible to all workers at anthropology. Such a collection as this, well cared for in a public museum, is a solid and permanent increase to the wealth of the country, for even if the methods of investigation now used are superseded by others, and the present literature comes to be looked upon as obsolete, the specimens will remain as materials for building up the history of the human race; and as the interest in the subject increases—as it certainly will—many of these evidences of the physical structure of people passed or passing away will come to be objects of priceless value.

W. H. F.

M. H. MILNE-EDWARDS

WE referred some time ago to the fact that a medal, subscribed for by a number of his admirers, had been presented to the venerable zoologist, M. H. Milne-Edwards. No one better deserves such a recognition,

Medal presented to M. H. Milne-Edwards.

and none know better than the French how to do such an honour gracefully and impressively. Our illustration is reproduced from *La Nature* of May 7, where will be found a pretty complete list—and it is a long one—of M. Milne-Edwards' works. The medal, a production of some artistic merit, is the work of M. Alphée Dubois.

THE ZOOLOGICAL RESULTS OF THE SOCOTRAN EXPEDITION

AT the meeting of the British Association in 1878, upon the motion of Mr. Sclater, a Committee, consisting of Dr. Hartlaub, Sir Joseph Hooker, Capt. J. W. Hunter, Prof. Flower, and the mover, was appointed to take steps for the investigation of the natural history of Socotra. Socotra, it was stated, was one of the few spots in the world which seemed never to have been trodden by the foot of the naturalist, and would in all probability be found to contain distinct insular forms, of which it would be highly interesting to know the relations, and to secure specimens for our collections.

The grant of 100*l.*, given by the Association for this excellent object, having been subsequently increased by two sums devoted to the same purpose out of the Government Fund of 4000*l.* administered by the Royal Society, the Committee felt strong enough to proceed to action, and in the winter of 1879 were fortunate enough to secure the services of Prof. J. B. Balfour, of the University of Glasgow, for a special expedition to the island.

Prof. Balfour left England on January 9, 1880, accompanied by Alexander Scott, a gardener from the Royal Botanic Gardens, Edinburgh, as collector, and reached Aden by the French mail on the 24th, where he obtained every sort of advice and assistance from the civil and naval authorities for his expedition. Owing to adverse winds and other difficulties Prof. Balfour did not manage finally to reach Socotra until February 11, when the party, which had been reinforced by the addition of Lieut. Cockburn of the 6th Royals and a corps of attendants from Aden, were put on shore at Gollonsir, a village situated at the north-west end of the island, by H.M.S. *Seagull*. In his report to the Socotran Committee Prof. Balfour gives the subjoined account of his subsequent proceedings:—

"Making in the first instance Gollonsir our head-quarters, we explored the adjacent country to the south and south-west until February 25, when we struck tents, and sending our heavy baggage and stores by sea, started to march to Hadibu. We took four days to accomplish this, reaching Hadibu late on the night of the 28th inst.

"Having communicated to the Sultan the fact of our arrival, he came to Hadibu on March 1, when we had an interview.

"Establishing our *depôt* now on the Hadibu plain, about a mile from the town, we spent the time until the 7th inst. investigating the magnificent Haggier range of hills shutting in on the south the Hadibu plain.

"On March 8, leaving a tent-Lascar in charge of the *depôt* at Hadibu, we started upon a trip to the eastern end of the island, going eastward along the northern side and returning westward by the southern side of the island. During this trip we reached Ras Momé, the extreme eastern headland. Camp at Hadibu was again entered on March 18.

"As yet we had not seen much of the southern parts of the island, so on March 22 we left Hadibu on our last excursion. Crossing the Haggier range we emerged upon the southern shore at Nogad, traversed the coast-line for some distance, and then recrossed the island so as to

come down upon Kadhab village on the north side. We regained Hadibu on the 27th inst."

From Hadibu the party were conveyed back to Aden in H.M.S. *Dagmar*, and arrived at the latter port on April 9.

The two months thus spent in Socotra were certainly not sufficient for the proper investigation of its fauna and flora, though considering the time occupied very satisfactory results, as will be seen further on, were obtained. As observed by Prof. Balfour in his report, what has been done by the expedition is but a fragment of what remains to be accomplished. In exploring the island he deemed it better, considering the short time of the sojourn, rather to attempt to cover as much ground as possible, with the view of obtaining a representative collection, than to examine in detail a limited tract of country. By doing this much barren land was travelled over, and many rich and fertile spots were necessarily only superficially looked at. Especially amongst the hills of the Haggier range there are valleys which would well repay a careful and extended investigation. The expedition must, therefore, be considered only preliminary, for Prof. Balfour feels assured that a rich harvest awaits any collector who may hereafter visit the island.

"If, at any future time," Prof. Balfour observes, "an expedition is sent to the island, it would be well if the date of its arrival were timed so that it should have the last months of a year and the first months of the following upon the island. Our expedition reached the island too late in the year, so that before we left the heat was so intense as to prevent our doing so much work as we desired. Again, the inaccuracy of our knowledge of the geography of the island is a point to which the attention of future expeditions should be directed. The chart based on Wellsted's observations is the only available one, and that is so incomplete and incorrect as to be almost useless to any one moving about the island."

Collections in all branches of natural history were made by Prof. Balfour's expedition, Prof. Balfour, as might have been anticipated, devoting himself specially to the botany of the island. As arranged by the Socotran Committee, the first set of the zoological specimens have been sent to the British Museum, and that of the plants will go to Kew when Prof. Balfour's memoir on them has been published. The rocks and geological specimens have been placed in the hands of Prof. Bonney of Cambridge.

The collections are as yet but imperfectly worked out, but sufficient has been done to give results of very great interest in every branch of natural history.

The Birds, reported upon by Mr. Sclater and Dr. Hartlaub,[1] are found to belong to thirty-six species—generally "North-East African in character, being mostly such as are included in Heuglin's 'Ornithologie Nord-ost-Afrikas.'" Six however are peculiar to the island, the most remarkable of them being a new form of sparrow with a very thick bill, which is named by Messrs. Sclater and Hartlaub *Rhynchostruthus Socotranus* (Fig. 1). It is however possible that the *Rhynchostruthus* and other new species may still turn up on the peninsula of Gardafui, of which the zoology is almost unknown to us.

Mr. Butler's report on the Butterflies and Moths captured by Prof. Bayley Balfour and his assistants in Socotra[2] tells us that of the thirteen species of which examples were brought, not less than seven were new to science. "Of the new forms five are allied to previously-recorded types from the following localities:—one from the Comoro Islands, one from South-West Africa, one

FIG. 1.—*Rhynchostruthus Socotranus.*

FIG. 2.—*Tropidiphorus Balfouri.*

from Zanzibar, and two from Arabia. Without the help of these last two it would therefore have been impossible for any one not acquainted with it to guess at the locality from which this collection had been obtained."

The land-shells obtained in Socotra have been assigned to Lieut.-Col. H. H. Godwin-Austen, F.R.S., for examination, and his report on the Cyclostomaceæ was read before the Zoological Society on February 1 last.[1] Col. Godwin-Austen states that the Socotran Cyclostomaceæ, as a whole, "are, as might have been expected, African and Arabian in character, the relationship being certainly, as regards the operculated forms, more Arabian than African. The collection contains a number of very distinct, fine, and interesting forms, of which some were already known, but many are new, and considerably extend the list of Socotran species. The large area of limestone formation on the island is especially favourable to the existence of these creatures, while island conditions have as usual modified and increased the species."

"Judging from the land-molluscan fauna of Socotra," continues Col. Godwin-Austen, "there is strong evidence that the island was once directly connected with Madagascar to the south. We know the great antiquity of that island, and it is not unreasonable to suppose that in Socotra, the Seychelles, Madagascar, and Rodriguez, we have the remnants of a very ancient, more advanced coast-line on this western side of the Indian Ocean, which line of elevation was probably continuous through Arabia towards the north. With an equally advanced coast on the Indian side, the Arabian Sea would under these con-

[1] "On the Land-Shells of the Island of Socotra collected by Prof. J. B. Balfour," Part 1 (Proc. Zool. Soc., 1881, p. 251).

[1] "On the Birds collected in Socotra by Prof. J. B. Balfour" (Proc. Zool. Soc. 1881, p. 165).

[2] "On the Lepidoptera collected in Socotra by Prof. J. B. Balfour" (Proc. Zool. Soc., 1881, p. 175).

ditions, have formed either a great delta, or a narrow arm of the sea into which the waters of the Indus and Euphrates drained. Such conditions would have admitted of the extension of species from one side to the other, which the later and more extensive depression of the area, as shown in Scinde, afterwards more completely shut off."

Amongst the more remarkable of the operculated land-shells described by Col. Godwin-Austen is a new species of *Tropidiphorus*, which is proposed to be named *T. Balfouri* after its discoverer (Fig. 2).

The Reptiles collected by Prof. Balfour in Socotra have been worked out by Dr. Günther[1] and Mr. W. T. Blanford, Dr. Günther taking the Snakes and Amphisbænians, and Mr. Blanford[2] the remaining Lacertilians. Both of these collections were found to be of considerable interest. Among the snakes is a new form allied to *Tachymenis*, which Dr. Günther has proposed to call *Ditypophis*, and a new species of *Zamenis* (*Z. Socotræ*). Both these indicate an alliance with the circum-Mediterranean fauna. On the other hand the Socotran Sand-Asp (*Echis colorata*) belongs to an Arabian and Palestine species, while the Amphisbæna of Socotra (*Pachycalamus brevis*, gen. et sp. nov.) has its nearest allies in Eastern and Western Tropical Africa. Of the six species of lizards of which examples were in Mr. Blanford's series, three proved to be new to science.

At the same meeting of the Zoological Society Mr. Charles O. Waterhouse read a paper on the Coleopterous Insects which had been collected by Prof. Bayley Balfour in Socotra. The number of species of which examples were collected was stated to be twenty-four, and showed that the fauna of Socotra, judging from this collection, was distinctly African. Twelve of the species were described as new to science.

It will be seen, therefore, that although the zoological collections made by Prof. Balfour were very small in each group—in some cases almost of a fragmentary character—the results in every case present features of great interest. It is obvious that, judging from what is thus known, Socotra must possess—what was thought scarcely probable by many at the time the scheme for exploring it was first started—an indigenous fauna of considerable extent, one well worthy of further investigation, which the Socotran Committee, we believe, are quite resolved to undertake if they can obtain the necessary means. As regards the flora of Socotra we have said nothing, because Prof. Balfour, who has himself undertaken the investigation of the botanical collections, has not yet completed his task. But a preliminary examination has shown, we believe, that his series embraces about 150 absolutely new flowering plants, amongst which are from fifteen to twenty representatives of new genera —so that it is manifest that, like the fauna, the flora of Socotra possesses a strong autochthonous element.[3] Of this we hope to be able to give some account when Prof. Balfour is further advanced in his work. Meanwhile there can be no question that the Socotran Committee have accomplished a most useful bit of work, and that in this case, at all events, the public money devoted to scientific research has been well applied.

A GEOLOGIST'S NOTES ON THE ROYAL ACADEMY

ONLY of late years has the importance of accuracy in the drawing of rock structure been recognised either by artists or by the general public. For this we are indebted to no one so much as to Mr. Ruskin, whose chapters on the subject in the fourth volume of "Modern Painters" should be read again and again by every student who considers the faithful representation of Nature not unworthy of the aims of Art. It is true that some of the greatest among the older masters—as Titian or Durer—rendered with great spirit and considerable accuracy the more salient features of rock structure, but from one cause or another they seldom entered into details, and were rather prone to exaggeration. The majority, till almost the present time, appeared to consider themselves unfettered, and "improved" upon Nature in accordance with the fancied requirements of the principal theme of their pictures. Some of the results may be seen in the volume to which we have referred. Within the last few years a due estimate of the special excellencies of Turner's work has produced a salutary influence, and more than one artist (like Elijah Walton, to speak only of the dead) has grappled successfully with the difficulties of rock structure. Thus the boulders, studied apparently from lumps of modeller's clay, the dilapidated crags, tottering like habitual inebriates, the attenuated peaks, which might have been decapitated with a walking-stick, are rapidly disappearing from the walls of our exhibitions. In many pictures however we still perceive more of good intention than of knowledge, and the number of those who cannot be said to "draw with the understanding" is by no means small.

We venture then to offer a few remarks on rocks as they are represented on the walls of the Royal Academy. In No. 13, "Gorse-cutting," passing clouds render the hillside in the background rather vague, but it may be doubted whether this is the only cause of an indefiniteness in the rock-structure, which is certainly also observable in that of the foreground. 28, "Llyn and Nant Gwynant," exhibits much careful mapping-out of the rocks, but cannot otherwise be said to be successful. There is a want of character in the craggy hillside in 55, "A Mountain Road," and the boulders are flat and indefinite, as though the artist had inserted them in his studio when the memory of their appearance in the field was beginning to fade from his mind. The same inability to seize the dominant characteristics of the rocks appears in 80, "Waiting for the Ferry." In 85, however, the "Land of Streams," its artist has been much more successful. Mr. C. E. Johnson has given us a painstaking study of a mass of hard stratified rock, which, as it dips away from the spectator, forms outcropping, curving ledges, over which the water dashes. In these, and in the craglets, both in foreground and middle distance, the principal facts of bedding and jointing are accurately rendered. Not so, however, in 89, "The Head of Teesdale," where we are led to conclude that the rocks are modelled from the same material as those in the scenes of theatres. The artist of 98, "A Storm in the Desert," has been more careful, but unless there is something exceptional in the locality it is difficult to conjecture what the rock may be. A mountain streaked with snow in the background of 122, "A Sermon in the Hayfield," is carefully studied, but still is rather wanting in character, and the colouring strikes us as crude. Mr. C. E. Johnson has again been successful in "The White Sands of Iona" (188), which is a very careful rock study. Rough craglets either of granite or of the granitoid gneiss, common on the western coast of Scotland, crop out among the slopes of sand. Of some the upper parts are smooth and polished, exhibiting traces either of the action of glaciers, like many another reef around the Western Highlands, or possibly in this case an example of the gentler attrition of blown sand; the rocky knolls in the middle distance should also be noticed. "The Scapegoat" (211) is a picture which causes us some little perplexity. There is an appearance of careful study both in the foreground craglets and in the bare mountains, which make up the scenery of this "Land not inhabited"; but still it is difficult to decide upon the

[1] "Descriptions of the Amphisbænians and Ophidians collected by Dr. Bayley Balfour in the Island of Socotra" (Proc. Zool. Soc., April 5, 1881).

[2] "On the Lizards collected by Prof. Bayley Balfour in Socotra." (*Ibid.*)

[3] A very fine new *Begonia* from Socotra, of which tubers brought home by Dr. Balfour have flowered at Kew, is figured in the April number of the *Botanical Magazine*, tab. 6553.

actual character of the rock or the locality which the animal has reached. The great block in the foreground, upon which the scapegoat is standing, might be either a limestone or a felstone. The same rock constitutes the nearer mountain, and this, judging from the peculiar way in which the dominant joint planes alter their direction, can only be igneous. Hence we must assume it to be a felstone with a rather platy jointing. If this be the case, then the mountain-crests are exceptionally sharp, and the structure to which this is due is insufficiently indicated. The same general character is maintained in the distant mountains, but these are even more jagged. The picture suggests a combination of some Sinai photographs, rather imperfectly understood, with memories, which have become vague, of the Southern Alps. Might it not also be doubted whether such a waterfall as that on the right would occur in a "lone land" on the borders of Palestine? In the "Diamond Merchants" (258), if we do not wrongly identify the lighthouse on the distant skerries, the scene represented is in the immediate neighbourhood of the Land's End. The rock there should be granite, and the structure of the craglet in the foreground will accord with this, though the colour is unusual for that district; but the cliffs beyond much more resemble, especially in a sort of streakiness, some of the stratified rock locally called "killas." In "Past Work" (489), by the same artist, there is a similar uncertainty of treatment in the rocks, showing that he has not thought them worthy of that appreciative study which he has bestowed upon most matters connected with the sea. The locality of 271, "The First to look out for the Homeward Bound," may be presumed to be also Cornwall; but the rocks belong to quite a new type, and if they are anything, must be some kind of hard mudstone. 315, "Mountain Tops," is bold in colouring and in design, but can scarcely be regarded as successful. The floating clouds and the strong shadow into which the hills are cast by the gleaming sky, naturally obscure their structure, but would hardly account for the streakiness which they exhibit, as though the picture had been finished by wiping it down with a brush parallel to the leading outlines of each peak. It reminds us of an exaggeration of one of Turner's views of Loch Coruisk. 317, "A Babbling Brook in Ochmore," though hung rather too high, shows a careful study of gently-inclined strata. Mr. Brett, in "St. Ives' Bay" (340), gives us an excellent study of some granite rock in the middle distance of his picture; but this artist's mastery of his subject is brought out better in "Golden Prospects" (445). In the foreground is a ruined craglet or miniature "tor" of granite—a wonderfully truthful study; form, structure, texture of the rock, and the crisp crusting of dry lichens, perfectly rendered. The bald patches of granite amid the rough vegetation are admirably truthful, as is the hazy light, which renders the more distant cliffs, massive as they really are, almost ethereal. Out at sea, if we mistake not, is the Longships Lighthouse and its dangerous reef. Those who in any way equal Mr. Brett in his love for the Cornish coast will find it hard to tear themselves away from this picture of one of its grandest scenes. It may however be remarked that the general effect suggests a day early in the summer, but still the heather is in bloom.[1] There is some good promise in the limestone hills in 475, "A Grecian Tomb," though the hazy evening light is favourable to the avoidance of difficulties. In the "Ramparts of Idwal" (406), the colour is rather too monotonous, but a knoll with ledges of rock cropping out through rough turf and in the bed of a streamlet is admirably rendered. The peculiar texture of certain rocks composed of indurated volcanic ash seems to have impressed itself upon the artist. In "Hope Deferred" (419) there is little character in the rock; that however in the foreground may only be intended for very hard earth. In "Lofoden" (485) Mr. E. T. Compton gives us a careful study of a rather massive schistose rock in the mountain in the middle distance, and of one more granitoid in the foreground. The jointed structure of the latter and rather scaly aspect of parts of the former are well rendered, and the general effect of the picture is truthful, though the author has not selected for his subject one of the most characteristic parts of the Lofoden Islands. It would not be difficult to find a view like this in several spots on the mainland, but the wilder mountains of Hindö and Ost Vaagen are without a parallel in Scandinavia. Mr. C. Stuart's "Uncertain Weather" (507) is hung too high, but it appears to be a very careful study of the well-known crag overhanging the tarn in Cwm Buchan. In Mr. B. W. Leader's "Glyder Vawr" (521) we have a careful study of the felstone crags in the upper part of that mountain. The somewhat curving surfaces of outcropping rock in the middle distance of the picture and the boulders in the foreground are well rendered. The peculiar effect which the artist has chosen—a sudden gleam of sunlight glinting upon wet surfaces, gives to the rock an exaggeratedly rugged structure. The effect may be truthful, but is certainly rare, and we may doubt whether it is wise to select one producing results so abnormal. A rock in the foreground of "O'er the Heather" (539) is spotty, muddy, and indefinite. An unfamiliar effect has been chosen in "Kynance Cori as it appeared one day last January" (564), for a thick coating of snow rests upon the rocks. It is hung rather high, but the author does not seem to us to have quite succeeded in catching the peculiar structure and weathering of serpentine. "Nature's Decay" (905) also suffers from being too high. The pile of *débris*—earth and trunks and fallen branches—masks a good deal of the rock, but what is visible seems to be carefully drawn; that in a more distant ravine appears a little conventional. Sir R. Collier, in his "Glacier of the Rhone" (984), gives us one of his usual careful studies of rock and ice. The outcropping ledges and scattered boulders among the rough herbage are carefully drawn, making a most truthful rendering of a portion of rugged mountain-side. It may however be doubted whether the ice in the lower part of a glacier could be so generally blue as it is here represented. "Boulders at Rest" (1352) has some good points about it. The granite crag is carefully studied, and the structure is well rendered, but the two "natural" arches have a rather artificial aspect, and certainly weaken the effect of the composition. The boulders beneath have their individuality remarkably well preserved, but the artist has not been so successful in rendering their texture, which is rather woolly. In "The Dead Sea from Engedi" (1360) the artist appears to have striven honestly but not very successfully to record the scene. The rocks exhibit a streakiness of dubious authenticity, and convey to one the impression that while the general effect was felt by him the reasons for it were not understood.

Several other pictures we have been obliged to leave unnoticed, either because the rocks are obviously quite a subordinate part (though from our point of view that is no reason why they should not be accurate), or because the picture has been hung so high that it cannot be properly studied, and criticism might be unjust to the artists. The water-colour drawings we have not yet examined. T. G. BONNEY

[1] While we were studying this picture a bystander described it to his companion as "No doubt the Needles idealised"

NOTES

THE death is announced of Mr. John Blackwall, F.L.S., at Llanrwst, on May 11, at the great age of ninety-two. He was elected a Fellow of the Linnean Society as far back as 1827, and was nearly its oldest member. His principal work was a magnificent illustrated Monograph of the British Spiders, published by the Ray Society about twenty years ago. He also

published a considerable number of papers on general zoology, in which the possession of keen powers of observation is everywhere evident. In 1834 a volume from his pen appeared under the title of "Researches in Zoology," a record of observations in the field, with deductions therefrom; a second edition was published in 1873.

THE *Launceston Examiner*, of March 14, announces the death of Mr. Ronald Campbell Gunn, F.R.S., at the age of seventy-three years. Mr. Gunn, who was born at the Cape of Good Hope, and landed in Tasmania in 1830, held successively several highly important official positions in the colony. Mr. Gunn's tastes, the *Examiner* states, were eminently scientific, but botany was his favourite study, and this subject he was indefatigable in pursuing. At an early period he was elected a Fellow of the Linnean Society of London, and subsequently a Fellow of the Royal Society of London, the highest scientific honour which can be conferred on any person. Mr. Gunn began to investigate the botany and natural history of Tasmania in 1831, and in the prosecution of his researches rambled over most of the colony. His botanical labours are recorded in Sir Joseph D. Hooker's "Flora of Tasmania," and accounts of his excursions and other scientific labours appear in the *Annals of Natural History*, *Journal of Botany*, &c. He was also editor of the *Tasmanian Journal*, a scientific serial published by the Royal Society of Tasmania. The late Mr. John Gould, in his valuable work upon the "Birds of Australia," acknowledges the assistance which he received while in Tasmania from Mr. Gunn. We may also mention that Mr. Gunn drew up for West's "History of Tasmania" a compendium of the zoology of Tasmania.

MR. ALBERT BRUCE JOY'S statue for the Harvey Tercentenary Memorial is now cast in bronze, and will probably be soon sent to Folkestone, the native place of the discoverer of the circulation of the blood, where a suitable site has been provided for it on that well-known promenade, the Lees. In modelling his successful statue Mr. Bruce Joy has closely followed the portrait of Harvey by Janssens, preserved in the Royal College of Physicians. Mr. Joy has also produced a reduction of the bust of Harvey.

THE Scottish Meteorological Society have received the observations made during last winter by Mr. A. O. Thorlacius, their observer at Stykkisholm, Iceland, from which it is seen that last winter was one of the severest of which there is any record, ice having formed in the harbour at least four feet thick. The only winter that can compare with the last during the present century was that of 1807, when the inhabitants of Grimso, an island lying nearly sixteen English miles off the coast of Iceland, walked across the ice to the trading station of Ofjord, a thing which was not known ever to have occurred before.

DURING his last visit to the United States, it will be remembered, Prof. Tyndall initiated a trust fund with the object of assisting students in physics who should show aptitude for original study and should wish to complete their education in Germany. It is stated that the fund has so far prospered as to furnish a moderate income for two students, who have just been nominated.

THE Glasgow Mechanics Institution, which as such has existed since 1823, has recently had its constitution altered and its name changed to that of "College of Science and Arts." At the close of the session Sir William Thomson, in distributing the prizes, mentioned that he had imbibed his first ideas of chemistry in the Mechanics Institution, and expressed himself much pleased with its present appearance, and the prospects of the Institution under its new name, and the superintendence of Mr. A. Jamieson, the principal. On this occasion the hall was lighted with Swan's electric lamps under arrangements very efficiently made by Mr. Jamieson.

THE Russian ladies seem to be advancing rapidly and surely in the direction of higher education. Besides the medical courses at St. Petersburg, there was opened two years ago in the same city a kind of ladies' university, being a series of courses for higher training in the mathematical, physical, and historical sciences. We learn now, from the annual report recently published, that notwithstanding all opposition on the part of Government to this institution, it has acquired further development. The third class is opened this year, and the fourth will be opened next year. The number of lady students, which was 789 in 1879, has already reached 840, and Prof. Famintzin observes that this number would have been much larger were it not for the obstructive regulations which are intended to check the further development of the institution. It is worthy of notice that the money necessary for the institution is collected from private sources—students' fees (5*l.* per annum) or by voluntary subscriptions. Like courses are already opened at Moscow and at Kieff, but the instruction given at Moscow is more superficial in what concerns natural science.

THE French Association for the Advancement of Science is to meet at Rochelle next year.

THE arrangements for the International Exhibition on the occasion of the International Medical Congress have within the last few days been nearly completed. The Exhibition itself, quite apart from the Congress, will be held in the eastern, the western, and the quadrant arcades of the Horticultural Gardens, and in some of the galleries of the Albert Hall. The Exhibition will be open from July 16 to August 13, but it is not yet decided whether there shall be any formal opening. That the Exhibition will be really international is indicated by the fact that there will be contributors from France, Germany, Austria, Italy, Belgium, Holland, Norway, and the United States. Much still remains to be done, but there is every prospect of the Exhibition being a credit to England. The secretary is Mr. Mark Judge, of the Parkes Museum of Hygiene, University College, Gower Street. From him particulars may be obtained.

IN the *Proceedings* of the Natural History Society of Glasgow Mr. J. A. Harvie Brown continues his reports on Scottish Ornithology. In the second part of the fourth volume we have the result of his observations, extending from October 1, 1879, to September 30, 1880. The utility of Mr. Harvie Brown's observation consists in a comparison of the occurrence of migratory birds in Scotland with the meteoric conditions of the season, and it seems perfectly certain that the increase or decrease in the number of our summer visitants depends greatly upon the strength and duration of favourable or adverse winds prevailing at the time they visit our shores. We are pleased to hear Mr. Cordeaux is also compiling records of birds noticed in the Humber district, and it would be interesting to naturalists if similar observations could be regularly made in different parts of Great Britain.

AN exhibition of microscopes and other scientific apparatus was held in January in place of one of the ordinary weekly entertainments at the Albert Institute, Windsor. This was so successful that another was held in April, and the Windsor and Eton Scientific Society formally constituted. The general meetings will take place monthly.

WITH respect to Burnham Beeches, we are told on good authority that the Corporation of London (who recently purchased the estate) have forbidden the picking of flowers on account of its disturbing the game! Among other things this prevents the science-masters at Eton going there on a half holiday with their

botanical boys or the members of the School Natural History Society.

IN consequence of Mr. Bidwell's severe indisposition, he has been unable to prepare his paper on "Telegraphic Photography," announced to be read before the Applied Chemistry and Physic Section of the Society of Arts to-day, and the reading has, therefore, been unavoidably postponed.

TWO strong shocks of earthquake occurred at Chio on the night of May 20, bringing down several of the houses that remained standing after the late catastrophe.

ABOUT a year ago the Boston Society of Natural History celebrated its semi-centennial by a jubilee meeting, and decided to commemorate the event by publishing a memorial volume consisting of memoirs from those among its present associates, eminent in various departments, whose circumstances enabled them to contribute. The New York *Nation* now announces the publication of the volume, a magnificent 4to of 600 pages, with 50 plates.

AT a meeting of the Sanitary Institute of Great Britain, held on May 18, Dr. B. W. Richardson, F.R.S., in the chair, the discussion was continued upon the address given by the chairman, entitled "Suggestions for the Management of Cases of Small-Pox and Infectious Diseases in the Metropolis and Large Towns." Mr. Pearson Hill gave a number of facts relating to the Hampstead Small-Pox Hospital, and a letter was read from Dr. Tripe giving statistics relating to the hospital at Hackney. Dr. W. H. Corfield, Dr. Willoughby, Mr. Hempson Denham, and Mr. Bridgwater also took part in the discussion.

A TRAPPER who recently arrived with musk-rat skins at Kingston, Ontario, delared, according to the *Colonies*, that the animal is becoming rapidly extinct.

THE telephone is being introduced by the New Zealand Government into places where the telegraph does not exist. Between Collingwood and Motucka, a distance of fifty miles, a line has been opened, and is said to work admirably.

THE work of the International Electrical Exhibition Commission is progressing, spaces having been allotted to the several countries on the ground-floor of the central transept. Meanwhile the general arrangement of the first floor has been decided upon. A saloon has been reserved for the Brush light and machines, and another for the Sawyer incandescent light. Mr. Edison will have a special saloon for his inventions. M. Maisme's incandescent light will be employed in lighting a saloon. The Jamin, Jablockhoff, Werdermann, and some others will each have its saloon. One of these is to be lighted by Tommassi voltaic elements.

IT is announced from Nanaimo that further important discoveries of coal have been made in Vancouver Island.

IT is believed in New Zealand that petroleum exists in large quantities in the North Island, and two companies are now engaged in sinking for it.

THE additions to the Zoological Society's Gardens during the past week include an African Cheetah (*Felis jubata*), a Secretary Vulture (*Serpentarius reptilivorus*) from Africa, presented by Mr. James S. Jameson; a Plantain Squirrel (*Sciurus plantani*) from Java, a Chipping Squirrel (*Tamias striatus*) from North America, presented by Mr. W. Bassano; a Ceylonese Hawk-Eagle (*Spizaetus ceylonensis*) from Ceylon, presented by Mr. G. Lyon Bennett; a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by the Earl Brownlow; three Bull Frogs (*Rana mugiens*), a Noisy Frog (*Rana clamata*) from Nova Scotia, presented by Mr. Hugo Müller; two Green Lizards (*Lacerta viridis*), European, received in exchange; a Crested Guinea Fowl (*Numida cristata*), a Long-nosed Crocodile (*Crocodilus cataphractus*) from West Africa, on approval; six Speckled Terrapins (*Clemmys guttata*), two Painted Terrapins (*Clemmys picta*) from North America, purchased; an Eland (*Oreas canna*), an Axis Deer (*Cervus axis*), born in the Gardens; three Spotted-billed Ducks (*Anas pœcilorhyncha*), bred in the Gardens.

GEOGRAPHICAL NOTES

THE anniversary meeting of the Geographical Society was held on Monday last, Lord Aberdare, the President, occupying the chair. Mr. Markham read the report of the Council for the past year, which showed that, though the number of Fellows had not materially increased, the Society was in a satisfactory and prosperous condition, its assets being valued at close upon 40,000*l.*, exclusive of the map collection and library. After a reference to publications it was stated that an observatory which had been built and fitted up at the Society's house had been in constant use by students and others who wished to practise observing or to get their instruments adjusted. The large map of Eastern Equatorial Africa by Mr. Ravenstein was said to be approaching completion, but the rate of progress is seemingly slow, as only three out of twenty-four sheets are reported to be printed off. Catalogues of the library and map-collection are in various stages of preparation. The presentation of medals afterwards took place, when Capt. de Fonseca Vaz, naval attaché of the Portuguese Legation, received the Founder's for Major Serpa Pinto, acknowledging the honour in a very happy speech, in which allusion was made to his own geographical work on the Zambesi. Mr. C. R. Markham received the Patron's medal on behalf of Mr. Leigh Smith, who is at Peterhead, preparing for another voyage to Franz-Josef Land. The Schools' prize medals were afterwards distributed among the boys from the City of London School and Dulwich and London International Colleges, whose names we gave in a recent issue. Mr. Francis Galton announced some changes in future examinations, adding that Australia has been chosen as the special subject for 1881-2. The ballot having been taken for the new Council, Lord Aberdare was re-elected President, and Mr. R. H. Major was elected a Vice-President in the place of the Hydrographer of the Admiralty, and Mr. D. W. Freshfield and Lord Reay respectively Secretary and Foreign Secretary in the places of Mr. Major and Lord Arthur Russell. Among the new members of Council are Col. Grant, Gen. Pitt-Rivers, and Col. Yule. Lord Aberdare lastly read a short address, in which he gave an interesting retrospect of the geographical work of the past year. The usual dinner was held in the evening, when the principal speakers, besides Lord Aberdare, were Count Münster, Lord Houghton, and the American Minister.

THE French, like the Italians, intend to explore the depths of the Mediterranean this summer; the Minister of Marine has decided to place the *Travailleur* at the disposal of a commission charged with the work of deep-sea exploration. The vessel will set out for the Mediterranean about the end of June.

THE following explorations will be made during this summer by the Russian Mineralogical Society:—In the province of Kostroma, for the completion of a geologicacal map of the province, by M. Nikitin; the exploration of the Tertiary and Chalk formations in the province of Bessarabia, by Prof. Sintzoff; in the Polish province of Lublin, in continuation of the researches of the late Prof. Barbot-de-Marny; the examination of the relations between the old and the new Caspian formations in the province of Astrakhan, in the Bistchi and Djaksy-Sassyk Mountains.

THE *Zeitschrift* of the Berlin Geographical Society, No. 92, contains the conclusion of Herr Niederlein's paper "On the Scientific Results of an Argentine Expedition to the Rio Negro"; "Travels and Topographical Surveys in the North Chinese Province of Chi-li," by Dr. v. Mollendorff, with two maps; an interesting extract from a Hawaiian manuscript, by Dr. Ad. Bastian, and varieties from Australia, by Herr H. Greffrath. In the *Verhandlungen* of the same Society are papers: "On the Maories of New Zealand," by Dr. Beheim-Schwarzbach; "On Ice caves and Abnormal Ice-formations," by Prof. Schwalbe; and "On the South Carpathians," by Dr. Paul Lehmann.

AT a recent meeting of the Bengal Asiatic Society, Mr. F. A. de Roepstorff read some interesting notes on the in-

habitants of the Nicobar Islands, a subject to which he has paid much attention. As the result of his visit and investigations last year he concludes that there is an element of Papuan origin among the people of the interior, and that this is strongly mixed with another not curly-haired race. It is true, then, as has been suggested, that there is a curly-haired race in the interior of Great Nicobar, but whether the Andaman Negrito and this tribe are related is very doubtful. Mr. Roepstorff hopes that future researches may enable him to settle the matter.

It is asserted in Algiers that a letter from Itarem, Chief of the Hoggar Tuaregs, has been intercepted, taking credit to himself for the massacre of Col. Flatters' expedition. This is the man who sent two of his relatives to Col. Flatters as a sign of goodwill, with which that officer professed himself so well satisfied.

The *Times* correspondent states that it is announced by Dr. Nachtigal that the first diet of German geographers will be held at Berlin on June 7 and 8.

ON DISCONTINUOUS PHOSPHORESCENT SPECTRA IN HIGH VACUA[1]

In a paper which I had the honour of presenting to the Royal Society in March, 1879,[2] I drew attention to the fact that many substances when in high vacua and submitted to the molecular discharge by means of an induction coil, emitted phosphorescent light; and I especially mentioned the phosphorescent sulphides, the diamond, the ruby, and various other forms of alumina, crystalline and amorphous.

Pure alumina chemically prepared has very strong phosphorescence. Sulphate of alumina is dissolved in water, and to it is added an excess of solution of ammonia. The precipitated hydrate of alumina is filtered, washed, ignited, and tested in the molecular stream. It phosphoresces of the same crimson colour, and gives the same spectrum as the ruby.

Alumina in the form of ruby glows with a full rich red colour, and when examined in the spectroscope the emitted light is seen to be discontinuous. There is a faint continuous spectrum ending in the red somewhere near the line B; then a black space, and next an intensely brilliant and sharp red line, to which nearly the whole of the intensity of the coloured glow is due. The wavelength of this red line, which appears characteristic of this form of alumina is, as near as I can measure, λ 689·5 m.m.m. This line coincides with the one described by E. Becquerel as being the most brilliant of the lines in the spectrum of the light of alumina in its various forms, when glowing in the phosphoroscope.

This coincidence is of considerable interest, as it shows a relation between the action of molecular impact and of sunlight in producing luminosity. The phosphorescence induced in a crystal of ruby by the molecular discharge is not superficial, but the light comes from the interior of the crystal, and is profoundly modified according as its direction of vibration corresponds or makes an angle with the axis of the crystal, being quenched in certain directions by a Nicol prism.

Sunlight falling on the ruby crystal produces the same optical phenomena. The light is internally emitted, and on analysis by a prism is seen to consist essentially of the one brilliant crimson line, λ 689·5. This fact may account for the extraordinary brilliancy of the ruby, which makes it so highly prized as a gem. The sun not merely renders the red-coloured stone visible, as it would a piece of coral, but it excites the crystal to phosphorescence, and causes it to glow with a luminous internal light, the energy of which is not diffused over a broad portion of the spectrum, but is chiefly concentrated into one wave-length.

The crimson glow of alumina remains visible some time after the current ceases to pass. When the residual glow has ceased, it can be revived by heating slightly with a spirit-lamp.

After long experimenting with chemically pure alumina precipitated from the sulphate as above described, a curious phenomenon takes place. When sealed up in the vacuum two years ago it was snow white; but after being frequently submitted to the molecular discharge for the purpose of exhibiting its brilliant phosphorescence, it gradually assumes a pink tinge, and on examination in sunlight a trace of the alumina line can be detected. The repeated molecular excitation is slowly causing the amorphous powder to assume a crystalline form.

Under some circumstances alumina glows with a green colour. Ammonia in large excess was added to a dilute solution of alum. The strong ammoniacal solution filtered from the precipitated alumina was now boiled. The alumina which the excess of ammonia had dissolved was thereby precipitated. This was filtered off, ignited, and tested in the molecular discharge. It gave no red light whatever, but phosphoresced of a pale green, and on examination with a prism the light showed no lines, but only a concentration of light in the green.

Two earthen crucibles were tightly packed, the one with sulphate of alumina, the other with acetate of alumina. They were then exposed, side by side, to the most intense heat of a wind furnace—a heat little short of the melting-point of platinum.[1] The resulting aluminas were then tested in the molecular stream.

The alumina from the sulphate gave the crimson glow and spectrum line.

The alumina from the acetate gave no red glow or line, but a pale green phosphorescence.

In my examination of rubies, many pounds of which have passed through my apparatus, I have been fortunate enough to meet with one solitary crystal, not to the eye different from others, which emits a *green* light when tested in the molecular stream. All others act as I may call normally. The spectrum of this green-glowing crystal shows, however, a trace of the red line, and on keeping the discharge acting on it for a few minutes the green phosphorescence grows fainter and a red tinge is developed, the spectrum line in the red becoming more distinct.

Besides the ruby, other native forms of crystallised alumina phosphoresce. Thus corundum glows with a pink colour. The sapphire appears to be made up of the red-glow and the green-glow alumina. Some fine crystals of sapphire shine with alternate bands of red and green, arranged in layers perpendicular to the axis. Unfortunately it is impossible to prepare a tube for exhibition containing this variety of sapphire, as it is constantly evolving gas from the numerous fissures and cavities which abound in this mineral.

The red glow of alumina is chiefly characteristic of this earth in a free state. Few of its compounds, except Spinel (aluminate of magnesium), either natural or artificial, show it in any marked degree. All the artificially crystallised aluminas give a strong red glow and spectrum line. An artificially crystallised aluminium and barium fluoride phosphoresces with a blue colour, but shows the red alumina line in the spectrum. Spinel glows red, and gives the red line almost as strong as the ruby.

The mineral Spodumene (an aluminium and lithium silicate) phosphoresces very brilliantly with a rich golden yellow colour, but shows no spectrum line, only a strong concentration of light in the orange and yellow. A phosphorescing crystal of Spodumene has all the internal light cut off with a Nicol prism, when the long axes of the Nicol and the crystal are parallel.

It became of interest to see if the other earths would show phosphorescent properties similar to those of alumina, and especially if any of them would give a discontinuous spectrum; considerable interest attaching to a solid body whose molecules vibrate in a few directions only, giving rise to spectrum lines or bands on a dark background.

Glucina, prepared with great care, is found to phosphoresce with a bright blue colour, but no lines can be detected in the spectrum, only a concentration of light in the blue.

The rare mineral phenakite (aluminate of glucinum), sometimes used as a gem, phosphoresces blue like pure glucina, no trace of the alumina line being found in its spectrum. This mineral shows a residual glow after the current is turned off.

Thorina has very little, if any phosphorescence. This earth is however remarkable for its very strong attraction for the residual gas in the vacuum tube. On putting thorina in a tube furnished with well-insulated poles whose ends are about a millimetre apart in the centre, and heating strongly during exhaustion, the earth on cooling absorbs the residual gas with such avidity that the tube becomes non-conducting, the spark preferring to pass several inches in air rather than strike across the space of a millimetre separating the two poles. It is probable that this strong attraction for gas is connected with the great density of the earth thorina (sp. gr. = 9·4).

Zirconia gives a very brilliant phosphorescence, approaching

[1] Paper read before the Royal Society, May 19, by William Crookes, F.R.S.

[2] *Phil. Trans.* Part 2, 1879, p. 660.

[1] This operation was kindly performed for me by Messrs. Johnson and Matthey.

in intensity that of sulphide of calcium. The colour is pale bluish green, becoming whiter as the intensity of the discharge increases: no lines are seen in its spectrum.

Lanthana precipitated as hydrate and ignited shows no phosphorescence. After it has been heated for some time before the blowpipe it phosphoresces of a rich brown.

Didymia, from the ignition of the hydrate, has scarcely any phosphorescence; what little there is appears to give a continuous spectrum with a broad black band in the yellow-green. On examining the light reflected from this earth when illuminated by day or artificial light, the same black band is seen, and with a narrow slit and sunlight the band is resolved into a series of fine lines, occupying the position of the broadest group of absorption lines in the transmission spectrum of didymium salts.

Yttria shows a dull greenish light, giving a continuous spectrum.

Erbia phosphoresces with a yellowish colour, and gives a continuous spectrum, with the two sharp black bands so characteristic of this earth cutting through the green at λ 520 and 523. These lines are easily seen in the light reflected from erbia when illuminated by daylight. It is well known that solid erbia heated in a flame glows with a green light, and gives a spectrum which chiefly consists of two bright green lines in the same place as the dark lines seen by reflected light.

A curious phenomenon is presented by erbia when the spark passes over it at a high exhaustion. The particles of earth which have accidentally covered the poles are shot off with great velocity, forming brightly luminous lines, and, striking on the sides of the tube, rebound, remaining red hot for an appreciable time after they have lost their velocity. They form a very good visible illustration of radiant matter.

Titanic acid phosphoresces dark brown, with gold spots in places.

Stannic acid gives no phosphorescence.

Chromic, ferric, and ceric oxides do not appreciably phosphoresce.

Magnesia phosphoresces with a pink opalescent colour, and shows no spectrum lines.

Baryta (anhydrous) scarcely phosphoresces at all. Hydrated baryta, on the contrary, shines with a bright orange-yellow light, but shows no discontinuity of spectrum; only a concentration in the yellow-orange.

Strontia (hydrated) phosphoresces with a beautiful deep blue colour, and when examined in the spectroscope the emitted light shows a greatly increased intensity at the blue and violet end, without any lines or bands.

Lime phosphoresces of a bright orange-yellow colour, changing to opal blue in patches where the molecular discharge raises the temperature. In the focus of a concave pole the lime becomes red- and white-hot, giving out much light. This earth commences to phosphoresce more than 5 millims. below the vacuum, and continues to grow brighter as long as the electricity is able to pass through the tube. On stopping the discharge there is a decided residual glow. No lines are seen in the spectrum of the light.

Calcium carbonate (calcite) shows a strong phosphorescence, which begins to appear at a comparatively low exhaustion (5 m.m.). The interior of the crystal shines of a bright straw colour, and the ordinary and extraordinary rays are luminous with oppositely polarised light. Calcite shows the residual glow longer than any substance I have as yet experimented with. After the current has been turned off, the crystals shine in the dark with a yellow light for more than a minute.

Calcium phosphate generally gives an orange-yellow phosphorescence and a continuous spectrum. Sometimes, however, a yellow-green band is seen superposed on the spectrum.

Potash phosphoresces faintly of a blue colour. The spectrum shows a concentration at the blue end, but the light is too faint to enable lines, if any, to be detected.

Soda phosphoresces faintly yellow, and gives the yellow line in the spectrum.

Lithium carbonate gives a faint red phosphorescence. Examined in the spectroscope, the red, orange, and blue lithium lines are seen.

I have already said that the diamond phosphoresces with great brilliancy. In this respect perfectly clear and colourless stones "of the first water" are not the most striking, and they generally glow of a blue colour. Diamonds which in sunlight have a slight fluorescence, disappearing when yellow glass is interposed, generally phosphoresce stronger than others, and the emitted light is of a pale yellowish green colour.

Most diamonds which emit a very strong yellowish light in the molecular discharge give a continuous spectrum, having bright lines across it in the green and blue. A faint green line is seen at about λ 537; at λ 513 a bright greenish blue line is seen, and a bright blue line at λ 503, a darkish space separating the last two lines.

Diamonds which phosphoresce red generally show the yellow sodium line superposed on a continuous spectrum.

There is great difference in the degree of exhaustion at which various substances begin to phosphoresce. Some refuse to glow until the exhaustion is so great that the vacuum is nearly non-conducting, whilst others commence to become luminous when the gauge is 5 or 10 millimetres below the barometric level. The majority of bodies, however, do not phosphoresce till they are well within the negative dark space.

During the analysis of some minerals containing the rarer earths experimented on, certain anomalies have been met with, which seem to indicate the possible presence of other unknown elements awaiting detection. On several occasions an earthy precipitate has come down where, chemically speaking, no such body was expected; or, by fractional precipitation and solution from a supposed simple earth something has separated which in its chemical characters was not quite identical with the larger portion; or, the chemical characteristics of an earth have agreed fairly well with those assigned to it in books, but it deviated in some physical peculiarity. It has been my practice to submit all these anomalous bodies to molecular bombardment, and I have had the satisfaction of discovering a class of earthy bodies which, whilst they phosphoresce strongly, also give spectra of remarkable beauty.

The spectrum seen most frequently is given by a pale yellowish coloured earth. It consists of a red, orange, citron, and green band, nearly equidistant, the citron being broader than the others and very bright. Then comes a faint blue, and lastly two very strong blue violet bands. These bands, when seen at their best, are on a perfectly black background; but the parent earth gives a continuous spectrum, and it is only occasionally, and as it were by accident, that I have so entirely separated it from the anomalous earth as to see the bands in their full purity. Another earthy body gives a spectrum similar to that just described, but wanting the red, and having a double orange and double citron band. A third gives a similar spectrum, but with a yellow line interposed between the double orange and the double citron, and having two narrow green lines.

At present I do not wish to say more than that I have strong indications that one, or perhaps several, new elements are here giving signs of their existence. The quantities I have to work upon are very small, and when each step in the chemical operation has to be checked by an appeal to the vacuum-tube and to the induction-coil the progress is tediously slow. In the thallium research it only occupied a few minutes to take a portion of a precipitate on a platinum loop, introduce it into a spirit-flame, and look in the spectroscope for the green line. In that way the chemical behaviour of the new element with reagents could be ascertained with rapidity, and a scheme could be promptly devised for its separation from accompanying impurities. Here however the case is different: to perform a spectrum test, the body under examination must be put in a tube and exhausted to a very high point before the spectroscope can be brought to bear on it. Instead of two minutes, half a day is occupied in each operation, and the tentative gropings in the dark, unavoidable in such researches, must be extended over a long period of time.

The chemist must also be on his guard against certain pitfalls which catch the unwary. I allude to the profound modification which the presence of fluorine, phosphorus, boron, &c., causes in the chemical reactions of many elements, and to the interfering action of a large quantity of one body on the chemical properties of another which may be present in small quantities.

The fact of giving a discontinuous phosphorescent spectrum is in itself quite insufficient to establish the existence of a new body. At present it can only be employed as a useful test to supplement chemical research. When, however, I find that the same spectrum-forming earthy body can always be obtained by submitting the mineral to a certain chemical treatment; when the chemical actions which have separated this anomalous earth are such that only a limited number of elements can possibly be present; when I find it impossible to produce a substance giving

a similar discontinuous spectrum by mixing together any or all of the bodies which alone could survive the aforesaid chemical treatment;—when all these facts are taken into consideration, and when due weight is given to the very characteristic spectrum reaction, I cannot help concluding that the most probable explanation is that these anomalies are caused by the presence of an unknown body whose chemical reactions are not sufficiently marked to have enabled chemists to differentiate it from associated elements.

THE DISTANCES OF THE STARS[1]

EVERY one who is acquainted with the rudiments of astronomy knows that the sun with its attendant planets is merely an island group in the vast realms of space.

An island the size of this room in the middle of the Atlantic would hardly be more remotely apart from the surrounding shores than is our solar system from the bodies which surround it in space. To determine the distance from this solar system to the stars which surround it is the problem for our consideration to-night.

Recent Researches on 61 *Cygni.*—It is now almost exactly forty years (February 12, 1841) since the gold medal of the Royal Astronomical Society was awarded to Bessel for his discovery of the annual parallax of 61 Cygni. On that occasion Sir John Herschel delivered an address, in which he glanced at the labours of Struve and of Henderson as well as those of Bessel. The discovery of the distances of the stars was alluded to as "the greatest and most glorious triumph which practical astronomy has ever witnessed." From this date the history of our accurate knowledge of the subject may be said to commence. Each succeeding race of astronomers takes occasion to investigate the parallax of 61 Cygni anew, with the view of confirming or of correcting the results arrived at by Bessel.

[The parallactic ellipse which the stars appear to describe, having been briefly explained, the method of deducing the distances of the stars was pointed out.]

The attention of Bessel was directed to 61 Cygni by its proper motion of five seconds per annum. When Bessel undertook his labours in 1838 the pair of stars forming the double were in the position indicated on Fig. 1. When O. Struve attacked the problem in 1853 the pair of stars forming 61 Cygni had moved considerably. Finally, when the star was observed at Dunsink in 1878, it had made another advance in the same direction as before. In forty years this object had moved over an arc of the heavens upwards of three minutes in length.

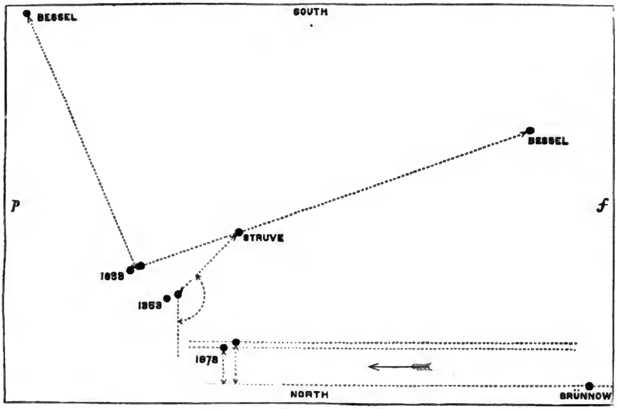


FIG. 1.—61 Cygni and parallax comparison stars.

The diagram contains four other stars besides the three positions of 61 Cygni. These are but small telescopic objects, they do not participate in the large proper motion of 61 Cygni, and they are undoubtedly much more remote from us. Bessel chose as the comparison stars the two objects marked with his name. He measured the distance from the central point of 61 Cygni to each of the two comparison stars. From a series of such measures he discovered the parallactic ellipse of 61 Cygni. He was led to the same ellipse by each of the two comparison stars.

Fifteen years later (1853) Struve undertook a new determination. He chose a comparison star different from either of those Bessel had used. Struve's method of observing was also quite different from Bessel's. Struve made a series of measures of the distance and position of the comparison star from 61 (B) Cygni. Struve also succeeded in measuring the parallactic ellipse.

There was, however, an important difference between their results. The distance, according to Bessel, was half as much again as Struve found. Bessel said the distance was sixty billions of miles; Struve said it could not be more than forty billions.

The discrepancy may be due to the comparison stars. If Bessel's comparison stars were only about three times as far as 61 Cygni, while Struve's star was about eight or ten times as far, the difference between Struve's results and Bessel's would be accounted for.

To settle the question, observations were subsequently made by Auwers and others; the latest of these investigations is one which has recently been completed at Dunsink Observatory.

Dr. Brünnow proposed and indeed commenced a series of measures of the difference in declination between 61 Cygni and a fourth comparison star. These observations were made with the south equatorial at Dunsink. The carrying out of this work devolved on the lecturer, as Dr. Brünnow's successor. Two series of observations have been made, one with each of the components of 61 Cygni. The results agree very nearly with those of Struve.

[1] Lecture at the Royal Institution of Great Britain, on Friday, February 11, by Prof. Robert S. Ball, LL.D., F.R.S., Astronomer Royal of Ireland.

On a review of the whole question there seems no doubt that the annual parallax of 61 Cygni is nearer to the half second found by Struve, than to the third of a second found by Bessel.

To exhibit the nature of the evidence which is available for the solution of such a problem, a diagram showing the second series of observations has been prepared (Fig. 2). The abscissæ are the dates of the second series of observations made at Dunsink. The ordinates indicate the observed effect of parallax on the difference of declinations between 61 (B) Cygni and the comparison star. Each dot represents the result of the observations made on the corresponding night. The curve indicates where the observations should have been with a parallax of $0''.47$, the effect of the parallax in declination being only $0''.40$. The discordances are not so great as might perhaps be at first thought. The distance from the top of the curve to the horizontal line represents an angle of four-tenths of a second. This is about the apparent diameter of a pennypiece at the distance of ten miles. The discordance between the observations and the curve is in no case much more than half so great. It therefore appears that the greatest error we have made in these observations amounts to but two or three tenths of a second. This is equivalent to the error of pointing the telescope to the top edge of a penny-piece instead of to the bottom edge when the penny-piece was fifteen or twenty miles off.

The entire quantity to be measured is so small that the errors, minute as they are, bear a large proportion to the parallax. In this lies the weakness of such work. By sufficiently increasing the number of the observations, and by discussing them with the aid of the method of least squares, considerable confidence may be attached to the results.

Groombridge 1830.—This star has been the subject of much parallax work. It has a telescopic proper motion of seven seconds annually. Mr. Huggins or Mr. Christie could perhaps ascertain by the spectroscope what its motion may be in the line of sight. From the theory of probabilities the total proper motion may not improbably be nine seconds. We shall however take it at seven seconds. The parallax has been determined by Struve and by Brünnow. It is very small, being one-tenth of a second. The actual velocity of 1830 Groombridge must therefore be at least 70 radii of the earth's orbit per annum, or 200 miles per second.

Newcomb has employed this result to throw light on the question as to whether all our stars form one system. If an isolated body in our system is to remain there for ever, the theory of gravitation imposes the imperative condition that the velocity of the body must not exceed a certain amount. Assuming that the stars are 100,000,000 in number, and that each star is five times as large as the sun, assuming also that they are spread out in a layer of such dimensions that a ray of light takes 30,000 years to pass it, Newcomb shows that the critical velocity is twenty-five miles per second.

As this is only the eighth part of the velocity of Groombridge 1830, we are thus led to the dilemma that either the masses of the bodies in our system must be much greater than we have supposed, or Groombridge 1830 is a runaway star, which can never be controlled and brought back.

Search for Stars with Parallax.—The lecturer has been engaged for some years at Dunsink Observatory in a systematic search for stars which have an appreciable parallax. Up to the present about three hundred stars have been examined. In the majority of cases each of these stars has been observed only twice. The dates of the observations have been chosen so as to render the effects of parallax as manifest as possible. It is not of course expected that a small parallax of a few tenths of a second could be detected by this means. The errors of the observations would mask any parallax of this kind. It seems however certain that no parallax could have escaped detection if it equalled that of α Centauri, *i.e.* one second of arc.

The stars examined have been chosen on various grounds. It had been supposed that some of the red stars were possibly among the sun's neighbours, and consequently many of the principal red stars were included in our list. No conspicuous parallax has however been detected in any of the red stars up to the present. Many of the principal double stars are also included in the list. Other stars have been added on very various grounds; among them may be mentioned the Nova, which some time ago burst out in the constellation Cygnus, and dwindled down again to a minute point. The earth's orbit however does not appear any larger when seen from Nova Cygni than from any of the other stars on our list.

Groombridge 1618.—We have however found one star which seems to have some claim to attention as one of the sun's neighbours. The star in question is Groombridge 1618. It lies in the constellation Leo, and is 6·8 magnitude. Groombridge 1618 has a proper motion of $1''.4$ annually. From a series of measurements of its distance made on fifty-five nights from a suitable comparison star the parallax of Groombridge 1618 appeared to be about one-third of a second. As this seemed to be a result of considerable interest, measures were renewed for a second series of forty nights. The result of the second series confirms the first. Measurements of the position angle were also made at the same time. Some difficulties not yet fully explained have arisen, but on the whole the measurements of the position angle seem to confirm the supposition that the parallax of Groombridge 1618 is about one-third of a

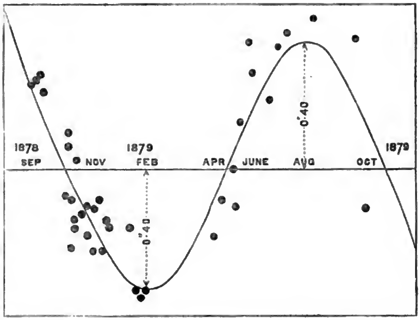


Fig. 2.—Parallax in declination of 61 (B) Cygni. Abscissæ indicate dates; ordinates indicate parallax; dots indicate observations.

second. The orbit of the earth viewed from Groombridge 1618 is about the same size as a pennypiece at the distance of seven miles.

Secular Proper Motions.—Geologists have made us acquainted with the enormous intervals of time which have elapsed since the earth first became the abode of living animals. Regarding a period of 50,000,000 of years as comparable with geologic time, some considerations were adduced as to the effect of proper motions during such an interval. It was pointed out that in all probability none of the stars now visible to the unaided eye can have then been visible from the earth.

The Nature of Space.—The possible connection of parallax work with the problems of the nature of space was then alluded to. It was shown that if space be hyperbolic the observed parallax is smaller than the true parallax, while the converse must be the case if space be elliptic. The largest triangle accessible to our measurements has for base a diameter of the earth's orbit, and for vertex a star. If the *defect* of the sum of the three angles of such a triangle from two right angles be in any case a measurable quantity, it would seem that it can only be elicited by observations of the same kind as those which are made use of in parallax investigations.

THE SECULAR INEQUALITIES IN TERRESTRIAL CLIMATES DEPENDING ON THE PERIHELION LONGITUDE AND ECCENTRICITY OF THE EARTH'S ORBIT

A PAPER on this subject, by the Rev. Dr. Haughton of Trinity College, Dublin, was read before the Royal Society on February 24 last. Dr. Haughton shows that the two inequalities in question depend upon terrestrial radiation only, and in no way upon sun heat.

Having noticed that the hottest and coldest time of day *follows* noon and midnight by an interval often considerable; and in like manner that the hottest and coldest days in the year *follow* midsummer and midwinter [1] by an interval often of many days; Dr. Haughton saw in these facts a close analogy with the diurnal tides, which follow the sun or moon's meridian passage by an interval of some hours.

Dr. Haughton was thus led to solve the differential equation on which the problem depends, by assuming an expression similar to those so well known and so long employed in the mathematical discussion of the tides of the ocean.

The result fully justified the assumption of expressions similar to diurnal tidal expressions, for when the differential equation is integrated for a day and summed for a year, all the periodic terms disappear, and nothing is left but terms depending on the perihelion longitude and eccentricity, which represent the exact mathematical expression of the two inequalities first noticed by Adhémar and Croll.

The final result takes the form—

Mean annual temperature

$$= k\left((\Theta_0 + a) \pm (a_1 \cos \varpi + \beta_1 \sin \varpi)e\right) \quad . \quad . \quad (1)$$

where

$k_{,}$ = Constant,

Θ_0 = Mean annual temperature of place,

$a_{,}$ = "Control" [2] temperature of atmosphere at place.

a_1 and β_1 are defined by the following equations:—

$2\sqrt{a_1^2 + \beta_1^2}$ = Range of annual temperature.

$\frac{\beta_1}{a_1}$ = Tangent of the arc which represents the retardation of the maximum and minimum temperature.

ϖ = Longitude of earth's perihelion.

e = Eccentricity of earth's orbit.

Using Ferrel's temperature tables, Dr. Haughton finds the following maximum secular ranges of mean annual temperature:—

Maximum Secular Range

Latitude.	Northern hemisphere.	Southern hemisphere.
°	°	°
0	0·185 F.	0·185 F.
10	0·375 ,,	0·585 ,,
20	1·100 ,,	0·875 ,,
30	2·065 ,,	1·110 ,,
40	2·750 ,,	0·985 ,,
50	3·685 ,,	0·710 ,,
60	4·610 ,,	0·540 ,,
70	4·985 ,,	—
80	4·925 ,,	—

This table shows that the average maximum effect of the astronomical causes involved in perihelion longitude and eccentricity never can exceed 5° F. in the northern hemisphere, and barely exceeds 1° F. in the southern. At particular localities, where there is a great range of annual temperature, the effect may be somewhat greater. For example, at North Grinnel Land the range becomes 6°·5 F. It will be seen how little benefit this would confer upon that locality, when it is remembered that the present mean annual temperature of North Grinnell Land is 2°·42 F. *below* zero, and that by the secular range it could be raised to 0°·21 F. *above* zero, or depressed to 6°·29 *below* zero.

At Discovery Harbour Tertiary plant beds were found by the Arctic explorers, which indicate a *July temperature greater than 63°·7 F.*; the present July temperature of Discovery Harbour is 37°·2 F. above zero, or *only five degrees above the freezing point of water.* How is this remarkable change in climate to be accounted for? Geologists cannot much longer evade answering such questions as these.

[1] In the British Islands January 15 is reckoned the time of maximum cold, which is twenty-four days after midwinter.

[2] By this is meant the temperature of the upper layers of the atmosphere of place, which controls the radiation; this temperature varies with the latitude, and is probably always below zero Fahrenheit.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Cambridge Museums are every year the scene of a large increase of work, new departments being continually added, even under the present conditions of impecuniosity. The annual reports recently issued on the condition and progress of all the departments testify to much excellent work. The death of Prof. Miller, who for forty-nine years occupied the Chair of Mineralogy, has brought a valuable bequest to the museum he presided over, in the shape of 300 volumes of books and many specimens and scientific instruments, including his famous goniometer and the physical apparatus, balances, thermometers, and barometers employed by him in his investigations for the restoration of the standards of weight. Prof. Lewis, who has succeeded to the Chair of Mineralogy, ably assisted by Mr. Solly, is cataloguing the museum, and already half the work is accomplished. Valuable specimens besides Prof. Miller's bequest have been added to the museum by purchase as well as by donation, including a small case of minerals belonging to the late Dr. E. D. Clarke, given by the Rev. B. S. Clarke, Boxted, Colchester. Prof. Lord Rayleigh's Apparatus Fund has reached 2025*l.*, including 500*l.* from himself, which has been spent partly upon instruments required to be multiplied, in consequence of the increased number of students; but the additions during the year also comprise a large electro-magnet with heavy glass and nicol; a 2½-inch achromatic telescope; Joule's apparatus for investigating the maximum density of water; telephones and various optical apparatus, including new double refractive apparatus for combining prismatic colours. Many important pieces of apparatus have been made for the Cavendish Laboratory in Prof. Stuart's laboratory. Professors Liveing and Dewar report that with the 200*l.* alloted to them they have purchased several pieces of apparatus permanently useful in a variety of investigations. Prof. Stuart's department (mechanism) has progressed very considerably during the year. The apparatus, machines, tools, and materials in connection with the department have been recently valued at over 1500*l.*, of which only about 250*l.* is University property, the rest having been provided by the private enterprise of Prof. Stuart. In the Geological Museum, despite the want of suitable workrooms, Mr. Tawney has succeeded in arranging the petrological collection. Mr. Keeping reports the addition of many important fossil specimens both from England and America. Prof. Humphry reports further additions to the rich collection of human skulls under his charge. Mr. J. W. Clark, Superintendent of the Museum of Zoology and Comparative Anatomy, calls special attention to the beautiful coloured drawings of animals that cannot be preserved in spirit, or are too small to be seen without a microscope, added by Prof. A. C. Haddon, late Curator in Zoology. His successor, Mr. A. H. Cooke, Fellow of King's College, has commenced the work of determining and cataloguing the Woodward and Hepburn collections of shells. Mr. Clark has added a series of preparations showing the structure of the manatee, from a specimen presented by the Directors of the Brighton Aquarium. The very fine skeleton of the musk-ox brought home by the North German Polar Expedition of 1872 has been purchased. The skeleton of *Ceratodus Forsteri*, from the specimen presented by Prof. Liversidge, has been prepared in the museum. Many additions have also been made to the reptilian, ornithological, and other series. Dr. Michael Foster notes that his classes for histological work have become so large that a new bench, less convenient as to light, has been added. The numbers attending his courses are between sixty and seventy men and twenty women for the elementary classes, and fifteen men for the advanced classes. He remarks that his students would profit more if not so much harassed by striving to attend too great a number of lectures and courses. Prof. Babington records a large amount of herbarium work, including the naming of Gardner's collection of Brazilian plants, numbering 5000 specimens, presented by the professor. He has also obtained, at a very moderate cost, the entire collection of the late M. Gaston Genevier of Nantes, consisting of about 7000 species from France, Spain, Algeria, Asia Minor, &c., and all the typical specimens—over 500 in number—of the Rubi, described in his monograph of the genus Rubus. A proposal has been made by the Cambridge Philosophical Society to make their large and useful scientific library available for scientific students generally, and to allow it to be the nucleus of a much-needed library of science in the new museums, if the University will provide the salary of a librarian

for it. It is desired that this library shall be placed in a room vacated by Dr. Michael Foster's classes, and formed by the amalgamation of two old classrooms.

THE VICTORIA UNIVERSITY.—The Preliminary Examinations for the year 1881 will be held at the Owens College on June 20 and following days, and on October 5 and following days. Regulations:—1. Candidates for these examinations are required to present certificates of matriculation in the University. 2. The days fixed for matriculation are June 13 and 14, between the hours of two and four p.m., and October 1 and 3. 3. Students, on presenting themselves for matriculation, are required to furnish to the Registrar of the University certificates of admission as students of one of the colleges of the University, to pay a fee of 2*l*., and to sign an undertaking to obey the regulations of the University. 4. The October Preliminary Examination is open only to students who have matriculated since the Preliminary Examination held in the previous June, or who failed in this examination, or were prevented attending it by reasons satisfactory to the General Board of Studies. Candidates are requested to communicate with the Registrar, Prof. Adamson, who will supply them with the detailed syllabus of subjects, regulations, and time-table for the examination.

ROYAL UNIVERSITY OF IRELAND.—The copy of the scheme for the organisation of the University as adopted by the Senate has now been laid, pursuant to Act of Parliament, before the House of Commons, and it has been, by order of that House, printed. It gives full details of the degrees to be granted, which are in Arts a Bachelor, a Master, and a Doctor of Literature degree; in Science a Doctor's degree; in Engineering a Bachelor and a Doctor's degree; in Law, Music, and in Medicine the same; in surgery a Master's degree, with a special diploma in Obstetrics and in Sanitary Science. All these degrees are open to persons of either sex. The examinations for women shall be held apart from those for men, but on the same days. Candidates for any degree must have passed the Matriculation Examination, which will be held not only in Dublin but at certain local centres. The examination will be held in the subjects of Latin, English, Elementary Mathematics, Experimental Physics, and in any one of the following languages: Arabic, Celtic, French, German, Greek, Hebrew, Italian, Sanskrit, or Spanish. Candidates must also pass a first University Examination, to which they will only be admitted after the lapse of one academical year from matriculation, the subjects for this being a more advanced course of that fixed on for matriculation. One year after this is passed the student in Arts may proceed to his second University examination, in which he will have his choice of a great variety of subjects, but Latin, Greek, and English on the one hand, or Mathematics on the other, are compulsory. At this stage of his career the student may select Biology, including Physiology, Botany, and Zoology, or Geology, and after the expiration of one more year he can proceed to his B.A. examination, for which he will be permitted to select either the Classics or Mathematics, with the selection of one other of a long list of subjects given. For the M.A. examination the candidate must be a B.A. of one year's standing at the least, and he may answer in any one of a selected group of subjects. The regulations for the degrees of Doctor of Literature and Doctor of Science are not yet matured. Twelve scholarships of 50*l*. each are to be offered each year for competition, four in Classics, four in Mathematics, and four in Modern Literature. Exhibitions varying from 100*l*. to 15*l*. will be given to Honour Men. There are to be forty-eight Fellows. The salary of a Fellow, if he be not also a Fellow or Professor of some other University or College attached to a University endowed with public money, shall be 400*l*. a year. If he be such, then he shall only receive so much as will bring his salary up to 400*l*. a year. These Fellows shall constitute a Board of Examiners. There shall be also fourteen junior Fellows, their salary to be 200*l*. a year. No Fellow or Professor of any other College or University is eligible, and the candidates must be Graduates of the Royal University of four years standing. All Fellowships are tenable for seven years. Thus if a senior Fellow be elected from an already endowed College, the chances are that while he will have to do his full share of the work, he will receive only as much salary as will bring his total emoluments to 400*l*. Thus a Professor of one of the Queen's Colleges (Belfast or Cork) if elected would only receive 5*l*. or 10*l*. a year, but if a Professor from the Catholic College in Dublin were elected, as it is not endowed, he could receive a full 400*l*. a year, and yet his duties would be —so far as the Royal University is concerned—the same as his colleague from the endowed College, who would receive almost no salary at all. Thus a scheme for endowing Colleges through the resources of the Royal University has been at last successfully carried out. The subjects and books for the various examinations appear to be most judiciously selected, and in many respects might teach a lesson to our older Universities. The Senate close their scheme by a request that provision may be made for securing for the University a proper Senate Hall, Examination Rooms, a Library, &c., and urge that these should be all built within the area of the City of Dublin.

ETON.—Mr. G. C. Bourne of Eton College has been elected to a Natural Science Exhibition of 50*l*. a year for four years at New College, Oxford, for proficiency in Biology. Mr. Bourne is one of the foremost athletes of his school, having rowed in the Eton crew at Henley Regatta for the last three years, as he will again in a few weeks' time. For the past two years he has filled the exalted but responsible post of "captain of the boats," but has nevertheless found time to devote himself successfully to his favourite study, and has gained new honours for his school in a field hitherto untrodden by Etonians.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, April.—The wearing power of steel rails in relation to their chemical composition and physical properties (continued), by Dr. Dudley.—Experiments on the strength and stiffness of small spruce beams, by Mr. Kidder.—Observations on the water-supply of Philadelphia, by Mr. Haines.—A fourth state of matter, by Mr. Outerbridge, jun.—The moon of Earth and Jupiter, by Dr. Chase.

Bulletin de l'Academie Royale des Sciences de Belgique, No. 2.—Note on the determination of the longitude of Karema, by Capt. Cambier.—New data on the non-existence of pentathionic acid, by M. Spring.—On a new fossil fish of the environs of Brussels and on certain enigmatic bodies of the crag of Antwerp, by M. van Beneden.—On phosphate-beds in Belgium (third note), by M. Petermann.—On the theory of polars, by M. Le Paige.—On a new form of reddish frog from the south-east of France (*Rana fusca Honnorati*), by M. Héron Roger.—Study on the hypophysis of Ascidians and the neighbouring organs, by M. Julin.

Bulletin de l'Academie Imperiale des Sciences de St. Petersbourg, t. xxvii. No. 2.—Development of the absolute perturbations of a comet, by O. Backlund.—Champignons recently collected in Mongolia and Northern China, by C. Kulchbrenner and F. de Thümen.—Observations of Jupiter's spots, by M. Kortazzi.—On the oxidation products of erythrite, by S. Przybytek.—The money of the Ileks, ancient Khans of Turkestan, by B. Dorn.—Remarks on the group of the Pteroclides, by M. Bogdanoud.—Relations between isobars and isanomalies of temperature, by H. Wild.—Influence of pressure on the electric resistance of metallic wires, by O. Chwolson.—The Russian species of humble-bees in the collection of the Academy, by F. Monawitz.—On the value of errors depending on the retardation or prematurity of impulsions in Weber's methods for measuring instantaneous electric currents, by O. Chwolson.

Archives des Sciences Physiques et Naturelles, No. 4, April 14.—Study on the chemical composition of albuminoid substances, by Dr. Danilewsky.—Automatic methanometer, or automatic analyser of fire-damp, by M. Monnier.—Researches on vegetation, by Prof. Westmann.—Distillation and rectification of spirits by the rational use of low temperatures, by M. Pictet.—On phyllotaxy (continued), by M. de Candolle.

Rivista Scientifico-Industriale, No. 7, April 15.—Second reply in defence of the true theory of the siphon, by Prof. Marangoni.—Determination of the specific gravity of solids soluble in all liquids, by Dr. del Lupo.—Relation of the specific gravity and the pressure of saturated steam, by Prof. Ciccone.

THE last number of the Russian *Journal* of the Chemical and Physical Society (vol. xiii. fasc. 4) contains the following papers:—On the rate of chemical reactions, by M. N. Kayander.—On the influence of chemical structure on the refrigerating power of organic bodies, by M. J. Kanonnikoff.—On the laws of double decompositions, by M. A. Potilitzin.—On the chemical value of the constituents of alcohols, by Prof. Menshutkin.—On ice under "critical pressure," by Prof. Boutleroff.—On electricity of con-

tact, by MM. Stoletoff and Sokoloff.—On the influence of pressure on galvanic resistance, by M. Khwolson.—On dynamo-electric machines without iron, by M. Latchinoff.—On the voltaic arc, by M. Sloughinoff.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 12.—"Physiological Action of β Lutidine." By Greville Williams, F.R.S., and W. H. Waters, B.A.

Up to the present the authors' investigations have chiefly related to the action of this poison upon the heart and central nervous system of the frog.

Various methods were used to study its effect upon the heart, and each gave most distinct results pointing to an increase of the tonicity. After the introduction of a small quantity of β lutidine into the system, stimulation of the vagus failed to cause a cessation of the heart's beat.

In frogs retaining their spinal cord the injection of the alkaloid removed all powers of reflex action, which removal the authors proved by other experiments to be dne to the β lutidine acting on the reflex centre. The alkaloid was found to be antagonistic to strychnine: removing strychnine-tetanus when injected after that alkaloid and preventing its appearance when injected beforehand.

Chemical Society, May 19.—Prof. Roscoe, president, in the chair.—The following papers were read:—On ammonium nitrite and the reaction between hydrogen and nitric oxide in the presence of spongy platinum, by L. T. Wright. The author has repeated the experiments recently made by G. S. Johnson, who stated that the synthesis of ammonia was effected by passing hydrogen and nitrogen over heated spongy platinum. The author states that the nitrogen was contaminated with nitric oxide. The substance used by Johnson—ferrous sulphate solution—for freeing the nitrogen from nitric oxide does not completely absorb that gas. When pure nitrogen obtained by the action of potassinm hypobromite on ammonium chloride, or by passing the nitrogen evolved by heating ammonium nitrite through an alkaline sulphite, was used no ammonia was formed. Hydrogen reacts npon nitric oxide in the presence of cold spongy platinum to form ammonia.—On the synthetical production of urea from benzol, ammonia, and air by the action of heated platinum, by E. F. Herroun. The author has aspirated air through benzol and ammonia, and then passed the mixed vapours over a heated spiral of platinum wire. Urea was formed, which was identified by its reactions and analysis. Acetylene can be substituted for benzol vapour.—On a proposed volumetric method for the ready estimation of a soluble sulphite and free sulphurous acid, or of free sulphurous and sulphuric acids even in the presence of sulphates, by O. V. Pisani.—On the identification of crystallised alkaloids by the microscope, and the use of polarised light, by A. Percy Smith.—On the colour-properties and colour-relations of the metals of the iron-copper group, by T. Bayley. The author continues in this paper his investigations as to the quantities of cobalt and nickel, or of cobalt, copper, and iron, which, when mixed as sulphates, produce colourless grey solutions.—On the effects of the growth of plants on the amount of matter removed from the soil by rain, by E. W. Prevost.—On the action of sodium on cinnamic ether, by F. Hatton.

Physical Society, May 14.—Prof. Fuller in the chair.—New Members: Mr. D. J. Blakely and Mr. Walter Kilner.—Prof. G. C. Foster read a communication from Prof. Rowland and Mr. E. H. Nichols of Baltimore, U.S., on electric absorption in crystals. According to the theory of Clausius, Maxwell, and others there should be no electric absorption in the case of perfectly homogeneous substances. Prof. Rowland tested this deduction in the case of glass, which is not quite homogeneous, quartz, and calcite. This was done by placing the material as the dielectric in a condenser formed of two amalgamated copper plates. The condenser was charged by six Leyden jars, and the absorption measured by a quadrant electrometer. The results were that quartz had about one-ninth the absorptive power of glass, and calcite none at all. Dr. Hopkinson said that the *kind* of glass was important, and threw doubts on the theory that the absorption was due to heterogeneity; paraffin wax had little absorptive power, and yet was very heterogeneous. Professors Perry and Ayrton thought that two non-homogeneous substances in combination might have no residual charge. Mr. Lewis Wright suggested that the optical character of crystals should be considered in these experiments, which might be extended to other crystals. Calcite is uniaxial.—Prof. Minchin, of Cooper's Hill, Engineering College, described his new absolute sine electrometer. This consists of two metal plates, in one of which is an aperture nearly closed by a metal trap-door suspended from the plate by two fine platinum wires, and resting against fine stops, when the plates are hung vertically. These plates are connected to the poles of the cell to be measured, and tilted out of the vertical till the attraction of the whole plate on the suspended trap or shutter is just balanced by the weight of the latter. The electromotive force is then proportional to the sine of the angle of displacement. Dr. Lodge remarked that the apparatus combined sensitiveness with practicability. The E.M.F. of a single cell could be measured by it, whereas Thomson's absolute electrometer could only give the total of a number of cells. Prof. Ayrton stated that he and Prof. Perry hoped to modify the instrument in the direction of sensitiveness by adding another plate and giving it a high charge. Dr. Coffin suggested reversing the process of taking an observation.—Prof. Foster read a paper by Dr. J. E. Mills, on the ascent of hollow glass balls through liquids. A glass ball of a pear shape rises through a liquid with a sensibly uniform velocity, which varies with the liquid. The time of ascent is proportional to the square of the diameter of the vessel, and depends of course on the specific gravity of the contents of the bulb. Dr. Mills measures the density of gases and liquids in this manner. Prof. Perry thought that the bulb should be of a shape having no re-entrant angles.

Geological Society, May 11.—R. Etheridge, F.R.S., president, in the chair.—Joseph Deeley, George Kilgour, Griqualand West, South Africa, and Roderick William MacLeod were elected Fellows of the Society.—The following communications were read:—Notes on the fish-remains of the bone-bed at Aust, near Bristol, with the description of some new genera and species, by James W. Davis, F.S.A., F.G.S. The fossil fishes described in this paper are from the Rhætic bed at Aust Passage. The fishes belong to the orders Plagiostomi and Ganoidei, some of the former being of considerable size. It is inferred, from the intermixture of Saurians and fishes, that the deposit is the result of shallow water existing near land, in which the fishes lived and the Saurians occasionally disported themselves. Besides the fossil remains of the animals which lived during the deposition of the Aust-beds, there are also others which appear to have been derived from the Mountain Limestone and the Coal-measures, representing such genera as *Psammodus, Psephodus, Helodus,* and *Ctenoptychius*.—On some fish-spines from the Coal-measures, by J. W. Davis, F.S.A., F.G.S.—The author described in this paper three species of a new genus of fossil fish from the Carboniferous formation, two of the species having been found in the Cannel coal of the West Riding of Yorkshire, and the other in the Burghlea limestone, near Edinburgh. *Anodontacanthus* is a straight spine, offering many points of resemblance to some of the Pleuracanths; it has a similarly close-grained microscopical structure, the internal cavity opens terminally at the base of the spine, and it was not deeply implanted in the flesh of the fish. It however differs from all the Pleuracanths in being quite free from external denticles; its surface is plain or but slightly striated, whilst that of *Pleuracanthus* always possesses a double row of denticles either ranged laterally along the exposed part of the spine or in some position between the lateral and posterior aspects of the spine. It is possible that evidence may be discovered which will render necessary the removal of these spines to the genus *Pleuracanthus;* but at present there is no evidence that such is advisable. All the specimens of *Pleuracanthus*-spine fourd associated with teeth or shagreen have been armed with the double row of denticles, and at present no evidence exists that spines without denticles were associated with remains of this genus. It is therefore considered best to institute a new genus for the three species with the name *Anodontacanthus,* in allusion to its having no teeth or denticles.—On some specimens of *Diastopora* and *Stomatopora* from the Wenlock limestone, by Francis D. Longe, F.G.S. Mr. Longe showed and described some specimens of Bryozoa from the Wenlock limestone of Dudley, which he compared with corresponding forms from the Oolites and later periods, and pointed out the close similarity of the Silurian with the later forms, in respect of the shape and dimensions of the cells, as well as in the habit of cœnœcic growth.—On a new species of *Plesiosaurus*

(*P. Conybeari*) from the Lower Lias of Charmouth, with observations on *P. megacephalus*, Stutchbury, and *P. brachycephalus*, Owen, by Prof. W. J. Sollas, M.A., F.R.S.E., F.G.S., &c., Professor of Geology in University College, Bristol; accompanied by a supplement on the geological distribution of the genus *Plesiosaurus*, by G. F. Whidborne, M.A., F.G.S. The greater part of this paper was devoted to the description of a remarkably fine specimen of *Plesiosaurus* from the *Ammonites-obtusus* zone of the Lower Lias, Charmouth. For the species the name of *P. Conybeari* is proposed. *P. Conybeari* agrees closely with *P. Etheridgii* in the relative length of head and neck; but it has eight more cervical vertebræ than the last-mentioned species. In the number of the cervical vertebræ it agrees with *P. homalospondylus*, but has a much larger cervico-cephalic index.—On certain quartzite and sandstone fossiliferous pebbles in the drift in Warwickshire, and their probable identity with the true Lower Silurian pebbles, with similar fossils, in the Trias at Budleigh Salterton, Devonshire, by the Rev. P. B. Brodie, M.A., F.G.S.

Institution of Civil Engineers, May 10.—Mr. Brunlees, F.R.S.E., vice-president, in the chair.—The paper read was on torpedo boats and light yachts for high speed steam navigation, by Mr. John Isaac Thornycroft, M.Inst. C.E.

Meteorological Society, May 18.—Mr. G. J. Symons, F.R.S., president, in the chair.—D. W. Barker, B. Jumeaux, W. Oelrichs, H. Porter, W. Roper, and Rev. G. R. Wynne were elected Fellows of the Society.—The following papers were read:—Comparison of Robinson's and Osler's anemometers, with remarks on anemometry in general, by Richard H. Curtis. The author in this paper gives a very clear statement of the present state of anemometry, and points out the defects in Osler's and Robinson's anemometers, which are the chief forms of recording instruments used in this country.—Notes on waterspouts observed at Cannes in January or February, 1872, by the Hon. F. A. Rollo Russell, M.A.—On some Swedish meteorological observations in connection with the return of the seasons, by Alexander Beazeley, M.Inst.C.E.

PARIS

Academy of Sciences, May 16.—M. Wurtz in the chair.—The following papers were read:—Meridian observations of small planets at the observatories of Greenwich and Paris during the first quarter of 1881, communicated by M. Mouchez.—Nebulæ discovered and observed at the Observatory of Marseilles, by M. Stephan.—On the supposed presence of Proteaceæ of Australia in the flora of ancient Europe, by M. de Saporta. He gives reasons for doubting the supposed relation of the fossil plants, and the anomalous direct implantation in the heart of ancient Europe of a whole colony of plants that are now confined to a region in the southern hemisphere.—M. Berthelot presented the second edition of his "Traité élémentaire de Chimie organique" (which is about double the first).—Report on a memoir of M. Graeff relative to a series of experiments at the Furens reservoir on outflow of water.—On the transformation of morphine into codeine and homologous bases, by M. Gremaux. Codeine he regards as a methylic ether of morphine, considered as phenol. He transforms morphine into codeine by heating it with alcoholic potash or soda and iodide of methyl. Using ethylic iodide, a new base is got, homologous with codeine; indeed a series of these *codeines* (as the author calls them) may be had, as numerous as the series of ethers of an alcohol.—On the most ancient reptiles found in France, by M. Gaudry. He presented a fine specimen of remains of *Stereorachis dominans*. By their enlarged ribs, the arrangement of the thorax, the scales with spines, and especially the characters of the humerus, the Permian reptiles of France (like some fossils of South Africa, studied by Prof. Owen), somewhat lessen the wide interval between reptiles and monotrematous mammalia. They have traits of resemblance to reptiles of the Trias, and to those of the Permian in the United States, studied by Mr. Cope.—Observations on Swift's comet at Marseilles Observatory, by M. Borrelly.—On the separation of roots of numerical equations, by M. Laguerre.—On the principle of conservation of electricity, by M. Lippmann. The principle is expressed by a condition of integrability. In the memoir the author's method of analysis is applied to various phenomena—dilatation of glass of a Leyden jar during charge, electrisation by compression of hemihedral crystals, and pyroelectricity of crystals. The existence and law of certain new phenomena, not yet verified, are deduced.—On a mode of graphic representation of phenomena produced in dynamo-electric machines, by M. Deprez. A curve, called the characteristic of the machine, is got thus: Communication being first broken between the ring and the exciting electro-magnets, a known current, from an external source, is sent through the latter. The ring is then rotated with a given velocity; then the difference of potential between the two extremities of the (broken) induced circuit is measured. The auxiliary current is varied, and its intensities are taken as abscissæ; the ordinates are the differences of potential of the ends of the induced circuit.—On the theory of rotatory polarisation, by M. Mallard.—On the hydrates formed by chloride of calcium, by M. Lescœur. He recognises the existence of CaCl1HO, of CaCl2HO, of CaCl4HO (only under 129°), and of CaCl6HO (under 65°).—On the solubility of mercurous chloride in hydrochloric acid, by MM. Ruyssen and Varenne.—Peptones and alkaloids, by M. Tanret.—On the non-existence of *Microzyma cretæ*, by MM. Chamberland and Roux. The Meudon chalk behaves like chalk sterilised by heating, and contains nothing capable of producing microscopic organisms or any fermentation; thus M. Béchamp's observations (1866) are controverted.—On the crystallisation of alums, by M. Loir. The different faces of a crystal have not the same attractive power towards the solution of the substance employed to feed it.—Phyllotaxy, by M. Baron.—Studies on the coal formation of Commentry, by M. Fazol. He supposes that all the materials of this formation have been carried by water and deposited in a deep lake during a tranquil geological period.—On the milch sheep, by M. Tayon. There is an inverse correlation between the production of wool and that of milk. The presence of hairs directed upwards on the teats and neighbouring parts is noticed.—On the alterations of milk in sucking bottles, ascertained along with the presence of a cryptogamic vegetation in the caoutchouc tube fitted to the glass bottles, by M. Fauvel. Of thirty-one sucking-bottles examined in the crèches, twenty-eight contained this cryptogamic vegetation.

VIENNA

Imperial Academy of Sciences, May 19.—V. Burg in the chair.—Dr. W. Biedermann, contributions to general nerve and muscle physiology (part vii.): on the chemical changes during polar excitation by electric currents.—Dr. E. Weiss, supplement to his communication of May 12 on the Swift comet.—Prof. L. V.v. Zepharovich, crystallographical-optic examinations into the camphor-derivates.—Dr. Hans Molisch, on the deposits of carbonate of lime in the stems of dicotyledonous wood.—Dr. F. Lippich, contributions to the theory of the Polyhedra.—G. Czeteczka, researches into yeast.—T. Haubner, on the magnetic behaviour of iron powders of different density.

CONTENTS

THURSDAY, JUNE 2, 1881

ARCTIC ECHINODERMATA

A Memoir on the Echinodermata of the Arctic Sea to the West of Greenland. By P. Martin Duncan, F.R.S., and W. Percy Sladen. Pp. 82, Six Plates. (London: Van Voorst, 1881.)

MESSRS. DUNCAN AND SLADEN will receive the thanks of zoologists for the publication of this memoir, which will owe its importance as much to the care with which it has evidently been prepared, as to the interest of the group with which it deals, and the value that it has in being a monograph of a definite zoological region. The time would, indeed, seem to have come when no further question is possible as to the existence of a characteristic circumpolar fauna; nearly ten years ago Prof. Alex. Agassiz directed attention to the wide distribution of that common form, which has unfortunately so very long a name, the regular Echinid—*Strongylocentrotus drobachiensis,* in his "Revision of the Echini," and the researches of Mr. Seebohm have led him to a similar conclusion as to the circumpolar distribution of Birds. Further evidence is given by the present authors, who sum up the matter thus:—

"When these details are carefully considered, it becomes evident that each one of the great groups of Echinodermata tells the same story regarding distribution. The fauna, as a whole, is not an extension northwards of species from more temperate climates, but is essentially circumpolar."

Where the range is so wide considerable variations are to be expected in the characters of the species, and we feel inclined to attach as much importance to the accounts which the authors give of the variations they have been able to observe as to their technical zoological definitions of the species. We have been unable to find in the memoir any notice of the number of specimens which were accessible to the authors, but we believe it was quite large enough to have made the account of varietal forms a necessary part of a complete account.

It was, at any rate, large enough to make the number of new species very small; a new *Antedon,* a new member of Sars' interesting genus *Pedicellaster,* to which Mr. Sladen has given the appropriate name of *palæocrystallus,* make up, with an Ophiurid described by Prof. Duncan, the sum of our gains in that direction. So striking were the characters of this Ophiurid that it was found necessary to form a new genus for its reception; curiously enough Dr. Duncan proposed a generic term—*Luetkenia*—which has already been twice used in zoology; but this was a matter of slight importance, as Messrs. Koren and Daniellsen had the priority in recognising the generic distinction of the Ophiurid in question, and of giving it the name of *Ophiopleura.* This priority was however merely a matter of months, and not of years, as the English naturalist would lead us to imagine by leaving uncorrected in his proof the date of 1867 for 1877 (see *tom. cit* p. 54). Dr. Duncan holds to the view he expressed in 1878 that the form has affinities with *Amphiura* and *Ophioglypha;* what Prof. Lyman's views are it is impossible to state very definitely, but from the position which he has given to *Ophiopleura* in his lately-published "Preliminary List," it would seem that he attaches greater importance to its Ophioglyphan than to its Amphiuran characters.

We are not quite sure that we should agree with some of Mr. Sladen's views on nomenclature; but it is, we fear, too late now to raise a protest against the use of the term *Asteracanthion;* as the genus to which the name is applied has some eighty constituent species, it would be a matter of satisfaction if of the parties who use *Asteracanthion* or *Asterias* one or other would yield to the arguments adduced by the others. We must confess that for ourselves the latter term appears to have every advantage both of right and of convenience over the more unwieldy title of Müller and Troschel. It is much more satisfactory to direct attention to the way in which the authors have grappled with an old and unknown synonym in the case of *Astrophyton Agassizii.* In the year 1819 Dr. Leach applied the specific term *arcticus* to a specimen of "*Gorgonocephalus*" brought home by Sir John Ross; the definition of this species was too ambiguous and short for practical purposes, and it was not until Stimpson described the Invertebrates of the Grand Manan that the Arctic form got the name by which it has since been called by every naturalist who has had occasion to mention it; Lyman, Verrill, Ljungman, Lütken and Norman have all known it as *Astrophyton Agassizii.* The specimen which is supposed to be the type of Leach's description has no title attached to it, or any known history; in other words, it can never be *known* what Leach meant to describe, though it is easy enough to *guess;* under these circumstances (by detailing which the authors take, as we hope, the wind out of any mere bibliographer's sails), "we do not feel justified in restoring Dr. Leach's name."

This is an eminently satisfactory conclusion; ill-drawn definitions and unlabelled or wrongly-labelled specimens have had their day, and a good long day has it been, in hampering the progress of a growing science; if zoology is to advance with the other branches of biology, a purist sense of justice must not step in and lead us continually away from the real business of natural history to the dryasdust occupation of elaborating synonymical lists. A dictator-speaker and some rules of urgency might well be invented in the interests of zoological progress.

The six plates, on which twenty-six of the thirty species described in this volume are carefully figured, make a very useful addition to a work which the Government Grant Fund of the Royal Society were fully justified in subsidising.

F. J. B.

GREEK GEOMETRY

Greek Geometry, from Thales to Euclid. Part II. By G. J. Allman, LL.D. Extracted from "Hermathena," Vol. iv. No. vii. 49 pp. (Dublin: University Press, 1881.)

IN NATURE, vol. xviii. p 291, we briefly referred to the first part of Dr. Allman's work, which gave an account of the earlier geometers, more especially treating of the labours of Thales and Pythagoras. The opening years of the fifth century before the Christian era were very dark ones for Greece, but "then followed the glorious struggle. . . . A solid basis was thus laid for the development of

Greek commerce and for the interchange of Greek thought, and a brilliant period followed—one of the most memorable in the history of the world." Athens became the centre of all intellectual movement. To her—the Athens of Pericles—came Hippocrates of Chios, and "in this city geometry was first published."

Our author agrees with Hankel (against Proclus) as to the important influence of the Eleatics (Parmenides and Zeno), "not only on the development of geometry at that time (circ. 450 B.C.), but further on its subsequent progress in respect of *method*." Clairaut, in his "Elements of Geometry" (recently translated by Dr. Kaines, the original text is cited by Dr. Allman), notices this influence in the case of Euclid. The paradoxes of Zeno led to the banishment of the Infinite (which plays so important a part in the modern treatment), "the infinitely small as well as the infinitely great." What Hippocrates may very fairly be supposed to have done in relation to the squaring of the circle is, we think, well put. "Simplicius has preserved in his 'Comm. to Phys. Ausc.' of Aristotle a pretty full and partly literal extract from the 'History of Geometry' of Eudemus." It is to Bretschneider we owe a careful revision and emendation of this fragment, and our author has skilfully attempted to determine what is Simplicius and what is simply Eudemus in this account. It is curious that Bretschneider merely notices the "circumstantiality of the construction and the long-windedness and the over-elaboration of the proofs," and Hankel expresses surprise that this fragment, "150 years older than Euclid's Elements, already bears that character, typically fixed by the latter, which is so peculiar to the geometry of the Greeks." Had the present pamphlet been confined to the elucidation of this single matter it would have had a sufficient *raison d'être*.

The next geometer whose contributions to geometry are determined and discussed is Democritus, more usually regarded as a philosopher. At this stage, too, our author takes stock, and shows that the progress made in this (about) half-century interval since Pythagoras mainly "concerns the circle."

We note the connection of the name of Hippocrates with another of the famous problems of antiquity, viz. the duplication of the cube: he seems to have been the first to reduce this question to the finding of two mean proportionals between two given straight lines, the greater of which is double the less. Many interesting particulars are given in connection with this problem. The general problem is stated to have been first solved by "Archytas of Tarentum, then by his pupil Eudoxus of Cnidus, and thirdly by Menæchmus, a pupil of Eudoxus"; this last used "the conic sections which he had discovered." A third problem, the tri-section of an angle, also came to the front about this time. Dr. Allman fully discusses this also, and shows that it was one which was fairly within the reach of a Pythagorean. Montucla however attributes to Hippias of Elis, a contemporary of Socrates, the invention of the quadratrix (the quadratrix of Dinostratus), by means of which (in a quite different way from Sylvester's Fan) an angle can be not only trisected, but divided into any number of equal parts. Allman sides with Hankel and shows the improbability of Hippias being the inventor, but he is against him when he refers the method of exhaustions to Hippocrates of Chios. It will have been seen that the great geometer of this period is Hippocrates, "who seems to have attracted notice as well by the strangeness of his career as by his striking discovery of the quadrature of the lune." The unfavourable statements of Aristotle, Eudemus, Jamblichus, and Eutocius are examined, and part of the summing up is, "We may fairly assume that Hippocrates imperfectly understood some of the matter to which he had listened; and that, later, when he published what he had learned, he did not faithfully render what had been communicated to him."

An examination of this pamphlet still further shows that the writer, while carefully using the recent works of Bretschneider, Hankel, Cantor, and others, has himself gone over the original authorities and formed his own opinions upon the difficult questions that turn up. It is, in our opinion, a most valuable contribution to the subject, and we shall be glad when the piecemeal work in "Hermathena" is done, and the book appears, as we believe it is the writer's intention that it should appear, in proper book form as one work.

OUR BOOK SHELF

The Zoological Record for 1879. Being Volume Sixteen of the Record of Zoological Literature. Edited by Edward Caldwell Rye, F.Z.S. (London: John Van Voorst, for the Zoological Record Association, 1881.)

THE editor's promise to the members of the Zoological Record Association has been kept, and the *Record* for 1879 was published in the month of April in this year. We gladly note in addition his confident expectation that the *Record* for 1880 will be published ere the present year ends. This sixteenth volume contains nearly 700 pages of well-condensed records of the literature of zoology of 1879. The lion's share of the hard work has fallen to Mr. W. F. Kirby, who, with Mr. McLachlan, records the literature of the Insecta. The Rev. O. P. Cambridge gives the record of the Arachnida for 1878 and 1879. The Vermes and Echinoderms are done by Prof. Jeffrey Bell, and the Cœlenterata and Protozoa are elaborated by A. G. Bourne, S. J. Hickson, and Stuart Ridley. The works on the Mammals are recorded by W. A. Forbes; on the Birds by Howard Saunders; on the Reptiles and Fishes—alas! that we should have to write it—by the late gifted A. W. E. O'Shaughnessy. Prof. E. von Martens still records the literature of the Mollusca and Molluscoidea, the only recorder still remaining as such of that small group who came to the assistance of Dr. Günther in 1864. We miss from last year's list of recorders Dr. C. Lütken, who served during his seven years well and faithfully; in him knowledge and experience of the subject he worked at were combined with much tact. The British Association, the Royal Society, and the Zoological Society of London have, as is now usual, handsomely assisted in aid of the publication of this most useful volume.

The most useful index to new genera and sub-genera seems most carefully done. The list of new genera is for the year almost 1000; so that evidently the zoological kingdom is not as yet worked out.

Wiltshire Rainfall, 1880. (Marlborough: C. Perkins and Son, *Times* Office).

THE compilers of this carefully-printed and, for the class of publications, luxuriously got-up annual merit our hearty commendation for the general excellence of the work thus put before us. From its physical geography Wiltshire forms a well-marked rainfall region, it being a little to the north of the centre of the county that the two Avons and several tributaries of the Thames take their rise. From this plateau the country slopes northward to the

Upper Thames, eastwards along the Kennet, southwards to Salisbury, and westwards along the North Avon. The rainfall of this region is now observed at twenty-eight stations, and the daily amounts are printed *in extenso*, and the eye readily notes the maximum monthly fall at each station, these being printed in thicker type. On each monthly sheet the means of the previous ten years' observations are given for the ten stations at which observations have been made for the whole of that period. The mean annual rainfall of these stations for the past eleven years is 32·14 inches, the monthly maximum being 3·49 inches in October, and the minimum 1·82 inches in March. As contrasted with the more strictly central districts of England, the summer rainfall is relatively less, and the autumnal and winter rainfalls greater; and as contrasted with places more open to the Atlantic to west and south-westward, the rainfall is relatively greater in summer and less in winter. At seventeen stations observations have been made for at least six years, at which, if the averages be struck for the eleven years, differentiating where necessary, the largest mean rainfall is seen to be 40·32 inches at Corsham, near the summit of the long ridge separating the North Avon from its tributary Box Brook, and the smallest 29·76 inches at Pen Hill in the north on the high ground between the Thames and its tributary the Cole;—the former being one of the heights most open to winds from the Atlantic, and the latter one of the most sheltered heights from these winds. As regards annual amount and variation with season and configuration of surface, the rainfall of Wiltshire curiously resembles that of Deeside, Aberdeenshire. An excellent map showing the stations and their heights and the physical features of the county accompanies the Report.

Pheasants: their Natural History and Practical Management. By W. B. Tegetmeier, F.Z.S. Second Edition, greatly enlarged. (London: *The Field* Office, 1881.)

MR. TEGETMEIER is so well known as an authority upon pigeons and poultry of all kinds that everything which he writes on the subject of these birds is sure to be received with attention, and it is therefore scarcely a matter of surprise that a second edition of his well-known "Pheasant" book should have been called for. The work will be found invaluable to any one projecting the cultivation of pheasants either in the covert or in the aviary. After a brief review of the habits of pheasants in a wild state, the author gives ample information as to their management in preserves and in confinement, and also discusses the much-vexed question of the gapes and other diseases to which these birds are subject. The second portion of the work is devoted to the natural history of the common pheasant and its allies which are suitable for introduction into our woods, and also treats of the more gaudily-coloured Golden Pheasant, Monâl, and other species adapted for the aviary. Valuable experiences of the rearing of these birds and their habits in confinement are given by Mr. Tegetmeier, who seems to have spared no pains to make his book interesting and instructive. The illustrations have been executed on wood by the well-known artist Mr. T. W. Wood, who has evidently studied the birds in a state of nature; and although the plumage of the pheasant family does not lend itself readily to this style of illustration, the attitudes of most of the birds are happily rendered, while some of the figures representing the "showing off" of the male birds are excellently conceived.

Nach Ecuador. Reisebilder. Von Joseph Kolberg, S.J. Zweite vermehrte Auflage, mit einem Titelbild, 140 Holzschnitten und einer Karte von Ecuador. (Freiburg-im-Breisgau: 1881.)

THE Archbishop of Quito proceeded to Rome in 1869 to attend the meeting of the Vatican Council, and he bore with him a commission from Don Garcia Moreno, President of Ecuador, to obtain powers to establish a Polytechnic School and College for the Republic. As a result he sent to Quito, in 1870, two Germans and one Italian, members of the Society of Jesus, who should lay the foundation-stone of the establishment, and in 1871 Joseph Kolberg, the author of this quarto volume, followed. The murder of President Moreno in August, 1875, gave a death-blow to the new institution. During the five years of his sojourn in the country Kolberg had been in the habit of sending home notes of his various tours, sketches of the manners and customs of the people he met with, and this in a somewhat methodic manner, as might be expected from a professor of the higher mathematics. These notes and sketches were published from time to time in a publication called *Stimmen aus Maria-Loch*, and they embraced among others an account of the voyage out, of a visit to Chimborazo, and an account of the catastrophe of Havra (1868), and incidental to these latter chapters the author introduces a theory of volcanic eruptions which he evidently thinks the best fruit of his visit to Quito. All these varied sketches and others on the natural history and geography of the country were, at the "request of friends," re-published in one handsome illustrated quarto volume, which was indeed to have been dedicated to President Moreno, but is now dedicated to his memory. The first edition was edited by the author's friend, R. Cornelly, S.J., and was published in 1876. The present edition, which has been corrected and enlarged throughout, has been published under the author's own superintendence. Some of the wood engravings are new and interesting; others, such as those representing the flying-fish and the Coral Island, have well served their generation.

Second Report of the United States Entomological Commission for the Years 1878 *and* 1879. With Maps and Illustrations. 8vo, pp. 322, and Eight Appendices, pp. 74. (Washington: Government Printing Office, 1880.)

THIS Report of the three Commissioners (Prof. Riley, Dr. Packard, and Dr. Thomas) appointed to investigate the ravages of the "Rocky Mountain" and other locusts forms a handsome volume, got up in the exhaustive and elaborate manner so marked in all the U.S. Government publications. It is exceedingly difficult to give an adequate notice in a short space, on account of the varied nature of the subjects touched upon. Our readers will gather from this remark that "Economic Entomology" in the proper sense of the term by no means occupies the entire volume, nor is it entirely confined to the "Rocky Mountain" pest in particular. A large portion is occupied by an elaborate investigation of the habits of migratory locusts in all parts of the world, gathered from a host of publieations, some of them of ancient date. The connection of meteorological influences with the migrations and development of North American locusts is fully examined. Chapters IX. to XI. treat on the anatomy of the locust, and form valuable contributions to the anatomy of insects in general, such as one would scarcely expect to find in a report of this nature; of these Chapters IX. and XI. are by Dr. Packard, and treat of the air-sacs and brain respectively; X. is by Mr. Minot, on general histology: these are illustrated by excellent plates. The "economic" chapters are more especially by Messrs. Riley and Thomas, and go exhaustively into the question, more especially as to attacking the insect in its breeding-places, experience proving that war waged against the migratory swarms is comparatively useless; in connection with this, suggestions of a very broad nature are made. The Government is advised to encourage settlement of waste lands and the making of railroads conducing thereto, to induce broad schemes for irrigation, to guard the present timber, and encourage the planting of forests, to effect judicious burning in the breeding-grounds, covering about 400,000

square miles as now estimated, to institute efficient systems of observations and warnings, &c., &c. Prof. Riley treats on the natural enemies of the locust, and illustrates the chapter by a remarkably well-executed plate. The general conclusion arrived at is that the evil may be materially modified, although utter extermination is out of the question.

The lengthy appendices give replies to the official circular from those interested, in widely-separated districts, often showing great practical and frequently scientific knowledge, sometimes combined with the illogical conclusions at which agriculturists often jump. Then there is list of species of locusts, &c., collected in the Western States in 1877, with descriptions of new species, worked out by Mr. Scudder; the plate illustrating this is not so good as usual, and scarcely sufficient for scientific purposes. A general bibliography of locusts (chiefly compiled by Mr. B. Pickman Mann), from 1542 forwards, must have occasioned great labour, and is correspondingly valuable. Other appendices treat on the flight of locusts (translated from the Italian); on a journey to Utah and Idaho, by Dr. Packard; and a translation of Yersin's researches on the function of the nervous system in Articulates. The six large folded maps on thick paper seem to be admirably adapted to illustrate the points to which each is directed.

We do not think it is pretended that some of the most useful chapters from a scientific point of view have any special, or even indirect, bearing upon the subject of Economic Entomology. The investigation of a locust's brain, for instance, will hardly reveal the mental condition of the insect, and show us why it is prompted to migrate or be so maliciously inclined towards destroying the hopes of the agriculturist. We prefer to regard these portions of the report as an outcome of a liberal endowment of research, the application of which to the nominal subject for inquiry is not too rigidly enforced.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Laurentian Gneiss of Ireland

ALLOW me to state that since writing the notice which appears in NATURE (vol. xxiv. p. 81), I have found that white crystalline marble has been described by Sir R. I. Murchison and Prof. Geikie as occurring in the Laurentian gneiss of Loch Maree, in Scotland. I had overlooked this statement, and was under the impression that limestone was absent from the Scottish Laurentian area. Its occurrence in both countries constitutes an additional point of resemblance. EDWARD HULL

Geological Survey of Ireland, May 31

Resonance of the Mouth-Cavity

WILL you give a place in NATURE to the inclosed letter which has just reached me? The writer is an organist and teacher of music of great repute in the North of England. The experiments which he describes will, I believe, interest many of your readers as much as they do me.

Trinity College, Cambridge, May 20 SEDLEY TAYLOR

MY DEAR SIR,—Travelling the other day by express from Scarborough to London, I found myself unconsciously moving my lips as if whistling a tune, which however I was not actually doing. Without any other action than the simple movement of the lips, I very distinctly heard different sounds in my mouth. Persevering in the practice of this discovered power of producing sound, I soon accomplished a fairly satisfactory performance—audible only to myself—of "Home, sweet home."

As soon as the train came to a standstill I found myself powerless to repeat the performance, for there was then no response to the contortions which by this time had attracted the attention of my fellow-passengers, who doubtless thought that I was being conveyed to a lunatic asylum. On resuming our former speed the "power of sound" once more responded to my efforts.

At once I perceived that I had made a discovery of which I had never heard or read in any of the numerous works on acoustics that I have studied. I perceived that I could single out different sounds from the noise of the train by a simple alteration of the size of the resonance-cavity of my mouth.

On my return home it occurred to me that the force of vibration in the air from the note of a harmonium might be able to set up a sympathetic resonance of the mouth. To my delight I found that I was right.

It is known that if a tuning-fork of proper size be held to the open mouth the latter can be so shaped as to give a powerful resonance; but I believe it is not known that the mouth for *any* sound (above about middle C to f''' or g''') is able to give a very distinct resonance.

Further experiments showed me that not only can the primes of notes within this limit be heard, but that any of these sounds are very clearly heard when they are upper partials of low notes.

This can be tested at the harmonium. Holding down, say, G on the first line of the bass, the third and following partials up to about the fifteenth, can be most clearly heard. The same held good when I experimented with men's and boys' voices.

But the most striking results were obtained at the organ. Holding the low C of the 8 ft. trumpet, the partials from c' to $f'''\sharp$ were most bright and clear. Other stops, according to their several qualities, yielded corresponding results. I tried in vain however to obtain resonance for a differential and summational tone.

When the upper partials of a compound sound are generated in the mouth, the sounds are so near that with careful adjustment beats come out very clearly.

It is well however to observe that the force or weakness of any single sound thus obtained depends greatly upon the distance from the source. JOHN NAYLOR

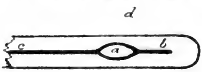
Scarborough, May 13

Suggestion Relating to the Kew Standard Thermometers

I SHOULD like to suggest, through your columns, two slight changes in the manufacture of the Kew standard thermometers, which I think will commend themselves to any observers who often have occasion to use these beautiful instruments.

1. The calibrating chamber at the top of the thermometer is now made as in the figure, where *c a b* is the capillary column which expands at *a* into the calibrating chamber. Instead of being rounded off at *d* the capillary column is continued a short distance to *b*. This causes serious inconvenience in the transportation of the instrument, or in its calibration, because a small particle of mercury readily detaches itself from that in the chamber *a*, and once in *b* with a cushion of air between it and the remainder of the column, nothing but heat will dislodge it.

It does not require very great skill on the part of the glass-blower to form the chamber *a* by means of the pressure of the



mercury itself against the walls of the capillary column. The glass-blower, as is perhaps well known, can soften the finished tube at *a*, and while the glass is in this condition the gentle application of the flame to the bulb will force the mercury into the part at *a*, and the careful application of both flames will then form a pear-shaped cavity of a form which will not retain a particle of mercury, and is exceedingly convenient in use.

2. It is often desirable to hang these thermometers in a comparator or other place, and it would facilitate this if a glass ring were attached to the upper end, as is the case with the ordinary chemical thermometers. It is to be observed that the plane of this ring should be parallel to the enamelling in the tube.

It is often convenient to know the kind of glass used in the

tube, and the date of filling. Something more exact than the commercial name of the glass would be needed in stating the former, but both of these particulars might with propriety be engraved on the tube. LEONARD WALDO

Yale College, New Haven, May 11

"How to Prevent Drowning"

ONE further hint may be added to those of Mr. MacCormac. It is as simple as practical, although it may not have been before recommended in print.

When a person is thrown into the water from an elevation the body sinks for a time, and may not rise quickly to the surface to permit fresh breath to be taken. In that case shut the lips firmly to prevent the escape of the breath, and swallow the breath. This is the art of the diver; it comes naturally to him when he seeks to prolong his stay under water; but it may not as readily occur to one unskilled in diving, whose only desire is to reach the surface. The act of gulping down the breath may be repeated three or four times, and thus protract the chances of escape.

Although every one may tread water, fresh or salt, Mr. Hill is undoubtedly right in saying that all cannot float upon fresh water without assistance from their hands or feet. Not one in ten can do so. When the swimmer shows his toes above the surface his hands are in constant action below, turning half-way round from the wrist and back again, to change the fulcrum.

WM. CHAPPELL

Stratford Lodge, Oatlands Park, Weybridge Station

THE bathing—I might almost say the drowning—season is now about to begin, and many lives will unhappily be lost. As the human frame, bulk for bulk, is lighter than water, all that is needful to save life is to permit the body to sink until it shall displace as much water as equals the body's weight. Then paddle gently, as the lower animals do, with hands and feet, the head being held erect, wherever it is desired to go. This direction being carried out is absolutely all that is needful under ordinary conditions to preserve life. These few directions ought to be stuck up in every bathing-place—every boating- and skating-place — in the three kingdoms. Children in every instance ought to be made to tread water from the earliest age, say in shallow slate baths with blood-warm water, or, when convenient and suitable, in some river, pond, or in the open sea. A leather belt with ring, and a stout rod with line and hook, are employed by Portuguese mothers to instruct their children. The mother, rod in hand, stands on the brink; the child learns in the water. In Paris swimming-schools the same procedure is resorted to. The business cannot be begun too soon. I saw mere infants sustaining themselves perfectly in the tepid waters of Africa. Treading water is far safer than swimming in a broken sea. Every adult, man or woman, who has not practised it should begin. Once the conviction instilled that the body is lighter than water, the risk of drowning is reduced to zero. The process involves no uncertainty, no delay. Very different from swimming, it can be acquired at once.

Belfast, May 25 HENRY MACCORMAC

Optical Phenomenon

MR. MURPHY's experience, described in NATURE, vol. xxiv. p. 80, is general enough. It was observed by Fechner in 1860, and is now commonly associated with his name, though Prof. Brücke of Vienna had also seen and explained the very same phenomenon some years before that. Nor was he the first, for, according to Aubert, there is a still earlier account due to Brewster in *Poggendorff's Annalen* for 1833.

Fechner's side-window experiment, as it is called, is best seen by employing a scrap of white paper on a black ground, or *vice versâ*, the eyes being accommodated for some other distance, so that double images of the paper are secured. Care must also be taken that the light from the window enters the nearer eye only through the sclerotic, so as to receive a reddish tinge. This diffused reddish light renders the eye after a short time comparatively insensitive to red, so that the light reflected from the white paper appears greenish, the black paper alone, from which no light is reflected, appearing of the reddish tinge. In contrast with this, in the other eye, which is sheltered by the nose from the window-light, the white light appears reddish, and the black greenish. Some little time is required for the illuminated eye to be exhausted for red before the contrast is very striking. Such is Brücke's explanation; but who will explain to us this "subjective phenomenon of contrast"? JAMES WARD

Trinity College, Cambridge, May 29

An Optical Illusion

IF your correspondent, Mr. William Wilson, will refer to vol. xxxiii. of the "International Scientific Series," page 86, he will find given by Prof. Le Conte a full description and explanation of the ocular illusion to which he refers in his letter (NATURE, vol. xxiv. p. 53). The explanation is identical with that given by yourself. SAMUEL DREW

Chapeltown, Sheffield, May 24

Occurrence of Neolithic Implements at Acton, W.

IT may interest your readers to know that I discovered, last week, on the surface of a field south of the Priory at Acton, an abundance of Neolithic implements, precisely similar as regards form, type, size, and material to those which occur so abundantly in the neighbourhood of Beer and Sidmouth, in Devonshire. They occur also on a large field on the hill at Acton, west of the Wilesden Railway, and are formed of grey or black chalk flints, which—or the implements—have been imported. On a field south of the Priory I found a flat, circular, grey, quartzite, beach pebble, derived possibly from the Bunter Conglomerate of South Devon, similar to those of the Dorsetshire and Devonshire coasts. Such pebbles are of frequent occurrence on the surface of the fields in the Neolithic districts of Beer and Sidmouth, and have been used as hammer stones and missiles. The association of this pebble with implements so like in every respect to those of South-East Devonshire is very remarkable.

The occurrence of palæolithic implements in the drift of Acton has been known for some years. They occur in remarkable abundance in the high level gravels of this locality as well as in the low level gravels of Hammersmith, and one cannot fail to find in newly-spread gravel examples of the minor implements, such as flakes, drills, &c., and occasionally larger implements. A series of the neolithic implements of Acton I purpose depositing in the Jermyn Street Museum. Their discovery at this locality confirms the conjecture I had formed that neolithic implements might occur in the Thames Valley, from having found implements of neolithic type in the drift, into which they may have got washed. SPENCER GEO. PERCEVAL

21, Notting Hill Square, W., May 20

Birds Singing during Thunder

A THUNDER-STORM of great severity passed over us, travelling round from west to south, between 4 and 8 o'clock p.m., May 28, and killing a man in the open air three miles from my residence. The thermometer stood about 70° all the while. During the storm, and even when the thunder-peals were loudest, the chaffinches kept singing, and the blackbirds' notes alternated with the thunder-claps. The rain was moderate, and as the air filled with insects and perfume, the swallows kept busily, skimming even while forked lightning was flashing. Horses in the fields however exhibited symptoms of terror. J. SHAW

Tynron, Dumfriesshire

Fire-Balls

I HAVE read with great interest Prof. Tait's lecture on Thunderstorms, and have had recalled to mind a singular fire-ball which I had the fortune to see some years ago during a thunderstorm in Portugal. I have a perfect recollection of the phenomenon without referring to my journal of that date.

I was standing in a window on the second floor of the Hôtel Braganza (in Lisbon), which stands close to and high above the Tagus, and had an unbroken view of the river. There occurred a flash followed by an instantaneous crash, but the tail of the flash, however, gave origin to two balls, which descended separately and not far apart, towards the river, and when quite close to, or in contact with the water, burst in rapid sequence, with explosions which might have been the crack of doom.

Sumatra, April HENRY O. FORBES

Sound-Producing Ants

IN NATURE, vol. xxii. p. 583, which has lately reached me, I read a letter from Mr. Peal on sound-producing ants, and I

cau corroborate his observations. It is nearly two years since I noted this fact in a species of *Polyrachis*, which makes its papery nests on the under side of bamboo leaves. The noise, resembling very heavily-falling rain, is caused by the insect striking the leaf by a series of spasmodic taps, both with its head and with the extremity of its abdomen, which it *inflexes* while so doing.

I came on a second large brown species in September last in Sumatra. The noise which, as in the case of the *Polyrachis*, resembled heavy rain, could be heard a long distance off. What struck me most about this species was the singular synchronism of the movements. These ants were spread over a space perhaps a couple of yards in diameter on the stem, leaves, and branches of a great tree which had fallen, and uot withiu sight of each other; yet the tapping was set up at the same moment, continued exactly the same space of time, and stopped at the same instant; after the lapse of a few seconds all recommenced at the same iustant. The interval was always of about the same duration, though I did not time it; each ant did not, however, beat synchronously with every other iu the congeries uearest to me; there were independent tappings, so that a sort of tune was played, each congeries dotting out its own music, yet the beginnings and endings of these musical parties were strictly synchronous. HENRY O. FORBES

Sumatra, April

The Pitt-Rivers Collection: Bell-Clappers—the Tooth-Ornament

IN the account lately given in your columns of the Pitt-Rivers Anthropological Collection I find it stated, in speaking of bells: "The clapper is a late addition to the bell which does not exist in Japan or China." When in West Java a year ago I saw iu the possession of a gentleman there a bronze (?) bell dug up on the site of one of the old Hindoo settlements, of which now only the graves remain. It had lost the clapper, but the hook, to which I have no doubt a clapper originally was attached, existed still. The form of the bell was much like those figured by Raffles in his "History of Java."

In speaking also of the development of ornamentation reference is made to the W pattern. In the Lampongs this is the most common and almost the only ornamentation. Its origin may possibly be as Mr. Low suggests; but farther up the country, where adornment is more frequent and varied, I find a very common pattern to be a circle ornamented all round the circumference with this toothed design, evidently, I thiuk, representing the sun, and it is uot improbable that on the circle being dropped where it could not well be introduced the "tooth-ornament" alone was retained.

Once travelling near Lake Doon, in the West of Scotland, I entered a lonely hut amid the mountains, where a woman was washing the floor—at least the stones set in it, for they were let into the mud at considerable distances apart. As she finished each stone she ornamented it with a piece of pipeclay with concentric circles, combining, where the stone was larger, two of these concentric ornaments into one by a stalk—as of a stem with two flowers on it . Did she still unwittingly retain the ornamentation of the European Bronze Period? HENRY O. FORBES

Sumatra, April

ON TOTAL SOLAR ECLIPSES OCCURRING BEFORE THE END OF THE PRESENT CENTURY

AT various times during the last six years we have given in our "Astronomical Column" particulars (including elements) of most of the total eclipses of the sun that will happen before the close of the nineteenth century. As the attention of many astronomers may soon be directed to arrangements for observing the eclipse on May 17th, 1882, we present here, in a collective form, the principal characteristics of such phenomena during the interval in question, which are likely to possess special interest under the circumstances. We shall refer to twelve eclipses, commencing with that of the ensuing year.

(1) 1882, MAY 17.—The most accessible positions on the central line will be in Upper Egypt and the extremity of the peninsula of Sinai about Sherm, but the duration of totality will be greater in the vicinity of Teheran. Where the central eclipse crosses the Nile, totality will commence at about 8h. 33m. a.m. local mean time, continuing 1m. 12s. According to General Stebnitzki's recent determination of the geographical position of the apparatus-room of the Indo-European Telegraph at Teheran, the central line will pass 8′ to the south of it, and here the duration of total eclipse will be 1m. 44s., which may be considered the longest available on this occasion: the sun's altitude will be 67°. The central eclipse passes off the Asiatic coast near Shanghai, running about 18′ north of that place; a direct calculation for Shanghai shows a partial eclipse only, greatest at 5h. 21m. p.m., magnitude 0·996, while at the neighbouring meteorological observatory of Zi-ka-wei, the eclipse is also partial, magnitude 0·994. On the central line in the longitude of Shanghai, the total eclipse continues only 35s. with the sun at an altitude of 18°.

(2) 1883, MAY 6.—In this case we have an eclipse where the totality will extend to nearly six minutes, but unfortunately this long duration falls upon the Pacific Ocean, and it does not appear that there is any land where it can be observed. By the Admiralty chart of the Marquesas, a duration of 2m. 53s. might be available on the Island Fetou-houhou, or Chanel Island, the sun at an altitude of 63°, and totality commencing about 0h. 42m. local mean time. At the head of Anna Maria Bay, Nouka-hiva, there is a partial eclipse only, magnitude 0·97. The central line lies wholly upon the Pacific: greatest duration of total phase 5m. 56s. in about 147° W. and 9° S.

(3) 1885, SEPTEMBER 8.—Observable in New Zealand soon after sunrise. In the longitude of Wellington the duration of totality will be 1m. 55s., with the sun at an altitude of 15°, at Wellington itself the duration will be hardly 40s.; the central line passes some forty-five miles to the north. The greatest eclipse falls in mid-Pacific in 58° S. latitude.

(4) 1886, AUGUST 29.—Totality will continue longer in this eclipse than in any other occurring within the interval which we are considering, but again it will happen that the greatest durations fall on the ocean, in this case upon the Atlantic. At the southern extremity of the Island of Grenada, or in 61° 35′ W. and 11° 59′ S. there will be a total eclipse with the sun at an altitude of nearly 20°, commencing at 7h. 10m. a.m. local mean time and continuing 3m. 15s. In 14° 13′ W. and 2° 58′ S. the sun will be upon the meridian at the middle of the eclipse, and totality will last for 6m. 27s. The central line meets the African coast in about 12° 14′ S. and here the duration of the total phase will be about 4m. 38s., with the sun at an altitude of 39°. [This eclipse is a recurrence of that of 1868, August 18, when the central line passed across Hindostan from near Kolapore to Masulipatam, where the duration of totality was 5m. 45s., but attained a maximum of 6m. 46s. on the west coast of the Gulf of Siam. At its next recurrence, 1904, September 9, the total phase continues 6m. 19s. but in mid-Pacific longitudes a little south of the equator. On September 21, 1922, though there is no land where the totality will be longest, a duration of about 3¼ minutes will be available on the east coast of Australia.]

(5) 1887, AUGUST 19.—It was long supposed that the central line in this eclipse would extend to England, but it appears to commence in 11° 39′ E. and 51° 38′ N. It will be most favourably observed in Asiatic Russia, but some fifty miles north of Moscow the total eclipse will continue 2m. 30s. with the sun at an altitude of 17°, and this is perhaps the most westerly station that observers should be induced to fix upon. In Moscow the duration would seem to be about one minute. At Berlin the sun will be totally eclipsed immediately after rising. On Lake Baikal

totality will continue about 3m. 38s., with the sun at an altitude of 50° and near the meridian.

(6) 1889, DECEMBER 22.—The greater duration of totality in this eclipse falls upon the Eastern Atlantic, but where the central line meets the African coast in Angola (about 10° 6′ S.) it continues 3m. 35s., with the sun at an altitude of 56°. At Bridgetown, Barbados, totality commences about 6h. 47m. A.M., with the sun at an elevation of 6°, and continues 1m. 48s.

(7) 1892, APRIL 26.—Almost entirely an ocean track on the South Pacific, commencing indeed in the Antarctic Ocean at a latitude of upwards of 75°: an impracticable eclipse, though the duration of totality attains a maximum of more than four minutes.

(8) 1893, APRIL 16.—Probably, all classes of observation considered, this will be the most favourable eclipse occurring before the end of the century. On the west coast of South America, rather less than a degree north of Coquimbo, where the sun will have attained an altitude of 24°, totality will continue nearly three minutes, commencing about 8h. 14m. a.m.: hence the central line traverses Brazil, passing off the South American continent near Ciara, and here the sun, at an altitude of 77°, and near the meridian, will be totally eclipsed 4m. 44s., or within a second or two of the longest interval possible on this occasion. Perhaps the central eclipse may pass about 10′ north of Ciara. After traversing the Atlantic it enters Africa close to Bathurst, at the mouth of the Gambia, where the total phase still continues about four minutes, thence through Central Africa to a point from 4° to 5° west of Khartoum, where it leaves the earth. From these circumstances an extended course of observations may be expected.

(9) 1894, SEPTEMBER 28.—On this occasion we have either a sea-track or a passage over inaccessible regions, except that the eclipse may ultimately be found to be total in the Seychelles; the tabular position of the moon, upon which our calculations referring to this phenomenon are founded, perhaps admitting of alteration to the amount required. The central line commences in the middle of Africa just north of the equator, leaving that continent near the Juba River, the mouth of which is almost upon the equator. In the longitude of Mahé in the Seychelles it appears to pass about 38′ to the south. The maximum duration of totality occurs in about 86° E. and 34° S., and is close upon two minutes. From this point the course of the central line is in the direction of Macquarie Island, near to which it passes off the earth, without, so far as a preliminary computation enables us to say, certainly encountering land after leaving the African continent.

(10) 1896, AUGUST 9.—Stations will doubtless be found for the observation of this eclipse, as although in the first half of its course, at least, the track lies at considerable northern latitudes, the season of the year is favourable. The central line enters Norway, near Tana in Finmark, and in 28° 46′ E. and 70° 31′ N. the duration of totality is 1m. 43s. with the sun at an altitude of 15°. After rising to a still higher latitude the central eclipse begins to descend, until we find it occurs with the sun on the meridian in about 112° 21′ E. and 65° 38′ N., and the latitude continues to diminish until the total phase leaves the earth. In 136° 21′ E. and 51° 5′ N., near the Amoor River totality continues 2m. 38s. with the sun at an altitude of 46°. The total eclipse may be observed also in the northern parts of Yesso, Japan, but does not afterwards meet land. [This will be a recurrence of the eclipse of 1806, June 16, observed by Bowditch in America, of that of 1842, July 8, well observed in the South of France and in Italy, and of the "Himalaya eclipse" of 1860, July 18, when a numerous party was conveyed to the south-west of Europe in H.M.S. *Himalaya*, there meeting with observers from all parts of the Continent, and unitedly putting upon record important details of the phenomena observed. Its last recurrence was on July 29, 1878, when so good an account of it was given in the United States by American and European astronomers.]

(11) 1898, JANUARY 22.—This eclipse may be well observed in Hindostan, where the central line enters the peninsula in about 73° 25′ E. and 16° 38′ N.; totality will commence at 0h. 45m., and continue about 2m. 6s. It commences in Senegambia, and leaves the earth in East Mongolia. Although many observations may probably be made in India, it will be seen that the duration of the total phase is comparatively short.

(12) 1900, MAY 28.—The central line entering upon the earth in the Pacific in 18° N. traverses the south-east portion of the United States, from Louisiana (not far from New Orleans) to Norfolk, on the Atlantic coast, and at the point where it leaves the American continent totality commences about 8h. 47m. a.m., and continues 1m. 40s. with the sun at an altitude of 47°. Crossing the Atlantic, upon which the greatest duration of totality falls, it enters Portugal near Ovan in about 40° 49′ N., and here the total phase continues 1m. 30s., with the sun at an elevation of 42°. The eclipse may be well observed in Portugal and Spain; at Alicante totality lasts 1m. 18s. This eclipse will be a recurrence of that of May, 1882, and the available durations of totality, it will be seen, are about the same on both occasions. In Hallaschka's *Elementa Eclipsium*, by an oversight, this eclipse is represented as broadly annular; the geocentric excess of the moon's semi-diameter over that of the sun will be, however, about 9″.

The following table exhibits the approximate positions of beginning and ending of total phase, and of the central eclipse at apparent noon, for the twelve eclipses included in the above remarks:—

Year.	Central Beginning.		Total at Apparent Noon.		Central Ending	
1882...	3·1 W.	10·7 N.	63·8 E.	38·8 N.	138·9 E.	25·5 N
1883...	155·9 E.	34·8 S.	147·2 W.	9·2 S.	86·9 W.	13·6 S.
1885...	156·9 E.	40·9 S.	138·7 W.	57·7 S.	75·6 W.	74·6 S.
1886...	79·6 W.	9·9 N.	14·2 W.	3·0 N.	47·3 E.	22·0 S.
1887...	11·7 E.	51·6 N.	102·3 E.	53·8 N.	173·8 E.	24·5 N.
1889...	78·9 W.	15·4 N.	6·5 W.	11·1 S.	60·9 E.	6·9 N.
1892...	144·1 W.	76·1 S.	138·7 W.	67·3 S.	81·7 W.	38·4 S.
1893...	95·7 W.	36·3 S.	36·6 W.	1·0 S.	28·6 E.	16·4 N.
1894...	26·9 E.	1·7 N.	86·3 E.	34·3 S.	163·0 E.	56·4 S.
1896...	1·0 W.	63·5 N.	112·4 E.	65·6 N.	179·1 W.	18·6 .N
1898...	10·0 E.	11·0 N.	68·8 E.	12·9 N.	119·3 E.	45·9 N.
1900...	116·6 W.	18·0 N.	44·8 W.	45·0 N.	31·8 E.	25·4 N.

A CHAPTER IN THE HISTORY OF THE CONIFERÆ[1]

THE CUPRESSINEÆ

THESE are classed as the first tribe of the Coniferæ in Hooker's "Genera Plantarum," wherein seven genera are recognised. The Cupressineæ are large trees or shrubs, very resinous, with small scale-like leaves. The cones are small and globular, and composed of six, eight, or rarely ten peltate and persistent scales, except in the juniper, in which they coalesce into a fleshy galbulus or berry. The seeds are small, compressed, frequently triangulated, and, except in *Juniperus* and the Biota section of *Thuja*, provided with small membranaceous wings at the angles. The order comprises many of the hardiest shrubs in existence.

Their origin can possibly be traced back to the Permian genus *Ulmannia*, and they seem to have become the preponderating tribe during the Jurassic and Wealden, to judge from the prevalence of wood known as Cupressinoxylon. The earlier forms, described as *Widdringtonites*, *Echinostrobus*, *Thuyites*, and *Thuyopsis*, though of great interest are still imperfectly known, even from the Cretaceous, but with the Tertiary period most of the

[1] Continued from vol. xxiii. p. 414.

existing genera appear, apparently as completely differentiated from each other as at the present day.

A few of the Cupressineæ, as cypress and some of the junipers, inhabit swamps or places liable to inundation, while other species of the same genera seek out the loftiest mountains and excel almost all other shrubs in hardiness, the juniper and cypress being found in Central Asia at altitudes respectively of 15,000 and 16,000 feet.

Many of them seem able to adapt themselves to a great range of climate. *Fitzroya*, a stately cedar 100 feet in height on the western slope of the Patagonian mountains, dwindles to a small bush a few inches high on the confines of perpetual snow, and the Chilian *Libocedrus*, 100 feet high on the Cordilleras, dwarfs to a small bush in Magellan. Of all the genera, however, *Juniperus* is the most hardy, extending itself as low scrub-bushes on most mountain chains to far beyond the limits of trees, and occupying to the south the barren rocks of Cape Horn (*J. uvifera*), and to the north penetrating Labrador, Newfoundland, Hudson's Bay, and Greenland (*J. Canadensis*).

Although of relatively less bulk than the Sequoiæ or the Pines, some species attain colossal dimensions, as the Oregon red cedar, *Thuja gigantea*. This tree, said by Gordon to be from 50 to 150 feet, and by Herschel 200 feet, high, seems actually to have reached an altitude of 325 feet, and a diameter of 22 feet, for a gigantic plank, exhibited by the State of Oregon at the Philadelphia Exhibition, was stated to have been cut at 118 feet from the ground from a trunk of these dimensions. *Libocedrus decurrens* exceeds 200 feet, and in the Himalayas the gloomy *Cupressus torrulosa* has been met with 150 feet in height and 16 feet in girth at five feet from the ground.

The woods of many of the species are valuable—those of *Frenela columnaris, Callitris quadrivalvis*, and some species of juniper being esteemed by cabinet-makers for furniture and veneering. The mottled butt wood of the "Thuja" of Pliny, and the "citrus" of Horace commanded fabulous prices during the Roman Empire. Cicero is said to have paid a million sesterces[1] (9000*l.*) for a table made from this wood, and of two tables belonging to King Juba, and sold by auction, one fetched 1,200,000 sesterces, although the largest recorded diameter is only about 4½ feet. The wood is still turned into tazza in Paris, and examples of it are preserved in the Kew Museum. Some of the most valuable gums, balsams, and resins, and amber are obtained from the tribe.

The first, and palæontologically most important, genus is CALLITRIS. This is subdivided into four sections, by many authors recognised as distinct genera—(1) *Pachylepis* or *Widdringtonia*; (2) *Tetraclinis* or *Callitris* proper; (3) *Hexaclinis* or *Frenela*; and (4) *Octoclinis*. The first section is doubtfully recorded as *Widdringtonites*, from the Lias of Switzerland and Würtemberg and from the Wealden and Cretaceous of North Germany, and Kome in Greenland. *Widdringtonia* is definitely found at Aix and other Eocene localities of France by Saporta, in the Miocene of Oeningen, at Bilin, and questionably so in the absence of fruits in the Greenland Eocene. It is now confined to South Africa and Madagascar.

The second section, *Callitris* proper, is distinguished by its cone formed of four truncated valves in pairs, and is represented at present by a single species confined to Northern Africa. Its fruits however are not only met with at Sheppey, but at Aix, St. Zacharie, and Armissan in France, and at Haring in the Tyrol.

Between this and the next section of *Callitris* should be placed, if cupressineous at all, the extinct genus (?) *Solenostrotus* of Endlicher, founded on Bowerbank's figures of fruits with five valves each.

The third section, *Frenela*, has a cone of six scales in opposite pairs, and is now entirely confined to Australia and New Caledonia, nearly two dozen species being more or less known. One of the most distinctly Cupressineous fruits yet met with fossil corresponds exactly with the Port Jackson *C. Endlicheri*, but has eight scales, and therefore falls into the *Octoclinis* section, also Australian, but now limited to a single species.

We have thus the most absolute proof that different sections of CALLITRIS flourished in these latitudes during the Eocene period, and therefore that the Palæarctic, Ethiopian, and Australian botanical regions overlapped and intermingled to some extent at that time. They do not seem, however, to have been present much to the north of our own latitude.

The genus ACTINOSTROBUS does not appear to have been found fossil except by Ettingshausen at Sagor (1859), and even these two specimens seem very indistinct and much smaller than either existing species, and are ignored by Schimper in his list of species. The existing Patagonian FITZROYA has no known fossil representative.

The fourth genus, LIBOCEDRUS, is distinguished by its flattened oblong cone of four to six leathery and very unequal scales, and by its thick scale-like and peculiar foliage. It occasionally forms very large trees, and in distributed over all but the Oriental and Ethiopias regions,[1] though the actual species have a limited range. The range of Libocedrus in the Tertiaries is singular. It appears from below the London clay at Bromley, then completely disappears in Europe until the Miocene, when it reappears at Bilin, Schossnitz, Radaboj, Armissan, Sinigaglia, from near Bonn, from Monod in Switzerland, and the amber-beds of Prussia. Another species, said to be allied to the Chilian tree, is found in the Eocene of Greenland. As most of the species of Libocedrus inhabit considerable altitudes, even reaching the snow level, and all of them are hardy in England, it is fair to infer that prior to the London clay the climate (and this is borne out by the rest of the flora) was much cooler than during subsequent Eocene times. That Libocedrus was really absent from temperate Europe during the latter part of the Eocene period is beyond all doubt, and that we actually experienced a change in climate such as had been inferred from the faunas of the Thanet sands and Woolwich and Reading beds is fully confirmed by the flora.

THUYA has small ovate or oblong cones of 6-10 valvate unequal scales, and foliage somewhat similar to Libocedrus, though less symmetric. There exist twelve to twenty-one species, divided into five sections, and some forming trees that are gigantic. The great majority are Japanese, but two species inhabit the Nearctic regions. The genus first appears in the Arctic Eocenes, descending into Europe during the Miocene, when it formed the vast amber-producing forests along the Baltic. The oldest beds to the south from which it is known, though it is rare there, are those of Armissan in France, and the late Miocenes of Marseilles and Tuscany. It is unknown from England, and the *Chamæcyparites* of the older Eocenes of Europe are now transferred to Sequoia.

The sixth genus, the stately CUPRESSUS, is only known fossil from two German Miocene localities. The existing species are mostly found in mountainous regions.

The seventh genus, JUNIPERUS, is very extensive, and is present in every geographical region except the Australian, being also one of the three British indigenous Gymnosperms. It has been recorded fossil from Aix, Häring, and the amber-beds of Prussia, but the smallness of the fragments and the absence of any traces of berries renders its occurrence, especially in the former localities, somewhat doubtful.

The range of the fossil CUPRESSINEÆ, as ascertained throughout the Tertiaries, is thus seen to be perfectly

[1] Hooker, "Tour in Marocco," p. 389.

[1] As restricted by Wallace.

natural; the hardy genera are never associated with the more tropical Eocene floras, and the sub-tropical genera did not range farther north than the present temperate latitudes, nor extend into the later Miocenes. Hardy species occupied these latitudes in the old temperate Eocene times, retreated as the temperature increased, and re-descended from the north as it again decreased, reaching finally as far south as North Italy. The habits and even the species of the genera have not materially altered since the Tertiaries commenced, and they appear to furnish comparatively safe data for physiological inquiries. The most remarkable fact taught by them, a fact beyond all question, is that types now distinctive of widely separated botanical regions actually lived side by side together in Western Europe in the Eocene age.

As the true nature of the various Eocene and Miocene floras becomes unfolded, thanks principally to the able work of Saporta, the fluctuations in temperature that Europe and America have experienced will be measurable and their ebb and flow calculable, with some approximation to certainty. The sensational extremes implied by the over-positive determination of fragments that no human being could determine with certainty, will then, it is to be hoped, be once and for ever discredited.

THE STORAGE OF ELECTRIC CURRENTS

PRACTICAL electricians seem to have made up their minds that a system for the distribution of electricity for the purposes of electric lighting or of driving electric motors will be incomplete unless it comprises a means of storage of the currents to provide against the risk of any temporary derangement or inconstancy in the generating apparatus. An accumulator of currents would in fact render the same service in an electrical system as do gasometers in systems for distributing gas, or the hydraulic accumulators in a system of hydraulic machinery.

At the present time much attention is directed to such *accumulators*, or, as they have been hitherto called, *secondary batteries*.

The principle of the secondary battery dates back to the very early days of Voltaic electricity, when in 1801, one year after Volta's "pile" had made its appearance, Gautherot, a French *savant*, observed that wires of platinum or of silver which had served as electrodes for the decomposition of water containing a little salt or sal ammoniac acquired the property of giving a brief current after being detached from the pile. This phenomenon, familiar to every electrician under the name of "polarisation of the electrodes," was observed again by Ritter of Jena, in 1803, with electrodes of gold wire; and the observation immediately led him to devise a battery from which these secondary currents could be readily obtained, and which constituted the first of all secondary batteries. He tried many different arrangements, using various metals—platinum, silver, iron, &c., but with lead he obtained no result. He attributed this secondary action to a soaking or accumulating of the two opposite kinds of electricity into the surfaces of the plates or into the intervening liquid. The true explanation was given by Volta and Marianini, and later by Becquerel, when they showed that the action arose from the deposits of oxygen and hydrogen, or of acid and of base upon the two electrodes, whose surfaces thus became chemically changed and capable of acting towards one another like the zinc and copper plates of an ordinary battery. Grove, in 1843, brought the matter to a decisive proof by constructing his curious gas battery, in which the positive and negative poles were both platinum plates, the one surrounded by oxygen gas, the other by hydrogen. Ritter's failure to obtain any effect from electrodes of lead arose from his employment of solutions of chlorides as the liquid, the chloride of lead which resulted on the passing of the current being a non-conductor, which at once stopped the current. M. Gaston Planté, who, in 1859, took up the study of the subject, found, after experimenting with many metals, that electrodes of *lead*, when immersed in dilute sulphuric acid, gave rise to very marked polarisation-effects; for after passing through lead electrodes the current from two Bunsen's cells, the

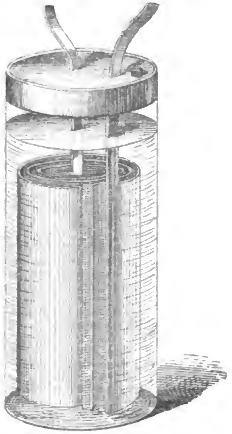


Fig 1.

This figure represents two sheets of *lead*, separated by two sheets of canvas rolled up together and placed in a glass jar containing water, two strips of sheet-lead protruding through the closed top of the jar.

secondary currents were extremely strong and of considerable duration. He therefore constructed large secondary batteries, using for this purpose two sheets of lead immersed in dilute acid. In order to reduce the internal resistance by bringing the opposed surfaces as nearly as possible together the two sheets were of large size and

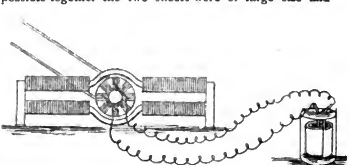


Fig 2

were rolled together in a spiral form, being kept from touching by the interposition of sheets of coarse canvas, or in later forms by means of bands of india-rubber. The general form of a single cell of Planté's secondary battery is shown in Fig. 1. Such cells weighed over twenty pounds, and when properly prepared had an electromotive

force 2½ times that of a Daniell's cell; their internal resistance was also very small, being from one-eighth to one-twentieth of an ohm. These cells improved with use; the liberated gases attacking the surface of the lead electrodes, so that they gradually became of a spongy texture, while the surface of the plate at which oxygen was liberated became covered with a film of brown peroxide of lead. When both electrodes were thus "formed" by charging the cell at intervals of a few days in opposite directions, the Planté cell became a veritable *accumulator*

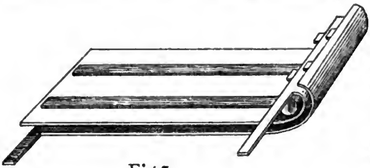


Fig 3

Represents two sheets of lead, separated by strips of thick felt, prepared for rolling together round a roller.

of electric currents, and was able to store up from a comparatively feeble source a supply which could yield vastly stronger effects for a short time. In fact the secondary battery became in Planté's hands a kind of Leyden-jar for storing currents of electricity; the essential point of difference between the two being that while the Leyden-jar accumulates a *charge*, and can be charged or discharged in an instant—or in other words possesses only an "instantaneous capacity"—the secondary battery accumulates currents which may flow into it for many hours, and which may take also a considerable time for their discharge, its "continuous capacity" being very great as compared with its instantaneous capacity. The currents stored up in the secondary battery are however not stored up as accumulations of electricity. They are stored up in the form of chemical work done in the cell, this chemical work being capable of being retransformed at will into the energy of electric currents. When the charging current from an independent battery or from a dynamo-electric machine (see Fig. 2) is passed through a Planté cell, the electrode by which the current enters becomes more highly peroxidised than before, while a corresponding amount of deoxidisation takes place at the electrode by which the current leaves the cell. When the cell thus charged is used as a battery it gives back a current which flows *out* from the electrode by which it formerly flowed *in*; passing through the cell from the deoxidised to the peroxidised electrode, until they are both reduced to a state of chemical similarity. If the cell is joined to the dynamo-machine which charged it, in order to drive it round as an electro-magnetic engine or motor, it will cause it to rotate in the *same* direction as that in which it was driven when used as a generator; the principle of reversibility applying both to the cell and to the machine.

Several forms of secondary battery adapted for storage of currents have been suggested in recent years. In Philadelphia Professors Houston and E. Thomson have tried a modification of the Daniell's cell, in which sulphate of zinc was electrolysed between electrodes of copper, the metallic zinc so deposited afterwards serving as the negative pole of the cell. Another suggestion, due to M. d'Arsonval, was to use an electrode of lead along with one of zinc, dipping into a solution of sulphate of zinc. The charging currents deposited metallic zinc upon the latter and liberated oxygen at the former, which, as in the Planté cell, became coated with spongy peroxide of lead. As this latter is not a very good conductor M. d'Arsonval further proposed to increase the effective surface by laying the sheet of lead horizontal and covering it with leaden shot, which should also become peroxidised.

The latest form of secondary battery is that of M. Camille Faure, described in NATURE, vol. xxiv. p. 68, of which there has been so much talk in the semi-scientific press, and which is now being made the central point of a great financial "operation" in Paris. There can be no doubt that this instrument, though the accounts of its performances have been grossly exaggerated, is an improvement upon that of Planté, of which it is a slight modification. The labour and difficulty of "forming" the Planté cell, that is to say of charging and re-charging it until a sufficient film of peroxide of lead should be produced, led M. Faure to try the effect of coating the lead plates at first with a film of *red lead* or minium, a lower oxide than the dark brown peroxide. The two sheets, after having been covered with minium, are rolled together precisely as in the Planté cells, as shown in Fig. 3, a sheet of felt being interposed to prevent internal contact. It was stated by M. Reynier that the capacity of such cells was forty times that of the Planté cell; but four times would have been nearer the mark if cells of equal size were compared. M. Faure's cells are made of large size and weigh 75 kilogrammes, or nearly 200 lbs. It is stated that one such cell would store a sufficient amount of current as to be able afterwards to yield in an hour an amount of work equal to one horse-power. Confirmatory observations are yet needed. Meantime let us just remind the enthusiast who brought over to England the "million foot-pounds" of energy stored up in a Faure cell, that he would have imported a dozen times as much stored energy if he had brought over instead a lump of coal of the same weight.

The uses for such secondary batteries may be of three kinds:—1. They may serve as portable supplies of electricity to be left and called for to recharge when exhausted. 2. They may serve to accumulate supplies of electricity from dynamo-electric machines, and store them until required for furnishing electric light or motive power on a small scale. 3. They may serve as equalisers of electric currents in a system in which the supply is liable to fluctuations. Suppose, for example, a dynamo-electric machine is employed to produce electric light. Any least thing which alters the speed of the machine, even for an instant, makes the light flicker and change in intensity; while the breakage of the engine-strap would at once cause total darkness. But if a secondary battery of suitable dimensions and power were inserted across the circuit between the dynamo-machine and the lamp, the inequalities of the current would be greatly modified. When the light was not in use the battery would store up the current. If the engine failed the battery would at once put forth its power. It is probably in this direction that the secondary battery will find no unimportant field of usefulness.

A SINGULAR CASE OF SHIPWRECK

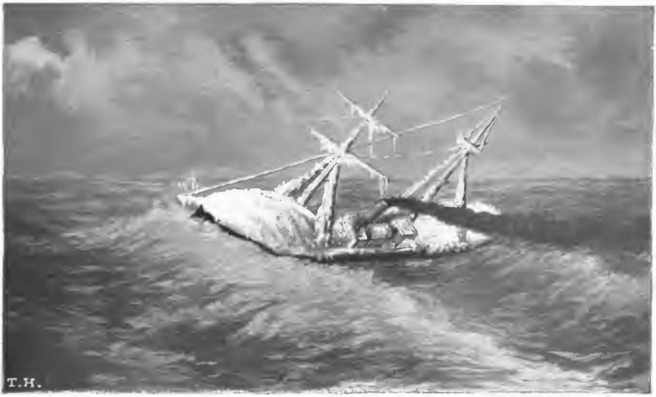
THE wreck of the Danish mail steamer *Phœnix*, which took place off the west coast of Iceland on January 29, was attended by rather unusual circumstances deserving of note. The vessel (about 450 tons burden) sailed with cargo and the mails from Copenhagen for Leith, the Faröe Islands, and Iceland, about the middle of January.

Nothing particular occurred until after leaving the Faröes, when she ran into a severe gale, which rapidly increased to a perfect hurricane, while at the same time the temperature fell to about 50° F. of frost (— 18° F.). Such cold is not extraordinary in these latitudes in winter, but fortunately it is seldom associated with very high winds. Under the circumstances in which the *Phœnix* was placed every sea that she shipped froze, and the deck soon became covered with a foot or two of solid ice.

As time passed on the continued action of the sea raised a perfect iceberg on the forward part of the vessel, while the showers of spray carried along by the steadily increasing gale covered the masts, yards, and rigging with an ever-thickening coating of ice. Two or three days passed without the least abatement of the storm, and then the half-smothered steamer went over on her beam ends. The crew succeeded in cutting away the masts, and she once more righted.

It however was clear, the gale showing no signs of breaking, that the relief was only temporary. The ice continued to form on the vessel, particularly about the forecastle, where, piled high above the bulwarks, and overhanging the sides, it threatened, by altering her trim, to raise the propeller out of the water.

Under these circumstances, on the morning of January 29, Capt. Kihl decided to run the steamer ashore while daylight lasted. At some distance from the land she struck on a sunken rock, and the crew, taking to the boats, only succeeded with the greatest difficulty in reaching the shore, saving nothing but their lives, the English mail, and a bundle of blankets which (when carried ashore) was found to be useless—frozen into a solid lump. Their situation in deep snow on the desolate coast of Iceland, about 100 miles to the north-west of Reykjavik, was very critical, and a party of two or three of the stronger sailors under the command of M. Jaspersin, the first officer, proceeded in search of assistance. It was not however until about three in the morning of the 30th, after suffering great hardships, that the sound of a pony kicking in a shed guided them to a house. The farmers immediately turned out, and eventually the scattered crew, twenty-two or twenty-three in number, were picked up, some of the clever little Icelandic dogs proving themselves most valuable auxiliaries in the search. Many of the men, however, were all but frozen to death in the snow. From this time all possible attention was given to them; but one, having both legs and arms frost-bitten, died; another afterwards had both legs amputated below the knees; and the steward lost three



fingers of each hand. Hardly one escaped more or less injury from the effects of the extreme cold to which they had been so long exposed. Capt. Kihl and the bulk of his crew soon after succeeded in getting to Reykjavik, and on April 13 they sailed in the sister steamer, the *Arcturus*, for Copenhagen. The officers and men of the wrecked vessel are of opinion that had Capt. Kihl not decided on the 29th to run the *Phœnix* ashore in daylight not a soul would have been saved, as the gale did not moderate for several days after; and the steamer, buried as it was under an enormous mass of ice, must have foundered in the night.

In Iceland this storm will be long remembered by the destruction it caused; and it is said that such a terrible winter has not been known for years. Sheep have perished in large numbers, and ponies have been killed to save hay. Then as the Icelanders depend on the outer world for flour, &c., the loss of the *Phœnix* with its cargo was a terrible misfortune. A letter from Reykjavik to a lady in Edinburgh, published on April 21 (and dated the 10th), states that the magazines were empty, and concludes thus: "It was very delightful to see the steamer *Arcturus* coming in the other day after having expected it so long. It brought both news and provisions, so that the poor people in the neighbourhood of Reykjavik can be helped for a while."

J. ALLEN ALLEN

NOTES

CAPT. FREDERICK JOHN OWEN EVANS, C.B., F.R.S., has been made a K.C.B. Among all the crowd of names gazetted for such honours on the Queen's birthday, not one had better deserved it by his services to his country, as well as to science, than the well-known Hydrographer to the Admiralty.

THE Visitation of the Royal Observatory takes place on Saturday. No doubt it will be numerously attended, as it is announced that Sir George Airy has resigned his post for the 1st of August

THIS is a week of *soirées*. The Royal Society *soirée* on Wednesday is followed by the Society of Arts *soirée* to-night, while the President of the Institution of Civil Engineers has issued invitations for another on Friday. These two last are held in the galleries of the South Kensington Museum.

MUCH interest has always been attached by anthropologists to the Stone Age of Egypt, on account of its bearing on the antiquity of man. Hitherto the finds of stone implements have been purely superficial, but in March last General Pitt-Rivers, President of the Anthropological Institute, discovered worked flints two to three metres deep in stratified gravel and mud near Thebes. The gravel had become so indurated in Egyptian times that they were able to cut square-topped tombs supported by square pillars in it, and these have remained in their original condition to this day. Some of the implements were chiselled out of the gravel in the sides of these tombs. General Pitt-Rivers will read a paper giving an account of this discovery at the Anthropological Institute on Tuesday next, the 7th inst.

MR. J. Y. BUCHANAN has, we learn, gone to Italy to see the ship which the Italian Government has fitted out for deep sounding in the Mediterranean.

THE "General Report of the Norwegian North-Atlantic Expedition" is being published in parts, each Memoir being distributed immediately on its leaving the press. The General Report will comprise the following Memoirs:—Capt. Wille, R.N. (Narrative of the Expedition—Description of the Apparatus, how constructed and used—Magnetical Observations); Prof. H. Mohn (Meteorology—Deep-sea Temperatures—Motion of the Sea—Astronomical, Geographical, Geological Observations); Mr. H. Tornoe (Amount of Air in Sea Water—Amount of Carbonic Acid in Sea Water—Amount of Salt in Sea Water); Mr. L. Schmelck (the Salts in Sea Water—Investigation of Bottom Samples); Prof. G. O. Sars (Crustacea, Pycnogonida, Tunicata, Bryozoa, Hydrozoa, Spongozoa, Rhizopoda, Protophyta); Drs. Danielssen and Koren (Holothurida, Echinida, Asterida, Crinoida, Gephyrea, Anthozoa); Mr. H. Friele (Mollusca, Brachiopoda); Dr. G. A. Hansen (Annulata); Mr. R. Collett (Fishes). The publication of the Report, for which a grant of money has been obtained from the Norwegian Storthing, is conducted in conformity with the directions of His Norwegian Majesty's Government. Of the memoirs we have received that on "Fishes," by Mr. R. Collett, and "Chemistry," by Mr. H. Tornoe.

WE greatly regret to learn that Dr. James Croll, F.R.S., has been compelled, in consequence of ill-health, to retire from his position on the Geological Survey. The same reason will account for his not replying to certain correspondence and criticisms which would otherwise have claimed his attention.

DR. AUGUST WILHELM EICHLER, Director of the Royal Botanic Garden and Museum, Berlin, and Professor of Systematic Botany at the University, has recently been elected a Foreign Member of the Linnean Society in the room of the late G. P. Schimper of Strassburg. Prof. Eichler is well known among botanists for his memoir, "Loranthaceæ et Balanophoræ," in Martius's "Flora Brasiliensis"; also as author of "Entwicklungeschichte des Blattes," and "Blüthendiagramme," &c.

WE regret to have to record the sudden death early in March of Mr. John Sanderson, one of the oldest colonists of Natal. From his arrival in 1850 he resided in Durban, where he conducted for many years one of the most influential newspapers. He was otherwise much occupied with public business, and was for some time a member of the Legislative Council. To botanists in Europe he was well known as an ardent explorer of the South African flora and active correspondent, and his name is commemorated by the beautiful genus *Sandersonia*.

A MONUMENT is to be erected to the memory of the late Dr. Broca, the founder of the Paris Anthropological Society; not less than 14,000 francs have already been collected. We are desired to state that the list will soon be closed, and that all subscriptions are to be sent as early as possible to M. Leguay, treasurer of the fund, at the Paris Anthropological Society.

THE Dorpat University offers two prizes of 750 and 500 roubles respectively for the two best models of a monument in memory of Karl Ernst von Baer. Sculptors are informed that the models will be received by the University until September 15 next, and that the carriage to and from Dorpat will be defrayed by the University. Professors Dr. Grosse (Dresden) and Bohnstedt (Gotha) are members of the committee of decision.

A MONUMENT of the celebrated naturalist, Freiherr von Siebold, was unveiled in the park of the Vienna Horticultural Society on April 22 last. The monument is four metres high, and is in the form of an obelisk with a granite pedestal. The upper part is formed by a very ancient memorial stone ornamented with floral designs, which was originally sent to the Vienna Exhibition by the Japanese Government, and was afterwards destined for this monument. Below this stone is a slab of marble bearing an excellent bas-relief of Siebold, the work of Schwanthaler. The whole monument is surrounded by living fir-trees, which were obtained from the Rax Alpe.

THE death is announced of Dr. Ludwig Rabenhorst of Meissen (Saxony). He was a well-known botanist and editor of the *Hedwigia*. Among his numerous publications we may point out "Die Süsswasser Diatomaceen" (Leipzig, E. Kummer) as an indispensable companion to all students of microscopical plants.

THE *conversazione* of the Society of Arts takes place to-night at South Kensington, and that of the Institution of Civil Engineers to-morrow night at the same place.

THE death is announced of Herr Andreas Schmid of Eichstätt (Bavaria), the editor of the *Bienenzeitung*, and author of numerous treatises and pamphlets on bee-culture.

THE Whit-Monday excursion of the Geologists' Association is to be to the Isle of Wight, and will last three days.

DR. SCHLIEMANN has recently been nominated "honorary citizen of Berlin," and has also been presented with the large gold medal "for Arts and Sciences" by the Grand Duke of Mecklenburg-Schwerin.

IN a brickfield near Lützen (Saxony) some 200 sepulchral urns, skulls, and bones have been found. Dr. Virchow has examined one of the skulls, and declares it to be of a very peculiar type, somewhat resembling the well known Neander Valley skull, but yet differing from it sufficiently to form a special type of its own. The whole discovery at Lützen, combining cremation and ordinary burial, is at present unique. No ornaments of any kind were discovered.

THE Jablonowski Society at Leipzig offers the following prizes in its scientific section for 1881:—700 marks (35*l.*) for an investigation of the motion of Encke's comet, at least for the period since 1848, taking into account all disturbing influences; for 1882: the same amount for a compilation of our present knowledge of the corrosion-figures of crystals, with an account of original experiments on this subject and a deduction of general maxims regarding the cohesion and structure of crystals as well as their molecular conditions; for 1883: the same amount for a determination of the photo-electrical tensions produced in artificial and suitably-coloured crystals by the action of light and their relations to the thermo-electrical effects produced by changes of temperature.

THE works will begin immediately for the construction of the International Exhibition Electrical Railway, as we are glad to state that the request of Messrs. Siemens has been granted by the Municipal Council of Paris.

OUR Paris Correspondent was present last Friday during an experiment made by M. Trouvé on a small boat between Port Royal and Pont des Arts. The boat, measuring 5m. 50 by 1·30, and carrying three persons, obtained a mean velocity of 1m. 30 per second, with a magneto-electric motor weighing 2 kilogr., and two series of six Wollaston elements weighing 12 kilogr. each. The trial lasted an hour and a half, and was interrupted by darkness. These experiments will be repeated shortly on the Bois de Boulogne lakes. The motor, which was constructed to give 8 kilogrammetres per second, did a duty which a single rower would have been unable to perform. The electro-magnetic motor was placed on the rudder, and the motion communicated to a small screw placed in the lower part by a chain. This system is not calculated to utilise the whole extent of the motive power generated by the elements, but it dispenses entirely with any alteration to the boat. This last circumstance is considered as decidedly important in popularising the system amongst yachtsmen. There is not the slightest vibration or noise of any description felt on board.

THE lectures which were delivered in connection with the Glasgow Naval and Marine Engineering Exhibition are about to be published.

MR. W. R. BROWNE has issued a carefully revised edition of the Library Catalogue of the Institute of Mechanical Engineers, combining both a list of authors and of subjects. It also contains a Subject-Index of papers in the *Proceedings*, 1847-80.

MR. C. W. HARDING, of King's Lynn, received prizes for two papers at the Recent Norwich Fisheries Exhibition:—1. "Essay on the Artificial Propagation of Anadromous Fish other than the Salmon, and the Re-stocking of the Tidal Waters of our Large Rivers Artificially with Smelts," &c.; 2. "On the Utilisation of Localities in Norfolk and Suffolk suitable for the Cultivation of Mussels and other Shell Fish."

ON Monday night shocks of earthquake were felt at the Observatory on Mount Vesuvius and at the villages at the foot of the mountain, especially at Torre del Greco. At nine o'clock on Tuesday morning the seismographic activity was decreasing.

THE Swiss Federal Commission for Meteorology has been definitively constituted for three years as follows:—M. Schenk, president; Professors R. Wolf (Zürich), Hagenbach (Basel), Plantamour (Geneva), Ch. Dufour (Morges), Forster (Berne), and Weber (Zürich), and M. Coaz Inspector of Forests at Berne. The Commission met at Berne on May 23, and discussed the institution of a Central Board at Zürich. Want of means has prevented it from opening the important meteorological station on the summit of Mount Sentis.

M. DE LESSEPS has been urging upon the Khedive of Egypt the re-establishment of the Cairo Observatory, originally founded by Mehemet Ali; its fine instruments have been long ago dispersed among various establishments. The atmosphere of Egypt is peculiarly favourable for observatory work, and we hope the Khedive will take M. de Lesseps' advice.

A MARINE Exhibition will be held at Hamburg in September next, of which nautical and astronomical instruments will form the principal part.

A CORRESPONDENT (A. H. McC.) of the New York *Weekly Evening Post*, writes on the question of the Sound of the Aurora:—In your edition of Saturday I noticed an abstract from "Record of a Girlhood," in regard to hearing the aurora borealis, and therefore beg to give you my experience on the subject. In the winter of 1846 I crossed the Atlantic from Newfoundland to Greenock in the brig *Amanda*. We had strong southerly winds the whole passage, without seeing the sun until after making land; three days previously a strong southerly gale carried away our only top-sail, leaving us without sufficient after sail, and consequently we were driven far to the northward. The day before we made land the wind suddenly changed to the north-west, and as night approached the sky became clear. At about 9 o'clock p.m. the captain called all passengers on deck, and a more magnificent spectacle was never contemplated—the whole heaven was a blaze of white light, the aurora darted and rushed from every point and reflected each colour of the rainbow. While it lasted we could distinctly hear the sound, as if the folds of heavy silk were shaken, sometimes sharp and quick, and then receding until the sound was lost, according to the intensity of the flash. During most of the time a book could easily be read on deck. The phenomenon lasted about four hours, during which time we all remained on deck. Next morning we made land, which proved to be Barra Head, Southern Hebrides, and were able to lay our course.

ON May 2 the German Fisheries Society held its annual meeting at Berlin, his Imperial Highness the Crown Prince being present. Herr Friedel, the director of the Märkische Museum, delivered a festive address on "Pre-scientific Fishing." Afterwards the president of the Society, Herr von Behr-Schmoldow, read the report for the year, which was highly favourable. Some 6,000,000 ova have been "sown out" in German waters. Excellent results were obtained with 300,000 American salmon ova and a similar number of shad ova. The intercourse with the fisheries societies in Austria, France, and the United States was very successful and remunerative.

AT the annual general meeting of the Sanitary Institute of Great Britain on May 25, Dr. Richardson, F.R.S., who for four years past has acted as Chairman of Council, and who now goes out of office by rotation, after expressing his thanks to his colleagues for all the courtesies they had shown him, said he could not, he thought, conclude his work as chairman more usefully than by giving a short account of some recently conducted researches which he had made as to the periods of incubation of the infectious diseases which the sanitarian has to combat. He proceeded to indicate that there are twenty-six well-known diseases of this kind, and they each have their special periods of incubation, which, though open to exceptions, are fairly regular. The period of incubation was that period which intervened between the acceptation of the poison that caused the disease and the first manifestation of effect. Diseases might thus be grouped according to their stages of incubation into five classes—*Shortest, Short, Medium, Long, Longest*. The shortest period was one to four days: under this head came plague, cholera, malignant pustule, and dissection poison. The second period was from two to six days, and under this head came scarlet fever, diphtheria, croup, erysipelas, hooping-cough, influenza, glanders, and pyæmia. The medium period was from four to eight days, and in it are included cow-pox and relapsing fever. The long period had ten to fifteen days, and included in it measles, mumps, typhus, and typhoid. The longest period, forty days, included syphilis, and might include hydrophobia. Dr. Richardson concluded his address by showing the important practical sanitary lessons that were connected with a correct knowledge of these periods of incubation.

THE Technological, Industrial, and Sanitary Museum of New South Wales is, we learn, intended to occupy a similar position and fulfil the same purpose in that colony which the South Kensington Museum, the Bethnal Green Museum, the Museum of Practical Geology, the Patent Office Museum, and the Parkes Museum of Hygiene do in London. To this end, it is intended

to collect together typical collections of all materials of economic value belonging to the animal, vegetable, and mineral kingdoms, from the raw material through the various stages of manufacture, to the final product or finished article ready for use. It is intended that the following shall be more or less completely represented:—Animal products; vegetable products; waste products; foods; economic entomology; economic geological specimens; educational apparatus and appliances; sanitary and hygienic appliances and systems; mining, engineering, and machinery; agricultural tools, appliances, and machinery; also soils, manures, &c.; models, drawings, and descriptions of patents; ethnological specimens; examples of historical furniture and of artistic workmanship in iron and other metals; photographs, electrotype, plaster, and other reproductions of examples of art workmanship where originals are not to be obtained. Exhibition catalogues, trade journals, price lists, and descriptions of new processes or industries. The acting secretary is Mr. Charles R. Buckland.

We have received the abstract of *Transactions* of the Anthropological Society of Washington for the first two years of its existence, ending January, 1881. It contains brief notices of the meetings of the Society, the papers read, as might be expected, relating mostly to American Indians. A paper by Col. Mallory, "On the Comparative Mythology of the Two Indias (Asia and America)" shows that many resemblances exist between them, arising solely, however, from the efforts of two quite distinct primitive peoples to interpret the same natural phenomena. Prof. Gill deals with the Zoological Relations of Man. The principal papers, however, are the annual addresses of the president, Mr. J. W. Powell, on the Evolution of Language, as Exhibited in the Specialisation of the Grammatic Processes, the Differentiation of the Parts of Speech, and the Integration of the Sentence, from a Study of Indian Languages; and on Limitations to the Use of Anthropological Data. Mr. Powell has also a long paper on "Wyandot Government—a Short Study of Tribal Society."

We have received a long letter from Mr. Gerald Massey on our review of his "Book of the Beginnings," which we regret we are unable to print. We should state, however, that the word *bottle*, p. 49, col. 1, line 17 from bottom, was a misprint for *beetle*; and the following extract from Mr. Massey's work (vol. i. p. 145) will show the sense in which he quoted Prof. Max Müller:—"If the first man were called in Sanskrit Adima, and in Hebrew Adam, and if the two were really the same word, then Hebrew and Sanskrit could not be members of two different families of speech, or we should be driven to admit that Adam was borrowed by the Jews from the Hindus, for it is in Sanskrit only that Adima means the first, whereas in Hebrew it has no such meaning."—(Quoted from Max Müller's "Science of Religion," p. 302).

The additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius* ♀) from South Africa, presented by Mrs. Findlay; a Common Marmoset (*Hapale jacchus*), a Black-eared Marmoset (*Hapale penicillata*) from South-East Brazil, presented by Mr. C. Stewart; a Two-spotted Paradoxure (*Nandinia binotata* ♀) from West Africa, presented by Mr. W. H. Hart; a Bennett's Gazelle (*Gazella bennetti*) from Afghanistan, presented by Brigadier-General Tanner; a Globose Curassow (*Crax globicera*) from Central America, presented by Mr. Allan Lambert; a Slow-worm (*Anguis fragilis*), British, presented by Mr. G. Mengee; three Peacock Pheasants (*Polyplectron chinquis* ♂ ♂ ♀) from British Burmah, six Nuthatches (*Sitta cæsia*), British, deposited; two Ring-necked Pheasants (*Phasianus torquatus* ♂ ♀) from China, two Swinhoe's Pheasants (*Euplocamus swinhoii* ♂ ♀) from Formosa, four Chilian Pintails (*Dafila spinicauda*) from Antarctic America, an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, a Dominican Gull (*Larus dominicanus*) from the Falkland Islands, a White-marked Duck (*Anas specularis*) from the Straits of Magellan, an African Wild Ass (*Equus tæniopus* ♂) from Abyssinia, purchased; a Cashmere Shawl Goat (*Capra hircus*), a Mouflon (*Ovis musimon* ♀), born in the Gardens; three Variegated Sheldrakes (*Tadorna variegata*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN

A New Variable Star.—Prof. Julius Schmidt publishes an account of his observation of a star of from 8m. to 9m. in Canis Minor on April 1, which had not been remarked as late as March 28, and of which not a trace was visible on the following night. On November 25, 1879, Mr. Baxendell discovered a star in Canis Minor of about 8·8m., and of a decided orange colour, where the *Durchmusterung* showed no star, which gradually diminished, until on January 11, 1880, it was estimated 9·6. Prof. Schmidt gives the position of this star from observations at Athens and at Dunecht, with the places of two of Bessel's stars as follows for 1880·0:—

		R.A. h. m. s.		Decl. ° ′ ″		
W.B., 7h. 1014	...	7 34 18·04	...	+ 8 40 20·9	...	(*b*)
Baxendell's star	...	7 34 49·98	...	+ 8 39 30·1	...	(*x*)
W.B., 7h. 1029	...	7 34 54·17	...	+ 8 42 29·4	...	(*a*)

Examining this neighbourhood with a 5½-feet refractor on the evening of April 1, he remarked a star (*c*) of between the eighth and ninth magnitude, and by "eine Ordinaten-construction" from a diagram then made, he judged its position for 1880·0 to be in R.A. 7h. 34m. 56·4s., Decl. + 8° 41′·4. If these places are reduced to the epoch of the *Durchmusterung* (1855·0) they will stand as follows:—

		m.		R.A. h. m. s.		Decl. ° ′	
b	...	9	...	7 32 56·6	...	+8 43·7	
x	...	9	...	7 33 28·5	...	+8 42·8	
a	...	8·3	...	7 33 32·8	...	+8 45·8	
c	...	9	...	7 33 35·0	...	+8 44·4	without a greater error than 0′·7.

On April 2, at 8h., with the same refractor, not the slightest trace of *c* could be discovered, and on taking the precaution to examine the stars within a space of two minutes (time) preceding and following the place by the previous night's observation, no similar object was found: a star 10m., wanting in Argelander, showed no motion in two hours. On April 3 and 4 Prof. Schmidt was similarly unsuccessful. Baxendell's star had been compared with Bessel's two stars with the Cometen-Sucher on eighty-six evenings between 1879, December 6, and 1881, March 28 (an instance, by the way, of the scrutinising system of observation which the indefatigable astronomer of Athens is accustomed to apply in his variable-star work): on no occasion was any object noted in the place of the star *c*.

Prof. Schmidt has remarks upon this observation to the following effect: if the object had been a distant planet it would have been found on April 2, 3, and 4 close to its place on April 1. If it had belonged to the group of minor planets it would have been readily identified on one of the evenings the vicinity was examined, by means of the *Durchmusterung*, without knowledge as to the direction of motion. If it were a variable star there was a variation of light from 8·5m. to absolute invisibility in twenty-four hours, which has never been remarked in any other variable; and lastly, if it were a *Nova*, of the class to which the well-known stars of 1848, 1860, 1866, and 1876 have been assigned, its illumination is limited to the interval between the evenings of March 28 and April 1, and therefore could have extended only to four days.

Referring the places of the objects observed by Baxendell and Schmidt to the star of 8·3m., No. 1029 of Weisse's Bessel, we find—

	Angle of position. °		Distance. ″
For Baxendell's star	207·8	... ...	135
For Schmidt's star	153·2	... ...	73

Fellöcker's careful work for the Berlin Chart, Hour VII., does not render any assistance in this case. One of our many amateurs might advantageously take up the systematic and frequent obser-

vation of the neighbours of W.B. 7h. 1029. It may be ultimately found that Schmidt's star affords an extreme illustration of the laws affecting variables of the class of U Geminorum.

Can Mr. Baxendell put anything upon record as to dates when he has examined the vicinity, which may bear upon the length of invisibility of Schmidt's star?

THE COMETS OF 1810 AND 1863 (V).—At the suggestion of Prof. Bruhns new elements of the Comet of 1810 have been investigated by Herr Thraen from the ten observations made at Marseilles by Pons, who discovered the comet on August 22. We had previously orbits by Bessel and Triesnecker. The observations are unfortunately affected with considerable errors, but the following appears to be the best system of elements obtainable from them:—

Perihelion passage, 1810, October 6·23793, M.T. at Greenwich.

Longitude of perihelion	63° 46′ 43″	M. Eq.
,, Ascending node ...	308 50 31	1810·0
Inclination	62 55 39	
Log. perihelion distance	9·986603	

Motion—direct.

We subjoin Prof. Weiss's parabolic elements of the Comet 1863 (v), discovered by Respighi at Bologna on December 28; notwithstanding their striking resemblance to those of the comet of 1810, Prof. Weiss was inclined to consider the comets different: he tried an ellipse with period of fifty-three years, but the comparison of the observations in January, 1864, was not so satisfactory as with the parabola. Michez, by direct calculation upon a month's observations, obtained a period of 109 years.

Perihelion passage, 1863, Dec. 27·76369, M.T. at Greenwich.

Longitude of perihelion	60° 24′ 28″	M. Eq.
,, Ascending node ...	304 43 26	1864·0
Inclination	64 28 46	
Log. perihelion distance	9·887344	

Motion—direct.

There is perhaps room for a further and more minute discussion of the observations of 1863-64, though the result may be adverse to the idea at one time entertained that the comets are identical. There was a near approach to the Earth at the end of January, 1864 (0·18), and the perturbations from this cause will require to be taken into account in a more refined determination of the orbit. The comet was observed at Kremsmünster till February 14; Prof. Julius Schmidt sought for it ineffectually at Athens on March 4.

Probably we have, in the case of these comets, an illustration that mere similarity of orbits, even though it may be pretty close, is not to be regarded as proof of identity.

CHEMICAL NOTES

HERR DRECHSEL claims (*Journal Pract. Chem.*) to have converted ammonium carbonate into urea by the continued action of a galvanic current, the direction of which is rapidly reversed by a self-acting commutator.

A MODIFICATION in the process for manufacturing iodine from seaweed is described by MM. Pellieux and Allary (*Bull. Soc. Chim.*), whereby, it is said, nine times more iodine is obtained than by the older process. The unburnt seaweeds are allowed to ferment in large heaps; the liquor which drains off is concentrated and dialysed in Dubrunfaut's apparatus before evaporation. The plants are burnt without further drying.

SEVERAL of the tinned preparations of the St. Louis Canning Company have been examined by Mr. Wigner (*Analyst*). The dietetic value of the corned beef is about twice that of boneless fresh beef; the cooked ox-tongues contain less salt and more nutritive matter than ordinary dried tongues. *Succotash*, an American preparation of haricot beans, Lima beans, and maize, cooked in the tin, with addition of a little fat, is recommended as a good specimen of boiled vegetable food.

THE number 197·2 is generally accepted as the atomic weight of platinum; Herr Seubert has very recently shown, in the *Berichte* of the German Chemical Society, that this number is very probably too high. As the mean of thirty-nine closely-agreeing results, Herr Seubert obtains the number 194·46. If this number is accepted the atomic weight of platinum is less than that of gold.

HERR TH. WILM has studied (*Berliner Berichte*) the action of finely-divided palladium, platinum, and rhodium, when heated in a stream of coal-gas. He finds that these metals decompose the gas, that when palladium is used a deposit of carbon takes place at some distance from the heated metals; that with platinum the carbon is deposited on the metal, but that on heating in a stream of air the carbon is burnt completely away, and the metal remains in its original form; with rhodium, however, the carbon appears to form a compound, the volume of which is considerably greater than that of the metal itself; this compound is decomposed only with difficulty, leaving metallic rhodium in a much more bulky form than that which it possessed before the experiment.

MICHAELIS AND SCHULTE describe, in *Berliner Berichte*, a new derivative of benzene, analogous with azobenzene, but containing arsenic in place of nitrogen. *Arsenobenzene*, $C_6H_5—As=As—C_6H_5$, produced by the action of reducing agents on C_6H_5AsO, forms slightly yellow coloured needles which are insoluble in alcohol and ether, and crystallise well from hot xylene. The new compound, unlike azobenzene, readily takes up two atoms of chlorine, forming—

$$C_6H_5—\underset{\overset{|}{Cl}}{As}—\underset{\overset{|}{Cl}}{As}—C_6H_5.$$

DR. A. R. LEEDS describes, in the same journal, the action of nascent oxygen, ozone, and hydrogen peroxide on benzene. Nascent oxygen was produced by the action of moist phosphorus on air in presence of benzene; if the action proceeds in sunlight a large quantity of phenol is produced, together with oxalic acid; but if in diffused daylight no phenol is formed. Phenol was also produced by the direct action of hydrogen peroxide on benzene, but when ozone acted on the same hydrocarbon, carbon dioxide, acetic, formic, and oxalic acids were produced, but no phenol was formed.

IN a preliminary note in the *Berliner Berichte*, Herr Donath states that the ordinary method of volumetric determination of oxidisable substances by titration with potassium permanganate in presence of acid may, in many cases, be advantageously replaced by an inverse process in which a neutral solution of the substance to be oxidised is added to a strongly alkaline solution of permanganate, whereby oxidation occurs with precipitation of manganese dioxide:—thus with manganese salts and with chromic salts the following actions occur:—

$$(1)\ 3MnO + Mn_2O_7 = 5MnO_2.$$
$$(2)\ Cr_2O_3 + Mn_2O_7 = 2CrO_3 + 2MnO_2.$$

A METHOD for determining molybdenum in molybdates is described by Danesi (in *Atti della Accademia*), based upon the fact that hydriodic acid is decomposed by molybdenum trioxide with separation of iodine. A weighed quantity of the molybdate under analysis is mixed with hydrochloric acid and a solution of potassium iodide; the amount of iodine liberated in the reaction is determined by titration with sodium thiosulphate solution. The results are very accurate.

IN the same journal a method for determining nitrites and nitrates in the same solution is described by Piccini: the method is based on the decomposition of ferrous chloride by nitrous and nitric acids with evolution of nitric oxide, and on the greater readiness with which this decomposition is accomplished by nitrous than by nitric acid.

HERR M. GRÖGER has prepared (*Wien. Akad. Ber.*) several sulphochromites, *i.e.* salts of a chromous acid in which oxygen is replaced by sulphur; the following salts are mentioned among others:—

$Na_2Cr_2S_4$	$CdCr_2S_4$
$Ag_2Cr_2S_4$	$SnCr_2S_4$.

THERE are about 300 known optically active carbon compounds. The specific rotatory power of but seven of these has been accurately determined for the pure substance, and for solutions of the substance in various solvents in all possible degrees of dilution; these seven are tartaric acid and ethyl tartrate, cane-sugar, dextrose, terpene from turpentine oil, nicotine, and camphor. The nature and quantity of the solvent employed exerts a marked influence on the rotatory power of the active compound; as a contribution to this subject the measurements made by Herr A. Becker (Deut. Chem. Ges. *Berichte*) of the rotatory power of asparagine and aspartic acid deserve mention. An aqueous solution of either compound exhibits left-handed rotatory powers;

addition of hydrochloric or sulphuric acid diminishes the lævo-rotatory action, and eventually converts it into a marked dextro-rotatory one; addition of acetic acid has a similar, but much less marked effect; with asparagine a point is reached at which the solution, in acetic acid, is optically inactive.

ATTENTION was recently drawn in these "Notes" to an attempt made by Th. Thomsen to show that the numbers expressing the specific rotatory powers of various carbon compounds might be expressed as whole multiples of certain fundamental constants; each of these constants was supposed to be characteristic of a group of allied compounds. Thomsen's methods of calculation have been severely criticised by Landolt (in the *Berichte*), who has shown that from the limited accurate data at our disposal such an attempt as that of Thomsen can only be regarded as a play on numbers, and is devoid of all scientific value.

EXPERIMENTS are described in the May number of the *Journal* of the Chemical Society by Jones and Taylor, which appear to leave little doubt that these chemists have succeeded in preparing a gaseous hydride of boron, and that the probable formula of this compound is BH_3. The new compound—the existence of which establishes another point of analogy between boron and the nitrogen elements—is prepared by decomposing magnesium boride by a dilute acid; the gas has only been obtained largely mixed with hydrogen; it burns with a green flame, and is decomposed by heat with deposition of boron.

IN the course of a paper on the appearance of nitrous acid during the evaporation of water (Chem. Soc. *Journal*), by Warrington, experiments on the detection of this acid, by the use of hydrochloric acid and naphthylamine hydrochloride, are described, which show that one part of nitrous acid is easily detected in 10,000,000 parts of water, and that as small a quantity as one part in 1,000,000,000 can be detected.

IN the *Zeitschrift fur anal. Chem.* Herr Seelheim describes experiments on the percolation of waters through soils, from which he draws the following general conclusions:—Only that stratum of any soil which is composed of the smallest particles need be considered in determining the permeability by water of the soil of a district. The composition of a soil must be ascertained, otherwise experiments on a large scale furnish no measure of the permeability of that soil. The thickest stratum of sand allows the passage of many hundred times more water than a layer of clay only one centimetre thick. The permeability of dykes may be regulated by inserting layers of clay between layers of sand.

SMALL quantities of carbon monoxide may be detected, *e.g.* in the air of rooms, by drawing the suspected gas over powdered glass moistened with diluted blood, shaking the blood with a drop of ammonium sulphide and examining by the spectroscope. Strips of paper soaked in a solution of 0·2 gram. palladium chloride in 100 c.c. water serve to detect carbon monoxide: the dried slips are suspended by platinum wires in a large flask with a very little water, and the flask is corked; with five parts of carbon monoxide in the atmosphere of the flask, a black shining deposit of metallic palladium appears on the paper in a few minutes; with one part, in two to four hours; and with 0·5 part, in from twelve to twenty-four hours.

A USEFUL historical account of the investigations made on the subject of dephosphorising pig-iron, appears in *Dingler's polytechnisches Journal*, and in abstract in the May number of the *Journal* of the Chemical Society.

THE rate of chemical reactions having been lately the subject of several investigations and discussions, M. Kayander publishes in the Russian *Journal* of the Chemical and Physical Society (vol. xiii. fascicule 4), the results of his last measurements. Without seeking to establish theories as to a connection between chemical affinity and the rate of reactions, M. Kayander simply tries to make measurements in a branch of chemistry insufficiently worked until now. To simplify the results he has employed a solid body and a liquid one, and has measured the rate of dissolution of magnesium in various acids; the magnesium was taken in the shape of small plates, having a surface of about 2000 square millimetres; the acids, in solutions of 0·01 of the atomic weight (in grammes) in a litre of water. Experiments as to the influence of various degrees of concentration will be published in a second paper. As to the influence of time, he arrives at the conclusion that the reaction begins at the very moment of the immersion of the magnesium in the acid; acids when mixed produce the same action as if taken separately. As to temperature, its influence is precisely that which it exercises on the diminution of the internal friction of the particles of the liquid against one another, and does not seem to influence the chemical properties of the reacting bodies; the figures M. Kayander has arrived at from a long series of measurements establish that the speed of the reaction is inversely proportional to the internal friction of the medium. The researches will be continued.

PHYSICAL NOTES

ON heating a plate of boracite lately Herr Klein (*Gött. Soc. of Sci.*) was surprised to observe a complete change of the optical image. The boundary lines of the optical fields prove variable with temperature, and often wholly disappear, perhaps reappearing in quite different places. Herr Klein concludes from these and previous observations that boracite does not owe its origin to a twin-like formation of parts of lower symmetry, but is regular, and produces simple individuals; and the optical properties, apparently in sharp contradiction to this, are really due to tensions produced in growth. These divide the crystal into parts of different tension, of which the sometimes stronger suppress the weaker, for certain temperatures and positions of the crystal. (Similar properties in crystals of analcime have been described by Herr Ben Saude to the Göttingen Society.)

AN ingenious, somewhat complex, apparatus, named an *automatic methanometer*, or automatic analyser of fire damp, has been recently brought before the Geneva Physical Society by Prof. Monnier (*Arch. des Sci.*, April 15). The fire-damp, in presence of air in excess, is decomposed in a glass vessel by a platinum wire rendered incandescent, and the condensation produced acts directly on a mercury manometer, having platinum wires inserted in its tube. The air of the mine is automatically forced by bellows, every hour and half hour, into the burner. The receiving apparatus stands in the central office. The system includes several electro-magnets, two batteries, pendulums with escapement, an alarm-bell, &c.

THE influence of pressure on the electric conductivity of metal wires has been studied anew by M. Chwolson (Imp. Acad. of St. Petersburg *Bull.*, March); Wartmann's previous experiments, in which wires were compressed between steel plates with caoutchouc lining, having failed to show whether pressure changes the *specific* resistance. M. Chwolson used a piezometer, giving pressures up to 60 atmospheres, the wire being wound round a glass tube, then passed through it, and the tube inserted in another, which was connected with the piezometer. (The two wire ends were brought out through binding screws.) Among other results, at 3·8 C. the copper wire showed a relative diminution of resistance of about 0·0000013 by one atmosphere of pressure; a hard brass wire about 0·0000011; and a lead wire (at 7° C.) about 0·000011, or ten times more than the brass. Pressing at 17° C. the calorific action preponderates over the direct action of pressure for copper and brass, while the reverse occurs with lead. Moreover, the author proves, in the case of the brass wire, that the pressure causes change of the specific resistance besides change of the resistance through change of the length and thickness. Every relative change of volume involves a relative change of the specific resistance about 3·6 times as great.

M. MASCART showed recently how the phenomenon of Talbot's fringes could be applied to measuring the refractive indices of gases and the difference between the refractive index of a solid and that of a liquid. M. Hurion has further thus measured the difference of the refractive indices of liquids, and in the *Journal de Physique* for April he shows how the refractive index of a liquid may with those fringes be directly determined. The two interferent rays are rendered vertical, so as each to traverse one of the halves of a partitioned rectangular vessel with glass bottom. The liquid being first at the same level in both divisions, its level in one is gradually lowered by a special contrivance, and this has the effect of displacing the fringes in the field of the telescope. Let e be the variation of level, f the number of fringes that have passed a point in the field corresponding to light of wave-length λ, then $e(m-1) = f\lambda$. The letter m represents the refractive index of the liquid for light of wave-length λ. (For further details we refer to M. Hurion's note.)

Prof. Trowbridge lately observed that a steel bar magnetised to saturation at 20° C. and subjected to a temperature of -60° C. lost 66 per cent. of its magnetism (a much greater percentage than that formerly observed by Wiedemann).

Prof. Robinson of Ohio concludes from experiments (*Jour. Frankl. Inst.*, March) that vibrations in extended media from a remote single centre of disturbance, can only be longitudinal, even in light; that vibrations will be to some extent transversal when due to two or more centres of disturbance not in the same line; and that undulations, to be in a condition called polarised, must consist of vibrations which are transversal, and that no necessity exists for assuming vibrations transversal in front of a polariser. These views are not only contrary to the accepted wave-theory of light, and to the conclusions derived from Maxwell's electromagnetic theory, but appear to be directly negatived by the experiments of Stokes and Figeau.

In his third paper on electrical shadows (*Gott. Soc. Nachr.*, February 5) Herr Holtz studies, *inter alia*, the differences in form of the light surface and shadows from the two electricities; the effects of using differently-conducting surfaces under the silk, and of using convex and concave spherical or cylindrical surfaces; the double shadows from two surfaces used as electrodes; the use of a silk screen between pointed electrodes, &c.

A new seismometer is described by Dr. G. Wagener of Kioto, Japan, for which he claims several advantages over the ordinary heavy-pendulum seismometers. It consists of a strong rigid frame in the form of a short quadrangular pyramid, from which is suspended an iron ball weighing about fifty pounds by means of a bundle of untwisted silk fibres three feet long. Below this ball is an indicating pendulum consisting of a hollow sphere pivoted near its centre of suspension upon a small polished ball, also rigidly fixed to the frame, and carrying beneath it a light arm, whereby its motions are multiplied twenty-four times. A small sphere fixed to the bottom of the iron ball plays into a cavity in the summit of the indicating pendulum. The latter has, by reason of its construction, a *very* short period of oscillation as compared with that of the iron ball. Hence when an earthquake occurs the inertia of the heavy ball will keep it for a considerable time in its position, while the pointer of the indicating pendulum moves toward the region whence the disturbance came, and can return almost instantly if the horizontal displacement be succeeded by a displacement in the opposite direction. That the movement of the pendulum may be registered accurately in point of time, a small silk thread attached to the bottom of the indicating pendulum passes through a small eye-hole in a porcelain plate immediately beneath, and thence passes round a light indicating wheel which is also in connection with a lever which at the slightest movement drops, and stops a clock. A kindred apparatus is employed to register the direction of the shock, eight threads from the indicating pendulum of a similar instrument being wound round eight indicating wheels for the eight chief points of the compass. For these instruments it is claimed that there is less error from oscillations than in the usual instruments, the inertia of the indicating pendulum checking the tendency of the weight to swing. A further registering apparatus, consisting of a chronograph drum actuated by a clock which is started by the first shock, is also described. It does not appear that the registering apparatus of Dr. Wagener is in any way an improvement upon the electrical apparatus hitherto employed. Lastly, Dr. Wagener describes an instrument for measuring any possible vertical displacements, a heavy body of considerable inertia being counterpoised while immersed in a tub of water, its movements being magnified by a lever and registered by a thread-wheel arrangement.

M. Gaiffe gives us reason to suppose that part of the disturbance in telephone lines, usually set down to "induction," is due to the conducting-wires being of a magnetisable metal iron, which, when moved in the magnetic field of the earth, experience induction-currents. M. Gaiffe introduced into a telephone-circuit two pieces of steel wire, one magnetised, the other not. On hitting them so as to make them vibrate sharply, sounds were produced in the telephone more strongly by the magnetised strip. The remedy is obviously to employ conducting-wires of some non-magnetic substance, such as copper or manganese bronze.

According to P. Tacchini, there are in the sun two regions of spots and faculæ at about equal distance (20° and 22°) from the equator, and about the same longitude, which showed continual activity last year. The fact he considers important for theories as to the sun's physical constitution.

The last number of the *Journal* of the Chemical and Physical Society of St. Petersburg (vol. xiii., fascicule 4) contains two letters by Prof. A. M. Boutleroff on ice under critical pressure. The former of them was written on February 13, when Prof. Boutleroff had not yet received the number of Nature which contains a detailed description of Prof. Carnelley's experiments; the second on March 17. The experiments which he has made, and which he describes in this second letter, were made, Prof Boutleroff says, on the same conditions as those of Prof. Carnelley, but the temperature of ice remained at -6°; a rise of temperature was observed only when a part of the bulb of the thermometer was free from ice, but even in this case it was very slow. "The refrigerating influence of ice was quite manifest, but it was not at any time possible to discover anything showing a rise of temperature." Prof. Boutleroff supposes that Prof. Carnelley has raised the temperature of his thermometer without raising that of ice. "It is true," he says, "that the calorimetric experiment of Prof. Carnelley seems to speak in favour of a heating of the ice itself; but can we not suppose that a kind of covering of hot vapour which was around the ice, remaining on its surface, was transported into the water of the calorimeter, and there neutralised the refrigerating power of the ice?" Prof. Boutleroff proposes also, for the same experiments, to make use of a cryophorus which might be easily appropriated for that use, and which he describes in that case as a cryoscope. When repeating Prof. Carnelley's experiments with a cryoscope, Prof. Boutleroff happened to raise the temperature of the thermometer to +40°, whilst the bulb of the thermometer was *nearly* completely covered with ice; but he failed to raise the temperature when he covered the surface of the bulb with a small sheet of platina. He concludes that the bulb of the thermometer in those cases, when it shows a temperature above zero, enters into contact with ice only at some few spots; and the rise of the thermometer might be explained, not only by the thermal translucency of the ice, but also by the circumstance that the vapour disengaged by the melting ice is heated, and reaches the bulb of the thermometer by the small channels formed in the ice; he therefore concludes that Prof. Carnelley's condition as to the ice being in a special state not quite reliable.

GEOGRAPHICAL NOTES

Dr. Wild, president of the International Polar Commission, has issued a circular stating that six countries have already intimated their intention to co-operate in carrying out the scheme of simultaneous meteorological, magnetical, and other physical observations in the Arctic regions. These countries, with the proposed stations, are Denmark at Upernivik, Norway in Finmark, Austria-Hungary in Jan Mayen, or perhaps East Greenland, Russia in Novaya Zemlya and at the mouth of the Lena, Sweden in Spitzbergen, and the United States at Point Barrow and in Lady Franklin Bay. Should other countries send in their adhesion to the scheme, this disposition of the stations may be somewhat modified. We are glad to see there is a probability that Germany may establish a station in the Island of South Georgia, and France a station at Cape Horn. An interesting feature in the scheme is that two of the eight proposed Arctic stations are to be equipped at the expense of private individuals, viz., the station in Jan Mayen or in East Greenland at the expense of Count H. von Wilczek of Vienna, and the station in Spitzbergen, as our readers are already aware, by M. L. O. Smith of Stockholm. Is it possible that no public-spirited Englishman will be found to provide the means for England co-operating in this truly international scheme of physical observations in the Polar regions, which play such an important *rôle* in the meteorology of the globe?

As much has been heard of late respecting the magnificent harbour which the French are likely to make of Lake Bizerta in Tunis, it may be well to correct a misapprehension which has long existed as to its depth. Even in the most recent gazetteers this is said to be fifty fathoms, whereas in a brief but excellent paper which he sent to the Geographical Society many years ago, Admiral Spratt, speaking from his own soundings on the spot, explicitly states that the greatest depth of water in the lake is *eight* fathoms, with an average of from five to six fathoms. This would no doubt be sufficient for all practical purposes, but at the

same time it is vastly different from the great depth given to the lake by old travellers, whose mistake has been perpetuated.

IN his anniversary address to the Geographical Society last week Lord Aberdare paid a just tribute to the services rendered to geography in the region west of Lake Nyassa by Mr. F. C. Selous, who has hitherto been best known as a mighty hunter of large game. This gentleman, we learn, in 1878 penetrated for 150 miles the unknown country north of the Zambesi, in the direction of Lake Bangweolo. He has since crossed in various directions the Matabele country south of the Zambesi, discovering two new rivers and defining the courses of others which had previously been laid down from vague information. His notes on the River Chobe have already been published by the Geographical Society. We understand that the fine trophies of the chase which Mr. Selous brought back from South Central Africa have been placed in the hands of Messrs. Rowland Ward and Co. for preparation.

FROM the report of the progress of the Ordnance Survey which has just been issued, accompanied by useful diagrams, we learn that it is expected that the whole survey will be completed by 1890, as the staff is to be augmented in consequence of increased funds being placed at the disposal of the Director-General.

THE programme of the first German "Geographentag" at Berlin, on June 7 and 8, contains the following addresses:—Prof. Zoppritz (Königsberg), on the condition of the earth's interior; Prof. Neumayer (Hamburg), on the importance of magnetic researches from a geographical point of view; Prof. Rein (Marburg), on the Bermuda Islands and their coral reefs; Prof. Bastian (Berlin), on the problems of ethnology; Prof. Kirchhoff (Halle), on the methods of teaching geography in schools. Professors Wagner, Meitzen, and Ascherson will speak on similar subjects.

DR. CREVAUX has completed his third South American journey. He descended the Guayabero River (a tributary of the Orinocco) on rafts, and made an exact survey of this river. The survey comprises 1275 miles, of which 375 are a complete desert. By the assistance of natives Dr. Crevaux and his companions reached Ciudad-Bolivar, whence they embarked for Trinidad on board a steamer. Shortly before the end of the journey one of the travellers, a sailor of the name of Burban, was killed by a sting-ray (*Trygon pastinaca*). Later on Dr. Crevaux visited the villages of natives in the Orinocco delta, collecting interesting anthropological data.

THE Central Union for Commercial Geography at Berlin intends to erect a Commercial Geographical Museum in that city. The preliminaries are so far completed that a hall for exhibiting the objects has been hired, a provisional committee formed, and the bye-laws printed. The Union is rapidly extending its branches all over Germany. Among the latest foundations are those at Cassel, Marburg, Hanau, Frankfurt, and Wiesbaden, *i.e.* no less than five in the province of Hessen-Nassau.

AT the May meeting of the Berlin Geographical Society the latest news of the German explorers in Africa were communicated to the members. A letter from Dr. Rohlfs was dated from Gondar. The traveller knew nothing of the death of the King of Abyssinia. The disposition of the king for the further journey of Dr. Stecker was very favourable, and the latter was to leave at once for Shoa-land with a guide. Dr. Pogge and Lieut. Wissmann had arrived at Malange on January 25. Here they intended to stay a while before leaving for the interior. Dr. Buchner arrived at Malange on March 8 on his return journey, and met Major von Mecho. Buchner's misfortune of being obliged to turn back after three unsuccessful attempts to penetrate further is already known to our readers. From Madagascar a letter was received from Dr. Hildebrandt. He left Tananarivo on February 17, and travelled southwards with great hopes of a speedy success.

A LETTER from Dr. Junker to the Austrian Consul at Chartum dispels all the rumours afloat regarding his supposed assassination. He only returned to his station in December last from the journey he had taken. He first crossed the Welle River and travelled in a westerly direction to the Mangbattu tribe. Then he proceeded to some Government stations in Eastern Mangbattu land, getting a little beyond Munsa's former residence, in the vicinity of which is Miam's tomb (not in the Niam Niam land, as indicated by the Italian map). The traveller crossed the Gadde and Bibali rivers at their confluence, and then returned to his station.

THE new number of *Le Globe* opens with a continuation of M. de Morsier's papers on the plains and deserts of the two continents, and also contains a sketch of the geographical work of last year by M. Bonthillier de Beaumont, as well as notices of the Arctic campaign of 1880 and the *Jeannette* expedition.

SOME long letters have recently been received from Père Livinhac, the head of the Algerian Missionary Expedition in Uganda. In referring to the organisation of the country he says that under the *Kabaka*, or absolute monarch, are the chiefs of the great families, called *Mohamis*, of whom three specimens came to England last year with Messrs. Wilson and Felkin. After these come chiefs of inferior rank, who own allegiance to the *Kabaka* through the *Mohamis*. Last of all is the class of slaves or *Wadu*. Mtesa, Père Livinhac says, is regarded by his subjects as a species of divinity, and they attach a supernatural virtue to objects which he has touched. He however appears to be very much under the influence of a clique of *Mohamis*, who threaten to dethrone him if he encourages foreigners.

THE Queensland Government have lately issued a large scale map of part of the Colony, on which is laid down the proposed route of the transcontinental railway to Point Parker, on the Gulf of Carpentaria. This, we observe, crosses the lower course of the Gregory, where, according to a recent official report, that river overflows and covers most of the plains for a considerable distance on either bank. It is difficult to reconcile this uncomfortable fact with the report of Mr. Watson's expedition, to which we lately referred, that high ground ran right down to Point Parker. If this be really the case, the surveying expedition must have followed a different course to the westward of that laid down for it, possibly crossing the Gregory at a much higher point in a comparatively unknown part of the country.

WE regret to learn that Père Law, whose unfortunate expedition from Gubuluwayo to Umzila's country was referred to in NATURE of May 5, died of fever and general exhaustion at that chief's kraal last November. During his comparatively short stay in Africa he had rendered conspicuous service to geography by the determination of numerous heights and positions.

A VERY interesting experiment is to be tried in West Central Africa by the members of the Livingstone (Congo) Inland Mission. We understand that seeds of the different species of Chinchona, which have been obtained from the Government plantations in India, are to be sent out to them with a view to ascertaining whether it could be successfully cultivated in the mountain valleys of the Congo.

IT is probable that the successor of Admiral La Roncière le Noury, late President of the French Geographical Society, will be M. Ferdinand de Lesseps.

SOLAR PHYSICS—SUN-SPOTS [1]

TO the student of science who contemplates the sun by day or the stars by night two questions will inevitably occur. The first will have reference to the source from which those vast orbs have derived their stupendous store of high-class energy; the second to the astonishing regularity with which they are able to give it out. It is not impossible to measure in a rough way the amount of heat which our own sun must have possessed. For in the first place we are forced to allow that our luminary must have shone as it does now for millions of years. In the next place the amount of solar heat received by the earth in one year will about liquefy a layer of ice 100 feet thick covering the whole surface of the earth; and lastly, the sun gives out 2,300,000,000 times as much heat as the earth receives.

These considerations viewed together will perhaps enable us to form a faint conception of the amount of light and heat which our luminary must have given out during its prolonged existence. And yet the sun is by no means one of the most powerful stars, being only about the average in brightness.

We ask then, in the first place, from what source has this inconceivably vast store of energy been derived? If science be not able with absolute certainty to reply to this question, it is yet able to indicate the most probable origin of the supply.

The only hypothesis yet thought of that can account for it is that which first occurred to Mayer and Waterston, and which has been worked out by Helmholtz and Thomson in such a way

[1] Lecture in the Course on Solar Physics at South Kensington; delivered by Prof. Balfour Stewart, F.R.S., April 27.

as almost to prove that there is no other known power capable of producing such a stupendous result.

According to this hypothesis we may imagine the particles of matter, when originally produced, to have been at a great distance from each other, all however being endowed with the power of gravitation—forming in fact a chaotic mass. As these particles gradually came together in virtue of their mutual attraction, heat would be generated in the condensing mass, and it has been calculated that this cause, by storing up a vast amount of heat in the sun, is sufficient to account for its wonderful outpouring of heat and light throughout a long series of ages.

But the whole of the riddle is not thus solved. A man may have vast resources and yet a total absence of ready money. Or a nation may have plenty of food and yet not be able to bring it fast enough into a famine-stricken district. And so the sun may possess in its interior abundance of high-class energy and yet be unable to bring it quickly to the surface—indeed it has been calculated by Sir William Thomson that if the sun were an incandescent solid body its surface would probably cool in a few minutes of time. The perplexing fact about the sun and stars is not so much that they have somehow obtained a vast store of energy, as that they are able to bring it to the surface with an astonishing regularity. Nevertheless this regularity, great as it is, is not apparently perfect. There are a good many examples of variable stars of which some few suffer sudden and extreme changes of brilliancy, while in others the variation is much less conspicuous. In these orbs the transport service by which the heat is brought to the surface appears to work unequally, and even in some cases to break down altogether. Were we much nearer to them than we are we might study these inequalities with advantage, and perhaps gain some insight thereby into the nature of the wonderful machinery that brings the heat to the surface.

As it is however we must chiefly confine ourselves to a study of the sun. Can we therefore hope to find out the nature of the machinery by which the light and heat of our sun are brought to the surface? and is this machinery unequal in its action? Is the sun, in fine, a variable star? First of all, let us have a clear conception of the precise meaning of this question. No doubt the clouds by day and the earth itself by night interpose themselves between us and our luminary so as to render its direct influence exceedingly variable; but this is not the point. Frequently in passing along the streets of an evening we see into the interior of some room which has just been lighted up; but immediately the blind is pulled down, and we see it no longer. The gas may however be all the while burning behind the blind with a constant lustre; or it may be that from water in the pipe or some other cause the flame is intermittent. Now this is the point which we wish to determine about our sun. Is sunlight intrinsically constant, or is it subject to variations? and if so, can we determine the extent and the periods of these variations? Now at first sight it seems exceedingly strange that we are compelled to ask this question.

It might naturally be imagined that astronomers, who can give us the light variations of Beta Lyræ or some other variable star with the greatest precision, must certainly be able to give us similar information about the sun. That they are totally unable to do so is unquestionably very strange. When however we begin to examine we find several reasons for this curious failure. In the first place we must all be glad to think that within historic times at least the variations of the sun's light-giving power can never have amounted to a large proportion of the whole. Had this been otherwise none of us could have been alive at this moment to speculate on solar variability.

Nevertheless these suspected differences, although not exceedingly great, may still be large enough to enable astronomers in some remote part of the universe to pronounce our sun to be a variable star. How is it then that we who are mainly concerned in this variability are yet unable at first sight to decide upon the fundamental question of its existence?

We have not far to seek for an answer to this enigma. The fact is we are too near and too deeply concerned in the issues to be able easily to detect the variation. We have never the opportunity of comparing the sun's light with the pure light of the stars in the way in which we can compare the light of one star with that of another. We must therefore resort to means by which the direct light and heat of the sun may be accurately measured. Now it cannot be said that instruments for this purpose do not exist, but they have not been systematically made use of to determine this important point, and indeed there almost appears to be a reluctance in humanity to face the fact of the sun's variability.

When, in process of time, the telescope came to be invented, by its means Fabricius and Galileo speedily discovered that the face of our luminary was not altogether free from spots. This fact had been previously known to the Chinese, who in the course of their long and peculiar civilisation had recorded many instances where such spots were large enough to be visible to the naked eye. But at present we have to do with the progress of European thought. The first accurate observer of these phenomena was Hofrath Schwabe of Dessau, a distinguished German astronomer. More than fifty years ago he set himself to the task of taking frequent sketches of the disk of the sun, which might record approximately the positions and areas of the various groups of spots. For forty years he continued to labour at this somewhat monotonous task with great perseverance, until at length his unwearied labours were crowned by a singular and unlooked-for discovery. This consisted in the evident periodicity of these phenomena. During some years Schwabe found the sun to be almost entirely free from spots, while on other occasions the solar disk was mottled over with very frequent groups, the period from maximum to maximum, or from minimum to minimum, being nearly eleven years. From the observations of Schwabe and others it would appear that 1828, 1837, 1848, 1860, and 1870 were years of maximum-spot frequency, while 1833, 1843, 1856, 1867, and 1877 were characterised by a nearly total absence of spots. Carrington, of this country, followed in the steps of Schwabe, and gave the world a very accurate record of the spots which appeared from 1854 to 1860 inclusive.

In 1858 De La Rue introduced the application of photography to solar research, and since then photoheliographs have been at work at Kew, Ely, and Greenwich, in this country, at Wilna and Moscow in Russia, at Mauritius, Melbourne, India, and Cambridge, U.S., more or less continuously up to the present time.

I can only allude to the magnificent solar pictures produced by Langley at the Alleghany Observatory, and more recently by M. Janssen, the distinguished French astronomer, as forming a new point of departure in the history of solar delineation. Janssen's pictures are more than a foot in diameter, and in them every minute detail of the sun's structure is accurately represented.

But it is time to tell you what a sun-spot really is. Prof. Wilson of Glasgow made in 1774 an observation which greatly startled the scientific world. He found that sun-spots behaved exactly as if they were caverns with sloping sides dug into the body of the sun. The bottom of these caverns is generally black, while the sloping sides are less so. The black portion is therefore called the *umbra*, while the less black sloping sides are called the *penumbra* of the spot. It is easy to explain the nature of Wilson's reasoning. The sun, it is well known, revolves on its axis about once in twenty-six days from east to west, so that a spot will take about thirteen days to travel across the visible disk or hemisphere. It will come on at the left-hand border or limb and disappear at the right, provided it remains so long. Now Wilson noticed that when a spot is near the limb the penumbra on the side nearest the sun's visual centre is hidden from our view, on the same principle by which, when looking into a silver jug, for instance, from one side of it, that interior which is nearest the eye is hidden from the view. In fine, he concluded, with perfect justice, that spots were pits or hollows with sloping sides, and we are justified in adding that they are cloud pits, and not caverns of solid matter.

These conclusions of Wilson have been abundantly confirmed by the Kew observers, Mr. De La Rue and his colleagues, and also by the spectroscopists who have devoted themselves to the sun.

It has furthermore been shown by these observers why the bottoms and sides, but more especially the bottoms, of such caverns should be blacker than the sun's ordinary surface. They are blacker because they are colder, and they are colder because they represent a down-rush of matter from the high and comparatively cold regions of the solar atmosphere—of some kind of celestial hail, we may perhaps imagine. So magnificent is the scale of operations that fifty or sixty of our own earths might be dropped into the cloud-cavern formed by the down-rush—at least in the case of large spots.

But a down-rush implies an up-rush, and we may add that a down-rush of matter comparatively cold implies an up-rush of

matter comparatively hot. We have abundant evidence of the existence of such up-rushes in the sun. Astronomers have been long familiar with the existence of two solar phenomena which occur together—*spots* and *faculæ*. Just as a spot represents something which is blacker, and therefore colder, than the ordinary solar surface, so a facula (torch) represents something which is brighter, and therefore hotter, than the surrounding regions. As I have said, faculæ and sun-spots accompany each other, and we have evidence from various quarters that the former are not merely high up in the solar atmosphere, but that they frequently represent matter in the very act of ascending, just as a sun-spot frequently represents matter in the very act of falling down.

If we turn now to those regions of the sun's disk in which there are no spots we do not find a uniformly luminous appearance. We find rather a fine mottled or granular surface consisting of certain bright patches and of others comparatively dark. The black patches may perhaps be regarded as very minute sun-spots, and the bright patches as faculæ on a small scale. Probably, too, the bright are up-rushes of comparatively hot, and the dark down-rushes of comparatively cold matter.

Thus we may imagine that the difference between a spotted and an unspotted portion of the solar surface does not consist so much in a difference in the kind of things there present as in their size. In the unspotted portion we have down-rushes and up-rushes side by side but on a small scale, while in the spotted region we have also down-rushes and up-rushes, but on a large scale.

It thus appears that a prominent characteristic of the solar surface is the presence side by side of gigantic up- and down-currents, the up-rushes consisting of very hot and very bright matter carried upwards from the heart of the sun, while the down-rushes consist of comparatively cold matter carried downwards from above.

We may add that this system of currents appears to be in all respects most powerful during periods of maximum sun-spots, at which times the velocities of solar matter are absolutely enormous. By a spectroscopic method we can estimate these velocities, and we find that on some occasions they reach the almost incredible speed of 150 miles per second.

As yet, however, we have only added another to the puzzles of solar physics. We began by expressing our astonishment at the power which the sun possesses of continuously pouring out vast quantities of heat and light, and we must now add to this our astonishment at the almost incredible velocity of its surface currents. We are thus presented with a couple of wonders instead of one; but is it not possible that the one of these may explain the other?

May not these gigantic currents denote the very machinery we are in search of, and in virtue of which the sun becomes able to carry light and heat from the interior to the surface, so as to give us a continuous and powerful supply?

The sagacity of the late Sir John Herschel was not behind in detecting the true state of the case. He suggested the probability that at times of maximum sun-spots the *sun-pot*, as he expressed it, may be in reality boiling very fiercely, and may therefore be giving us more of what we all want instead of less —be in fact preparing for a banquet instead of making arrangements for a famine.

Indeed we may be perfectly certain that the peculiar machinery which enables the sun to continue shining must be something which brings up with great promptitude to the surface new particles of hot matter from within, while it carries down with equal promptitude those that have already performed their light-giving office.

The sun is required to fire off without intermission a vast number of light- and heat-shots into space. And the battalions of particles that have done their work must quickly step behind to reload, while their places must be taken as quickly by a fresh and unexhausted levy of particles from within. Now this recruiting process, *which must exist*, can surely be nothing else than those violent up-and-down atmospheric currents which observation reveals to us on the surface of our luminary, and we are thus entitled, as a matter of speculation, to infer that our earth will probably receive peculiarly large supplies of sunlight on those occasions when there is most manifest disturbance on the surface of the sun. In fact we may regard the sun as a species of heat-engine. The ordinary conception of such an engine is that of something provided with cylinders, pistons, valves, wheels, and other mechanical appliances, the furnace and the boiler being kept generally out of sight; but the physical conception is something very different from this. A heat-engine, according to the physicist, is a machine having two temperatures: one being that of the source of heat, and the other that of the refrigerator: and it produces work while heat is carried from the higher to the lower temperature—from the source to the refrigerator, and not only so, but the faster the heat is carried the more work does it produce.

Here the object or end is to produce work, and the means employed is the carriage of heat. But if we regard the sun as an engine we may with propriety reverse this relation between means and end, and look upon the carriage of heat and light to the surface as the end aimed at, and the powerful surface-commotion as the means by which this end is accomplished.

I am by no means satisfied that we can fully explain why the currents on the sun's surface should be so very violent as observation proclaims them to be, but yet it is easy to see that the conditions there present are such as to favour the development of convection-currents of enormous power. Let us agree for a moment to study an ordinary furnace fire. We have here in the first place a carriage of hot air up the chimney which ultimately mixes with the cold air outside, while we have in the second an in-rush along the floor of the room of the cold air which feeds the fire, and which ultimately as hot air goes up the chimney and mixes with the cold air above. Now here we have a true convection-current, an up-rush of hot and an in-rush of cold air, and the more intense this current the more quickly will the fire burn.

It is easy to see in the first place why the hot air ascends the chimney. It does so because it has been expanded by heat, and is therefore specifically lighter than the cold air around it.

But why does a thing specifically lighter than the air ascend? Clearly on account of terrestrial gravitation. If there were no earth it would not ascend at all, and if the earth were less massive than it is it would not ascend so fast as it now does. Clearly then the draught of our chimneys depends upon the mass of the earth.

Again, the draught will depend upon the intensity of the fire, and also upon its size and that of the chimney, for it is obvious that an exceedingly small fire and short chimney would not draw well even though the temperature of the fire should be very high.

We thus perceive that the intensity of convection-currents depends—

1. On the temperature of the source of heat as compared to that of the cold parts of the arrangement.
2. On the force of gravity.
3. On the scale of the arrangement.
4. We may add that for strong currents it is necessary to have some substance, such as air, that expands greatly under an increase of temperature.

And furthermore such currents are still more augmented in violence by the presence of a condensable substance in the atmosphere, and are thereby rendered abrupt, and, to some extent, incalculable, in their operations, inasmuch as a small cause may produce a very great effect.

Now we have all these elements of power together on the sun's surface. For in the first place the intensity of the sun's heat is very great as compared with the cold of surrounding space. Secondly, solar gravity is very great, being about twenty-eight times greater than terrestrial gravity. Thirdly, the scale of the whole arrangement is very great; and lastly, the substance there present, gas and vapour, is one which expands greatly on being heated. On the sun's surface therefore all these causes of convection-currents exist in great strength; and if we bear in mind that they must be multiplied together rather than added we shall not fail to perceive how strong must be the effects which they will produce. Notwithstanding all this, it appears to me that we have more to learn with respect to the causes which produce the extraordinary violence of solar currents.

Although the series of sun pictures made by Schwabe is the first having pretensions to accuracy, yet Prof. Rudolph Wolf has endeavoured to render observations of sun-spots made at different times and by different observers comparable with each other, and has thus formed a list exhibiting approximately the relative number of sun-spots for each year. This list extends back into the seventeenth century, and is in many respects of much value.

By this means Prof. Wolf has shown that the eleven-yearly

period runs through all the recorded observations of sun-spots since the telescope came to be used. And furthermore it appears that these eleven-yearly oscillations are not always of the same magnitude; sometimes they are large, and sometimes small. They were probably small about the middle of last century, becoming large towards the end of it; they were again small about the early part of the present century. They have recently been large, and we may suspect that in future there will again be a falling off.

Besides exhibiting this complicated periodicity, sun-spots have many other characteristics, the most prominent of which I will now bring before you. Of these the most peculiar is a proper motion of their own. If there were no sun-spots it would be very difficult to determine the elements of the sun's rotation. Accordingly sun-spots have been used for this purpose ever since the telescope was invented. They are carried by the solar rotation from east to west across the visible disk of the sun in about thirteen days, and hence we may conclude that the sun, roughly speaking, rotates round its axis in twice thirteen, or twenty-six days. But Carrington found that spots move fastest when nearest the solar equator, and slowest when nearest the solar poles; and in consequence of this proper motion of spots there is an uncertainty as to the exact period of solar rotation. Another point of interest is the distribution of spots over the solar surface. There are never any at or near the sun's poles, the zone in which they break out having its limits about 30° on each side of the equator. It might be expected from this that we should have a maximum of spots close to the equator, but such is not the case. There are very few at the equator, the maximum number corresponding to a solar latitude of about 15° north or south. We must not however conclude that spots invariably exhibit a preference for this latitude, for Carrington has shown that on certain occasions they appear by preference to seek a higher latitude, widening out on each side of the solar equator simultaneously, while at other times they prefer a lower latitude, coming together towards the equator simultaneously on each side.

Dr. Smysloff of the Wilna Observatory has likewise observed a sort of hemispherical see-saw in the behaviour of spots. Sometimes they prefer the northern hemisphere of the sun—at other times the southern; but this observer is inclined to think that if we pursue our researches for a length of time sufficiently great we shall find an equal amount of spots in each hemisphere.

I have thus endeavoured to bring before you the fact that sun-spots exhibit curiously complicated laws of a roughly periodical nature. Two questions arise from this discussion: the one is of a theoretical nature, and has reference to the possible causes of this behaviour; while the other is of great practical as well as of theoretical interest, and has reference to the effect which these strange solar phenomena produce upon the magnetism and meteorology of the earth and upon the general well-being of the human race.

To be continued.

PROF. ALLMAN ON THE DEVELOPMENT OF THE CTENOPHORA

In accordance with his usual practice of making his anniversary address at the Linnean Society an exposition of recent progress in certain departments of zoological research, the President on this occasion (24th May, 1881) selected as his subject the advances which, during late years, had been made in our knowledge of the development of the Ctenophora.

He referred especially to the beautiful researches of Alexander Agassiz, and to those of Fol, Kowalewsky, and most recently of Chun. He pointed out the phenomenon to which he was the first to call attention, that immediately after the earliest stages of the egg cleavage a remarkable peculiarity shows itself, in the fact that the continued cleavage is no longer uniform, but takes place much more energetically in certain cleavage spheres than in others, whereby the former are broken up into a multitude of small cells, which gradually envelop the latter, thus giving us at this early period of embryonic development the foundation of the two germinal leaflets, ectoderm and endoderm. He showed, how the body thus formed becomes excavated by an internal cavity, which soon communicates by an orifice with the exterior, thus presenting, as shown especially by the researches of Chun, the condition of a *gastrula*; how the gastrula-mouth becomes afterwards closed by the continued extension over it of the ectoderm; how a new orifice, the permanent Ctenophore-mouth, makes its appearance at the opposite hole, the ectoderm here becoming invaginated, so as to form the permanent stomach which opens into the central cavity, which becomes the "funnel" from which spring all the vessels which are destined to distribute the nutritive fluid through the body; how, in the spot formerly occupied by the gastrula-mouth, certain cells of the ectoderm become differentiated, so as to form the rudimental nervous system; and how the great vascular trunks are formed by the differentiation of portions of the endoderm, into which offsets extend from the central cavity.

Prof. Allman further referred to the facts connected with the metamorphoses which the larvæ of the Ctenophora undergo between the moment of leaving the egg and the attainment of the mature form—facts for which we are mainly indebted to the researches of Alexander Agassiz and of Chun. He showed how the lobed section of the Ctenophora, as proved by the investigations of A. Agassiz on *Bolina*, and by those of Chun on *Eucharis*, are at first quite destitute of the "lobes" which constitute so characteristic a feature in the adult; and how the young Ctenophore has at this time all the characters of the more simply constructed Cydipidæ, *Eucharis* being also compressed like a *Mertensia* in the direction of the stomach axis, while in the adult the compression of the body is at right angles to this; how the lobes afterwards grow out laterally from the oral side of the body; how the meridional vessels at first ending in blind extremities extend themselves into the rudimental lobes, and there form the anastomoses and rich convolutions which become so striking in the adult, the stomach vessels finally entering into the anastomoses.

He also referred to Chun's remarkable discovery of the sexually-mature condition of the very early larva of *Eucharis*, from which was reared a young brood which returned to the larvæ form from which it originated.

Chun's observations on the metamorphoses of the Venus's girdle (*Cestum Veneris*) were also dwelt on. It was shown how the young cestum had a nearly globular form, and possessed all the essential features of the Cydipidæ, so that notwithstanding the extremely aberrant characters of the adult the young may be taken as affording a type of the gastro-vascular system, with the distribution of the vessels in the Ctenophora generally. The gradual extension of the Cydippe-like larva in the direction of the funnel-plane changes it into the long, flattened, band-like form of the adult, and brings about (with modifications in the number and direction of the swimming-plates, and the substitution of new tentacles to replace those of the larva which had disappeared) the singularly aberrant course of the vessels characteristic of the mature Venus's girdle.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Mr. J. W. Clark is to be re-appointed Superintendent of the Museums of Comparative Anatomy and Zoology for two years, at the end of which time the Council of the Senate appear to anticipate that some fresh arrangements as to this office may be made.

Mr. W. Hillhouse, Assistant Curator of the Herbarium, will give a course of lectures on Morphology and Systematic Botany during July and August, suited to candidates for the Natural Sciences Tripos. In connection with the course there will be practical work in the Gardens and Botanical Laboratory; and a botanical excursion will be made, weather permitting, on Wednesday in each week.

DUBLIN.—The Professors of the Medical School in Trinity College have, at the suggestion and with the sanction of the Rev. Dr. Haughton, the senior lecturer of the College, introduced into their summer courses of lectures, to a very large extent, practical instruction, instead of the time-honoured and now somewhat antiquated series of prelections. These summer courses chiefly consist of Chemistry, Histology, Botany, Comparative Anatomy, and Operative Surgery. In the Chemistry, instead of listening as formerly to an hour's lecture three times each week, the students work in the laboratory under the superintendence of Prof. Emerson Reynolds, F.R.S., for two hours every alternate day, and on one day in each week attend a demonstration by the Professor on the analysis of water, air, and articles of food. In the Histology Prof. Purser gives a lecture on one day in each week, at the close of which illustrative preparations are shown in the laboratory. On the other days the students are engaged on practical work in the new physiological laboratory, where, as in the chemi-

cal laboratory, each student has his own place, with full set of apparatus and reagents. The laboratory is open from 11 a.m. to 5 p.m. In Botany, Prof. E. Perceval Wright gives the first ten lectures to the class in the lecture-room, on the general details of the structure and morphology of flowering-plants. The second part of the course consists of ten demonstrations on such forms as Bacteria, Yeast-mucor, Saprolegnia, Oidium, Mushroom, the Algæ; and the remaining portion is given in the College Botanical Gardens, when each student is required to have a practical acquaintance with a certain number of natural families. The demonstrations in Comparative Anatomy are conducted by Prof. Macalister, F.R.S., who is fortunate in having the resources of the gardens of the Zoological Society to assist in the practical work of his class. So far as the experiment has this session gone, the results have been most happy, the students showing a far greater interest in their work, and the demonstrations being less formal than the lectures, they have greater facilities for asking questions.

THE fiftieth anniversary of the foundation of the Technische Hochschule at Hanover is being celebrated during this week. Numerous guests from all parts of Germany, as well as from England, Sweden, Norway, the Netherlands, and Russia have arrived at Hanover.

SCIENTIFIC SERIALS

Journal of the Academy of Natural Sciences of Philadelphia, vol. 7, second series, part 4, 1874-1881.—Wm. M. Gabb, descriptions of Caribbean Miocene fossils; descriptions of new species of fossils from the Pliocene clay-beds between Limon and Moen, Costa Rica, together with notes on previously known species from there and elsewhere in the Caribbean area (with four plates).—Andrew Garrett, on the terrestrial mollusca inhabiting the Cook's or Harvey Islands.—Dr. C. Chapman, the placenta and generative apparatus of the elephant (with three plates).—Dr. Joseph Leidy, on some parasites of the Termites (gives full descriptions, with figures, of the strange forms briefly described in the *Proceedings* of the Academy of Natural Sciences, Philadelphia, for 1877. *Trichonympha agilis* is a truly extraordinary form, possibly a protozoan intermediate between the Gregarines and Infusoria).—Dr. Joseph Leidy, remarks on *Bathygnathus borealis*.

Zeitschrift für wissenschaftliche Zoologie, Bd. 35, Heft 3, 1881.—Dr. J. W. Spengel, the organ of smell and nervous system in the mollusca, a contribution to our knowledge of the unity of the molluscan plan (plates 17, 19).—Dr. O. Bütschli.—Short contributions to a knowledge of the Gregarines: (1) on the development of *Gregarina* (*Clepsidrina*) *blattarum*; (2) on the power of adhering in *Monocystis magna*, and on the pseudonavicella in the monocysts of the earth-worm; (3) on some egg-shaped Psorospermia in the intestine of *Lithobius forficatus* (plates 20, 21).—Prof. F. E. Schulze, researches on the structure and development of sponges, x. On *Corticium candelabrum*, Schdt. (plate 22).—Dr. A. Gruber, on the process of division in *Euglypha alveolata* (plate 23).—B. Ulianin, on the development in amphipods (plate 24).—Dr. Paul Fraisse, on molluscan eyes with an embryonal type (plates 25 and 26) (Patella, Haliotis, Fissurella).—Dr. P. A. Loos, on the albuminiferous glands in amphibia and birds (plate 27).

Atti della R. Accademia delle Scienze Fisiche e Matematiche, Napoli, vol. vii.—F. Panceri, the phosphorescence and the phosphorescent organs in some Annellida (Chætopterus, Balanoglossus, Polynoe), plates 1 to 4.—On the seat of the phosphorescence in some Campanularia (with a plate), and observations of some new species of marine nematoid worms (*Desmoscolex elongatus* and *D. lanuginosus*, *Echinoderes meridionalis*, *E. minutus*, *E. eruca*, and *E. spinosus*, *Tristicochæta inarimense*, n.gen. et n.sp. near Chætosoma); all the new species are figured.—A. Costa, notes of a visit to Egypt, Palestine, and the coasts of Turkey (zoological).—V. Cesati, on a new species of Battarræa (*B. Guicciardiniana*), with a plate.—On a collection of Pterydophytes made at Borneo by Signor D. Beccari, with 4 plates.—G. Licopoli, on the fruit of the vine and its chief constituents, with a plate.—G. A. Pasquale, on a new species of Lonicera (*L. stabiana*), with a plate.—F. Gasco, account of the whale (*Balæna Biscayensis*, Esch.) captured at Taranto on February 9, 1877, with 9 plates.—G. Battaglini, on projective geometry.—E. Fergola, on the dimensions of the earth.—G. Nicolucci, the Cola grotto near Petrella di Cappadocia, in the province of Abruzzi, with three plates of animal remains.—On prehistoric researches about the environs of the Lake of Lesina.—L. Palmieri, on the present condition of electrical meteorology.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 12.—"Investigations on the spectrum of magnesium," by Professors Liveing and Dewar.

The flame of magnesium burning in air shows strongly, generally well reversed, the line at wave-length 2850 (Cornu); a strong triplet, resembling in the relative distance of its lines the other magnesium triplets, close to the solar line M; the well-known triplet near L; and a remarkable series of lines and bands, generally resembling the spectra of some compounds, extending from M to between L and K. Neither the strong triplet at M nor this series of bands are represented in the spectrum of magnesium either in the arc or spark. These flame-lines are remarkable as further evidence of the power of substances to emit, at comparatively low temperatures, radiations of short wave-length.

In the arc the authors notice a series of triplets, of which the least refrangible lines have wave-lengths about 2767, 2736, 2798, 2673, 2649, 2633 respectively. These, with the well-known triplets *b*, and those near the solar lines L, P, and S, form a series, similar to those described by the authors in the spectra of sodium, potassium, and lithium, in which the alternate members are sharp and diffuse, and succeed one another at shorter and shorter intervals in a way which indicates that they follow a definite law and are probably harmonically related, though not forming a simple harmonic series. They observe that the line w.l. 2850 is the strongest line of magnesium both in the flame and arc, and one of the strongest in the spark, and that it is nearly the octave of the line some time since observed by them at wave-length about 5710. They observe in the arc only the strongest two of the quadruple group described by Cornu from the spark at wave-lengths 2801, 2795, but they notice both in arc and spark a group of five lines a little higher at wave-lengths about 2782, 2781, 2779, 2778, 2777. All these lines, including the diffuse members of the series of triplets, they have often observed reversed when the arc is taken in one of their crucibles. The line w.l. about 4570, so conspicuous in the flame, first noticed by the authors in the spark, is well seen and easily reversed in the arc, and they believe it to be represented in the solar spectrum by the line w.l. 4570·9 in Ångström's map. Besides these lines they observe in the arc a pair of lines slightly less refrangible than the pair in the spark, described by Cornu near the solar line U. In the spark they observe two pairs of ghost-like lines below the triplet near L, which together with the fainter two of the quadruple group (2801, &c.), seem to suggest the possibility that some of the particles of magnesium have, owing to particular circumstances, their tones a little flattened in regard to these particular vibrations, though the constancy in the amount of displacement of the lines militates against such an hypothesis.

In regard to the *b* group, observations on the spectrum of the fourth order given by a Rutherford grating of 17296 lines to the inch showed that the iron line in b_4 is a little less refrangible than the magnesium line. The additional lines near this group observed by Fievez they ascribe to a periodic inequality in the ruling of the Rutherford grating, arising from an imperfection of the screw of the ruling machine, which produces a series of *ghosts* on either side of each principal line. The positions of these *ghosts* have been investigated mathematically by Peirce (*Math. Journal* of John Hopkins University), and observations of them tally with the theory. They are embarrassing in the case of bright lines, but may be detected by their changes of position in the spectra of different orders.

The magnesium-hydrogen spectrum which the authors have previously investigated and found to be produced at ordinary and reduced pressures when both elements are present, but not otherwise, they have now investigated further by observing the spark discharge between magnesium poles in hydrogen, nitrogen, and carbonic oxide, at pressures varying from one to twenty atmospheres. They find that in hydrogen, when no Leyden jar is used, the peculiar fluted spectrum of magnesium hydrogen is much more brilliant at higher pressures, becoming fully equal in brightness to the *b* group, notwithstanding the increase in

brilliance of that group by the higher temperature of the discharge in the denser gas, while the other lines of magnesium fade, and even the hydrogen lines C and F are invisible. This disappearance of the hydrogen lines they ascribe to the large quantity of magnesium carried over in the spark. When a Leyden jar is now connected with the coil the fluted spectrum continues bright for some time, but gradually fades as the continuance of the high temperature sparks decomposes the compound. In nitrogen the flutings are only seen at first, probably from hydrogen occluded, as is usually the case, in the magnesium, and disappear when the pressure is increased, and cannot be made to re-appear either by reducing the pressure or varying the discharge. In carbonic oxide the behaviour is similar. The authors conclude that the production of this spectrum is dependent on a combination of the two elements, and is not merely a matter of temperature. They describe this spectrum as consisting of three parts, a group of three sets of flutings in the yellow commencing at about the wave-lengths 5618, 5566, 5513; a group of two sets of flutings in the green commencing at about w.l. 5210, and close to *b*; and a group of two blue bands with their less refrangible edges at about w.l. 4849 and 4803 respectively. The authors differ from Ciamician, who has figured this spectrum, in regard to the number of flutings in the green and blue. This spectrum can be well seen when sparks are taken from a solution of magnesium chloride in a tube full of hydrogen.

After simplifying the spectrum of magnesium by separating that of magnesium hydrogen, and supposing the triplets to be harmonically related, and perhaps some of the single lines similarly related, there are still a greater variety of vibrations than any very simply constituted particle could be expected to be capable of assuming.

Linnean Society—Anniversary Meeting, May 24.—Prof Allman, LL.D., F.R.S., president, in the chair.—There was a very numerous attendance of the Fellows.—The Treasurer (Mr. Frederick Currey) read his Annual Report, stating that financially the Society continued prosperous. The invested capital at the present date is 3868*l.* 4*s.* 6*d.*, the sum of 140*l.* derived from Fellows' life compositions during the past twelvemonth having been invested in consols. The balance at the bankers at the end of the financial year (April 30) was 532*l.* 10*d.*, and at the bankers' and on hand at this date (May 24) 604*l.* 15*s.* 10*d.* The annual contributions amounted to 928*l.* 4*s.*, and sales of publications 230*l.* 19*s.* 7*d.*; there was an increase in the admission fees, and decrease in compositions. 104*l.* 8*s.* 3*d.* had been expended on the purchase of books for the library, and 48*l.* 1*s.* 11*d.* towards bookbinding and stationery; 765*l.* 18*s.* 2*d.* had been spent on the Society's publications. A handsome donation of 50*l.* had been made by Mr. G. Bentham.—The Secretary (Mr. B. Daydon Jackson) then read his Report. Since the last anniversary eleven Fellows of the Society had died and four had withdrawn. Against this thirty-seven new Fellows had been elected, besides one Foreign Member and one Associate. During the past year there had been received as donations to the library 106 volumes and 125 pamphlets and separate impressions of memoirs; from the various scientific Societies there had also been received in exchange ninety-six volumes and 248 detached parts of publications, besides twenty-three volumes of donations from the editors of independent periodicals. There had been purchased 90 volumes of importance, among these 63 serials, equal to 10 volumes, the total additions to the library therefore being 315 volumes and 373 separate parts. Mr. Kippist had presented framed water-colour sketches of Dr. Robert Brown's birthplace, London residence, and Sir J. Banks's library. The Society's collections and herbaria had been duly examined and reported on to the Council as in good condition. After a service of fifty years Mr. Kippist had resigned his position as librarian to the Society, and the Council, in acknowledgment thereof had granted him a retiring pension.—Mr. Baker, in the name of Mr. J. W. Miers, then presented to the Society a portrait of his father, the late John Miers, as a memento of his connection therewith, and Prof. Allen Thomson as representative of a small committee also presented the portrait of Prof. St. George Mivart, late Zoological Secretary, to the Society.—Prof. Allman then delivered his anniversary address, the subject chosen being "Recent Advances in our Knowledge of the Development of Ctenophora."—The Secretary afterwards read obituary notices of the several Fellows that had died during the year, making special mention of the life and labours of Mr. G. R. Alston, the late lamented Zoological Secretary, Mr. John Gould, the ornithologist, Mr. Gerard Krefft, of Sydney, Dr. J. Lander Lindsay, and Mr. R. A. Pryor, of Herts.—The scrutineers having examined the ballot, then reported that Mr. Alfred W. Bennett, Mr. Francis Darwin, Prof. E. R. Lankester, Sir John Lubbock, and Mr. Geo. J. Romanes had been elected into the Council in the room of E. R. Alston (deceased), Dr. T. Boycott, Prof. M. Foster, Dr. J. Gwyn Jeffreys, and Prof. St. George J. Mivart, who retired; and for officers Sir John Lubbock was elected president, Mr. F. Currey, re-elected Treasurer, Mr. B. Daydon Jackson, re-elected Botanical Secretary, and Mr. G. J. Romanes was elected Zoological Secretary.

Zoological Society, May 17.—Dr. A. Günther, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited and made remarks on examples of four parrots of the genus *Chrysotis* from various islands of the Lesser Antilles.—A communication was read from Mr. Carl Bock, in which he gave an account of the land- and fresh-water shells collected in the highlands of Padang, Sumatra, and in the eastern and southern parts of Borneo during his travels in those districts. Eight new species were described.—A communication was read from Mr. G. B. Sowerby, jun., containing descriptions of eight new species of shells from various localities.—Mr. W. A. Forbes read a paper on the anatomy and systematic position of the Jacanas (*Parridæ*), which he showed were in no degree related to the rails, but form a separate group, to be placed amongst the plovers and allied birds (*Limicolæ*). The author also called attention to the peculiar form of the radius in the genus *Metopidius*, which is not developed in the other genera of this group.—A communication was read from Mr. L. Taczanowski, C.M.Z.S., containing the description of a new species of weasel from Peru, proposed to be called *Mustela Jelskii*, after its discoverer.—A communication was read from Mr. W. F. Kirby, containing a description of the hymenopterous insects collected in Socotra by Prof. Bayley Balfour. Of these two were apparently new to science.—A communication was read from Mr. Francis Day, F.Z.S., containing remarks on the range of *Apogon Ellioti*.

Royal Microscopical Society, May 11.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Ten new Fellows were elected and nominated.—Amongst the objects exhibited were Seibert's polarising microscope, Nachet's binocular dissecting microscope, Verick's skin microscope, Houston's botanical microscope, embryological sections from the Naples Zoological Station, &c.—Papers and notes were read on a new and remarkable annelid (Mr. Stewart); the markings of diatoms (Dr. Matthews, Mr. J. Deby and Count Castracane); a new species of *Hydrosera Wallich* (Dr. Stolterforth); and improvements in illumination (Mr. J. Smith); also a note by Prof. Abbe on the conditions of micro-stereoscopic vision, with special reference to the fact that the lineal amplification of an object in depth is equal to the square of the lineal amplification in breadth, reduced, however, in proportion to the refractive index of the medium in which the object is: thus an object under a power of 100 times would be magnified 10,000 times (lineal) in depth if in air, 7500 times if in water, and 6600 if in oil or balsam.

EDINBURGH

Royal Society, April 4.—Prof. Fleeming Jenkin, vice-president, in the chair.—In the absence of the author, Prof. Alleyne Nicholson read a descriptive note by Dr. Carmichael McIntosh on the Phoronis, one of the specimens obtained in the dredging operations of the *Challenger*. This *Annelid* was of special interest as indicating in several particulars close similarity to certain well-known *Cœlenterates*.—Dr. R. H. Traquair read a paper on additional researches on the structure of the *Palæoniscidæ* and *Platysomidæ*. The more that the fossil remains of these families were studied the more difficult did it become to clearly differentiate them from each other; and from his later researches Dr. Traquair had been obliged to adopt a wider definition of *Palæoniscidæ* than he had formerly employed. Hence it appeared that the criticism that had been urged against his former papers, viz., that he had not shown sufficient ground for their separation as two distinct families, could still be urged with even greater reason; but this, according to the doctrine of descent, was a natural consequence of increased information. Certain of the specimens he had examined, though retaining the most essential characteristics of either family, as the case might be, possessed modifications of structure which were ordinarily typical of the other. The evidence was that the

Platysomidæ had been derived from the *Palæoniscidæ*.—Prof. Tait communicated a number of results of experiments on the heating effects of compression—experiments that had been suggested by his recent inquiry into the pressure errors of the *Challenger* thermometers. Specimens of various substances, such as raw potato, raw flesh, cheese, pith, bar-soap, liquorice, cork, india-rubber, &c., were compressed suddenly in a hydraulic press. The rise of temperature was measured by means of a thermoelectric junction set into the heart of the material, and connected to a delicate galvanometer in a contiguous room. After the heat evolved by the compression had diffused itself, so that the galvanometer came back nearly to its old zero, the pressure was suddenly relieved, and the heat absorbed in the return of the substance to its original bulk was measured by the reverse current produced. In most cases this cooling effect was exactly equal to the original heating effect. Cork however, which rose in temperature 1°·3 F. for one ton's weight increase of pressure, fell in temperature only 0°·9 F. on removal of the pressure—a result in striking accordance with the well-known sluggishness of cork in recovering its original bulk after withdrawal of the pressure that had compressed it. In this particular, india-rubber, whose temperature was raised about 1°·5 F. for the same increase of pressure, was markedly opposed to cork. Potato and raw flesh both indicated about the same rise of temperature—0°·7 F. Pith, again, which resembles these in the large percentage of water that enters into its constitution, was raised in temperature only 0°·37 F., giving no perceptible difference of effect over what would have been produced by water alone.

PARIS

Academy of Sciences, May 23.—M. Wurtz in the chair.—The following papers were read:—On the old Observatory of Cairo, by M. de Lesseps. This observatory no longer exists; but M. de Lesseps, in a letter here given, urges its restoration on the Khedive. It is stated that the Egyptian Institute in Cairo has received the *Connaissance des Temps* complete from 1679 to 1866.—Nebulæ discovered and observed at the Observatory of Marseilles, by M. Stephan.—On the genera *Williamsonia* Carruth., and *Goniolina* d'Orb, by MM. de Saporta and Marion.—On an experimental process for determining the sensibility of the retina to coloured luminous impressions, by M. de Grandimont. The observer looks steadily at the central point of a disk having apertures, beyond which coloured surfaces are placed. By degrees the sensation of colour is extinguished. On suddenly putting white surfaces in place of the coloured, the complementary colours come out with great brightness and purity. The instrument is named a *chromatroposcope*.—Barometer based on the equivalence of heat and pressure on the volume of a gas, by M. Decharme. It consists simply of an ordinary alcohol or mercury thermometer, and an air thermometer to show the volumes of gas corresponding to the temperatures observed. To find the atmospheric pressure, the data of these instruments are interpreted by the aid of curves and tables previously prepared.—On a cryptogam insecticide, by M. Lichtenstein. This is a *Botrytis* (same genus as that which infests the silkworm), found to have killed all the aphidians on a cineraria in the Jardin des Plantes at Montpellier. It operates only in the hothouse, and M. Lichtenstein could not inoculate either phylloxera or other plant-lice with it in open air.—On the geometry of spheres, by M. Stephanos.—On Fuchsian functions, by M. Poincaré.—The standards of weights and measures at Paris Observatory and the apparatus used in their construction; their origin, history, and present state, by M. Wolf. This note relates chiefly to the toise du Perou and du Nord (so called), which the author considers to be now in the same state as when they came from the hands of Langlois in 1735.—On the law of radiation, by M. Violle. This law may be represented between 0° and 1775° by the formula $I = mTb^{T^2}a^T$, where T represents the absolute temperature, m a constant coefficient, b the number 0·9999938, a = 1·03550 − 13 λ, λ being the wave-length in millimetres.—On the production of sound by the force of radiation, by Prof. Ball.—On radiophony; thermophone reproducing the voice, by M. Mercadier. Light is reflected from a plate of silvered glass, behind which is a small air reservoir, with a thin plate of mica or caoutchouc, which receives the voice through a tube. At the receiving-end, one of the author's small glass tubes, with a smoked piece of mica and ear-tube, is brought where the light is concentrated by a lens or concave mirror. With this arrangement speaking was distinctly, though faintly, heard at about 20m. distance, the windows of the speaker's room being closed.—On the same subject, by the same. A long tube (10m.) may be used by the speaker. Electric light and (less easily) oxyhydrogen light, gave the effects. Solar radiations act best when hottest. Alum solution interposed reduced the effects considerably; with a thin ebonite plate they were weakened, but still distinct; similarly with plates of tinsel of zinc, copper, aluminium, &c., $\frac{1}{10}$mm. to $\frac{1}{20}$mm. thick.—Modification of the Neef interrupter for the Ruhmkorff coil, by M. Ducretet. The vibrating slip is lengthened and its two ends fixed to two columns; the small soft iron mass in the middle. The vibrations are very rapid; the spark is modified, becoming continuous, more powerful, and hotter.—On the rotatory power of artificial codeine, by M. Grimaux.—On the proportion of carbonic acid contained in the air, by MM. Muntz and Aubin. This gives results of observation, by a method formerly described, at Paris and a station in the country. They appear mainly to confirm M. Boussingault's and M. Reiset's results. The proportions of CO_2 in normal air vary within but narrow limits. In the environs of Paris, whatever the direction of the wind, figures were got closely similar to those of M. Reiset near Dieppe.—Preliminary study of reactions without the intervention of a solvent, by M. Lorin.—On silicomolybdates, by M. Parmentier.—Action of ammonia on chloride of isobutylene, by M. Oeconomidès.—On some points relative to the organisation and the development of Ascidians, by M. Van Beneden. This relates to his observations at the Naples station in April last. The development of Ascidians does not (he considers) warrant the radical distinction drawn by the brothers Hartwig between a mesoderm and a mesenchyme.—The vessels of the ink-bag of cephalopoda, by M. Girod.—On the troubles of sensation produced by cortical lesions of the brain, by M Couty. The observations were upon apes and dogs. Anæsthesia, when it occurs, affects the side opposite that of the cortical lesion, and for touch, as for vision, is always incomplete. It is rare, and it has no relation to the place or extent of the cortical lesion. These troubles of sensibility have no necessary connection with other troubles.—Mechanism of infection in different methods of inoculation of symptomatic *charbon*; application to the interpretation of clinical facts and to the method of preventive inoculations, by MM. Arloing, Cornevin, and Thomas. An abortive *charbon* may be given by inoculation in the veins, in the connective tissue, or in the respiratory passages.—M. Richard announced the discovery of a cavern in the mountain of Ayuso (Segovia), containing a large number of prehistoric *débris*.

CONTENTS

THURSDAY, JUNE 9, 1881

THE STEPHENSON CENTENARY

GEORGE STEPHENSON was born June 9, 1781. The importance of this event to us who now inhabit civilised countries is certain; for whatever view we may take as to the inevitability of railways, it is matter of history that for twenty-five years—1815 to 1830—after Stephenson had to all intents perfected the system of railway and locomotive, which still holds its own, no other engineer or competent mechanic went even so far as to admit its merit. It is therefore to George Stephenson that we are indebted for our existing railways, for the immense extension of mechanical contrivance which has followed in their train, and for all that these have done for us in the way of improving the circumstances of life.

As the custom of centennial celebrations has become almost universal, it would partake of irreverence to allow the hundredth anniversary of the birth of one who has given us so much to pass unnoticed. But in what form can we celebrate such an event? No oratory can remind us of Stephenson's name when we continually hear the puffing of his engine. What monument can compare with the cuttings and embankments seen whichever way we turn? In truth Stephenson's works are ever before the eyes and sounding in the ears of all people. We have no political or social purpose to serve by a national ceremony. Killingworth or Newcastle will have its dinner and, as we understand, the intention is that some money should be subscribed for an educational foundation. This is all very well, but it is confined to a few who take a special interest in the place, and is no measure of that universal offering to the memory of our hero which goes up, not once in a hundred years, but hourly.

To the readers of NATURE who are not only of the travelling public, but to whom doubtless the works of Mr. Smiles are familiar, anything we can say as to the life and work of Stephenson must seem totally inadequate. But not to let the occasion pass we will endeavour, by reference to some of the features of Stephenson's work, to illustrate a thought which has recurred to us with ever-increasing force when considering the works of those who have pioneered the way in practical mechanics. This thought may be expressed somewhat as follows:—That if we are to accept the proved ability to predict results with certainty as conclusive evidence of a knowledge of the laws and principles on which these results depend, then it is evident that acute observation of mechanical and physical phenomena does lead to a very clear insight into the laws and principles involved, although the observer may be—generally has been—altogether unable, save by the prediction of results, to give definite shape to his abstract ideas, and much more to give them articulate expression. And further, that this apprehension of principles, acquired by the observation of the dependent phenomena, is the only real apprehension, and is a very different thing from that knowledge or conviction of the truth of principles which comes from reading or argument, and which, however useful for purposes of criticism, rarely if ever leads to a prediction.

In the instance of Stephenson we have a perfect example. He received absolutely no education except by his own observation of the animals and other works of nature in the vicinity of his dwelling, and the rude mechanism of the surrounding collieries. Such too were the exigencies of his existence, that although he was assiduous in the task of self-instruction, as in all other things, in 1815, at the age of thirty-four years, and at the very time when he was making his first engines, "Blucher" and "Puffing Billy," the first of a race destined to overrun the earth and create the greatest of all revolutions, though he could read and write he had not as yet mastered the rule-of-three. Yet in the construction of these very engines he showed his confidence in results, the prediction of which shows that he had acquired an insight into principles which were entirely unexpressed at that time, and as regards some of which their expression is still incomplete.

Amongst the mechanism of the railway, almost every detail of which was conceived by Stephenson, there are certain details or features which, with a view to rescue them from being altogether claimed for other inventors, the friends of Stephenson have ever marked as bearing more distinctly the impression of Stephenson's hand. These are the smooth driving-wheel, the chimney blast, and the multitubular boiler. This is as it should be.

But, as it seems to us, in thus bringing into prominence the special features of Stephenson's system, Stephenson's friends have effectually diverted attention from that which is of far more importance. Thus, although it has never been claimed for Stephenson that he was the first to use smooth driving-wheels, Trevithick and Hedley having been obviously before him, it is contended that Stephenson consistently from the first maintained the sufficiency of the adhesion, while the others invented "imaginary difficulties" which led them to contrive all sorts of means of preventing the wheels of their locomotives from slipping. This view of the matter is however essentially wrong, and is unfair to both sides, for on the one hand, while there is no evidence to show that Trevithick or Blenkinsop ever ignored the tractive power of smooth wheels, neither is there any evidence to show that Stephenson ever maintained that the adhesion of smooth wheels would suffice to accomplish that for which the rack was being used. Had he done so he would have been wrong. But, on the contrary, there is ample evidence to show that Stephenson clearly perceived—that at the very onset he determined by careful experiment—the limit of the adhesion of his smooth wheels, and that he never attempted to use them except on a level road. The question at issue is much broader and more important than that of mere mechanical contrivance. It was as to how far the locomotive should be set to the task of the horse in drawing its load over the hills and valleys, and how far the hills should be cut down and the valleys filled up.

This, the level road, the very form of the railway, was Stephenson's main idea. And it was his foresight and determination in respect of this that made his railways a success from the first. His experience and observation had led him to perceive what all subsequent experience has confirmed, that the locomotive, in virtue of its size and clumsiness, could only be usefully employed on a nearly level railway. He did not actually maintain that

it would be impossible to make a machine that would travel on common roads and even mount hills, but "even suppose that such a machine could be constructed to carry twenty or thirty passengers at ten miles an hour, put it on a level railway and it would carry 200 or 300 passengers at thirty or forty miles an hour."

In his first colliery railways at Killingworth and Hetton he laid the lines in a series of flat reaches separated by inclines, and working the inclines by fixed engines, confined the functions of the locomotive to drawing the waggons along the flat reaches. It was this insistence on the level road that enabled him to use smooth wheels, and not that he had discovered any adhesion previously unknown or that others had overlooked.

Stephenson's position was a nearly level line at any price on which the adhesion of the wheels is sufficient as against a road following the slope of the country, for which, according to his view, by whatever means the adhesion might be increased, the iron horse was ill adapted. In the clear conception of the importance of this level road, coupled with his determinate insistence in carrying out his view, no matter what the difficulty—the veritable removal of mountains—have we not the best of all proof that, however unconsciously, he was guided by a perception of that law which connects the limits in size and activity of structures, with the strength of the material of which they are composed. And by which law we may now perceive that it is only by smoothing the road and so reducing the call for strength and power that we have made our machines to exceed in size and speed the limits which Nature had reached in her animals.

Into another law, called the Conservation of Energy, there can be no doubt that Stephenson had an insight far beyond his time. He saw that the conveyance of a load was not a question of force, but of the product of force into the distance traversed, and that however great might be the tractive power of his engine, its speed must depend on the ratio of the rate at which steam could be generated to the load. So long therefore as the tractive power was so large as—compared with the steam-generating power of his boiler—to prevent his engine, when fully loaded, travelling at more than ten miles an hour, he could gain nothing by increased adhesion. But, on the other hand, in his first engine the desideratum was increased steam-generating power for the same weight of boiler.

With, as Robert Stephenson has told us, the direct object of accomplishing this, George Stephenson turned the exhaust steam in the form of a jet or blast up the chimney of his second locomotive, "Puffing Billy." If this is so, and there appears no evidence to the contrary, it was a prediction with regard to the motion of fluids, for the making of which there is as yet no established law in the theory of hydrodynamics. That the result is such as was here predicted, or that a jet of steam or of air playing at high velocity along the interior of an open-ended tube does impart motion to the air within the tube and causes a current, is of course now well known, but our present knowledge is derived from the experience of the locomotive chimney. There is no evidence that it was known to any one before 1815, nor indeed has there been found any other mechanical purpose of general importance in which the same action could be usefully employed. Neither in the stationary engine nor yet in the marine engine has it proved economical. Thus the locomotive and its offspring, the portable engine, were the only machines possessing this organ.

Although it has been the custom for writers on the steam-engine to speak as though the manner of action of the blast were self-evident, this only shows that these authors have not understood it—indeed how should they? The general law on which the action of the blast depends is that a jet of fluid issuing into surrounding fluid at rest will not, when it has more than a certain velocity, proceed in a straight vein or column, but begins at once to wriggle, and as it advances involves itself in an extremely complex manner with the surrounding fluid, with which it shares its forward momentum. It is only during the last few years that the generality of this action and the circumstances on which it depends have attracted attention, and the completeness with which the action has been overlooked is shown by the numerous attempts that have been made to invent fanciful explanations of the following phenomenon. When a jet of steam, say half an inch in diameter, issues from a high-pressure boiler, as from a gauge cock, although the steam itself must have the temperature of boiling water, still the hand may be held in the jet at a distance of two or three inches from the cock without any inconvenience. How has the temperature of the steam become lowered? is the question for the answering of which numerous hypotheses have up to quite recently been invented. The answer is that the temperature of the steam does not become lowered, any more than the strength of the mustard in a sandwich, but that the steam has involved within its column layers of cool air, sandwich fashion, and as the combination rapidly passes the same point of the skin, the sensation produced is that of the mean temperature of the air and steam.

It is on this action of a jet to mix itself up with the surrounding medium that the draught produced by the blast up the chimney depends, and Stephenson's confident prediction of this draught is the best evidence that observation had led him to a perception of the more general action.

Considering the capacity of the man as shown by his other work, it would have been a matter for surprise had not Stephenson acquired a unique knowledge of the phenomena of fluid motion. He had the best opportunity for observation—his whole time had been spent in the care of pumps, pumping-engines, and the arrangements for ventilating and draining mines. His habit was to bring all his ideas at once to the test of experiment; and in devising his safety-lamp he had carried out a very careful series of experiments on the behaviour of jets and the rate of their admixture with the surrounding air.

Although, as shown by the employment of the multitubular boiler, Stephenson's mechanical insight does not perhaps stand out in so very clear a light, inasmuch as he made this step at the suggestion of Mr. Booth, still it cannot have been fortuitous that in adopting these small tubes he should have at once introduced all those conditions on which their employment is alone successful.

That small tubes of the same aggregate capacity as a single flue afford greater cooling surface for the hot gases is indeed obvious, but it was Stephenson's own observation that taught him that such increase was desirable,

while the fact that the gases in passing through the small tubes would encounter much greater resistance than in the single flue rendered the successful employment of the multitubular boiler dependent on the increased action he could give to the blast. However in all respects he came out right in the very first trial.

In the "Rocket" he had a self-moving machine, which resembled the moving animal not only in the fact that they both derived their power of motion from the combustion of carbon, but the physiology of the machine resembled that of the animal system in that essential particular which connects the action of the heart and lungs with that of the muscles, so that any demand upon the activity of the latter is at once met by increased activity in the former. In the locomotive the law of adjustment is perfect. Whatever the load within the limit imposed by the adhesion of the wheels, and whatever the speed, the stimulating action on the fire is sufficient, and no more than sufficient, while in all cases the tubes are sufficiently long, and no more, to pass the heat generated into the boiler.

The functions of the locomotive engine more nearly correspond with the functions of moving animals than do the functions of any other machine, and hence it was essential that there should be a correspondence between the organisation of the locomotive and that of working animals, which correspondence may be dispensed with in other engines. Is it not probable, we ask, that he who produced the locomotive physiologically complete had been guided, however unconsciously, by the truth of his observation of those animals which his machine was to set free from their task? OSBORNE REYNOLDS

THE HISTORY OF SALT

The History of Salt. By Evan Marlett Boddy. (London: Baillière, Tindall, and Cox, 1881.)

THIS book is quite a literary curiosity: the author hopes, and not without reason, that it will be found to afford amusement. Mr. Boddy we take to be a medical student, and it is a kindness to him to suppose that he is young. After reading the first half-dozen pages of his work the idea gradually dawned upon us that he intended it for an elaborate joke, very much after the manner, we should suppose, of Mr. Benjamin Allen and Mr. Robert Sawyer, had those gentlemen been tempted to follow the paths of literature. But, *adhibenda est in jocando moderatio*, and never more so than when the joke is at the expense of a venerable parent. In dedicating his work to his father Mr. Boddy, for the credit of human nature, must be acquitted of the charge of a conscious joke, otherwise such an instance of filial disrespect would be without parallel.

This astonishing production owes its origin to a letter advising total abstention from salt, which had appeared in a temperance journal, and the author felt himself constrained, for the good of humanity, to deliver himself of the succession of "*farcical puerilities*" and "*whimsical crudities*" which make up the "*imaginative plerophory*" "*redundant of inane folly and trivial hyperbole*" of his book. The words in italics are Mr. Boddy's; he of course applies them to the opinions of other people. With the sanction of Vespasian's law, that it is unlawful to give ill language first, but civil and lawful to return it, we think ourselves justified in applying them to Mr. Boddy's book. And how richly that book merits them we proceed to make abundantly clear, and on the author's own showing.

Mr. Boddy is too hard upon the unfortunate letter-writer in the journal of temperance: he is not even grateful to him as the remote cause of the existence of his own book. The letter-writer, "with amusing self-complacency, accused it [salt, not temperance] of producing evils of an astounding nature—such is the latitude of pragmatical ignorance and silly egotism. The palpable absurdity of such an argument must be apparent even to the most careless thinker: it is with the view of exposing such a fallacy, both injurious and irrational, that I have written this treatise." One is tempted to ask—If the argument is so palpably absurd, even to the most careless thinker, why in the world has Mr. Boddy taken the trouble to write his treatise?

It does not seem to be generally known what would happen to a world devoid of salt; such, according to Mr. Boddy, is the "dense obtenebration with which the subject is surrounded." The picture of a saltless world, as drawn by our author, is something awful to contemplate. Nothing but the thought of "our ignorant conceits," our "unaccountable obliquity of judgment," and "the apathetic indifference" with which we have hitherto looked upon the humble condiment which has graced our tables "in the smallest receptacles, as if it were the most expensive article," and to which we, "in the most finical, grotesque manner," help ourselves "in almost infinitesimal quantities, as if it were a mark of good breeding and delicacy," would compel us to reveal the "imaginative plerophory." The nervous reader will be pleased to fortify himself with at least a teaspoonful of the condiment before he begins its perusal.

"Were the human race once deprived of the chloride of sodium, even for a limited period of time, we should not only lose a natural healthful incentive for our food, but disease, with all her attendant miseries, would spread with such relentless impetuosity as would defy, and even paralyse, the efforts of the most skilful physician, the ingenuity of the surgeon, and the scientific improvements and hygienic precautions of the sanatarian. The strength and vigour of manhood would fade as if blasted by disease, food would act as a poison, the blood would not be replenished with the salt which it requires, and consequently our skins would soon be covered with corruption, our cattle would die, our crops would be nipped in the bud, the air would be full of offensive insects, the soil would become foul and barren, the sea a waste of stagnant waters, and all the beautiful productions of nature would wither and decay, and our glorious earth would degenerate into a hideous solitude, solely inhabited, very probably, by monsters horrible to behold, more repulsive than those gigantic reptiles which once roamed by the dreary marshes of an incomplete world."

And yet, according to Mr. Boddy, "the English working classes are nearly, if not altogether, unacquainted with the benefit of salt": "at the tables of the wealthy it is perfectly absurd to see the small amount which is used." We are not even allowed the poor consolation of knowing that in our false economy we are unwittingly conserving our choicest blessing. "We do not diet ourselves as we should: in this respect we are far behind the veriest

savage, cannibal though he be." We cannot inure ourselves to salt at too early an age; we ought indeed to pickle our babies: "To rub new-born infants with salt" is a practice "in every respect cleanly" and "strictly conducive to health."

Mr. Boddy has evidently spent much pains on his history: but, as he confesses, in trying to begin at the beginning he has laboured under many difficulties. He has traced the history of salt from the time of Moses and Job by the aid of such written records as he has been able to meet with, but on the question of its history before their time he is obliged to fall back on his inner consciousness.

"The origin of salt is one of those enigmas of nature which as yet has completely frustrated the most accomplished and scientific geologists, and no suggestion has yet been made which will satisfactorily and conclusively account for its formation; for whatever hypothesis has been stated there is sure to be an objection so difficult to overcome that the author has been fain to admit that it is thoroughly impracticable, and therefore inadmissible."

Even our author is fain to express himself guardedly on this point—

"If we take salt as a whole, leaving out of the question altogether the different conditions in which it is found, and with no reference at all to its existing either in the earth, above the earth, in lakes, or in the sea, but looking at it simply as it is, a mass of rock, or a volume of water holding it in solution, it inclines one to the belief that it possesses a dual inchoation, though the original source of both may have been connate; but owing to extraneous causes which were brought to bear, one branch became crystallised rock-salt, while the other, through immaturity, remains in a state of solution."

"Why the sea is salt" has given rise to many pretty fables: Mr. Boddy invents still another fable; but it is not at all pretty: it is that "sea-water is the result of some subterranean communication with reservoirs of salt through the media of volcanic foci" (p. 53). This perhaps hardly does justice to Mr. Boddy's powers of narrative: the picture of the saltless world proves that he can do better; and yet even this is surpassed by that of the insect world of Cheshire on a rainy day (p. 60). But it is scarcely fair in the interests of the book itself to quote all its best things, even if our space and the reader's patience were longer.

Mr. Boddy is apprehensive of the reviewers: "An unknown author is placed at a great disadvantage and at the mercy of those who may laud a book to the skies if they please, satirically criticise another, and pass over a third with a sarcastic smile or a significant shrug of the shoulders. I am afraid that my little volume will unfortunately be found among the latter, but I candidly acknowledge that I hope it will be regarded as belonging to the first, or at least the second."

Our theory of the origin of this book differs somewhat from that of its author, as given above; Mr. Boddy's father (to whom the book is dedicated) was, we are informed, a ship's surgeon; and it occurs to us that this book is the result of the molecular motion of a brain which can trace its ancestry to a prolonged regimen of salt junk and pickled pork. It is the most striking instance of heredity we have yet met with, and despite our fear that Mr. Boddy may describe our notion as "a brazen assertion and a subtle paralogism," we commend it to the notice of Mr. Francis Galton.

OUR BOOK SHELF

Text-Book of Practical Organic Chemistry for Elementary Students. By H. Chapman Jones. 100 pp. (London: Joseph Hughes, 1881.)

MOST teachers of organic chemistry have felt that if their students could be made to work through a fairly simple series of typical experiments the work of learning would be rendered easier, and the knowledge gained would be made more definite and more real. Just such a series of experiments is described in this little work by Mr. Chapman Jones. The experiments are well chosen and clearly described; no costly apparatus is required, yet the student who works carefully through the book will certainly have laid a solid basis of knowledge of organic chemistry on which he may build a satisfactory structure.

An outline of methods whereby organic acids may be detected is given towards the end of the book, but the main part is devoted to experiments illustrative of fractional distillation and precipitation, formations and general properties of leading hydrocarbons, alcohols, and acids, etherification, &c.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Trevandrum Observatory

As I was reading in a recent issue of your valuable journal (vol. xxiii. p. 482) a letter on the magnetic storm of August, 1880, showing the universality and simultaneity of the disturbance by comparing the observations at Greenwich, Toronto, Zi-ka-Wei, and Melbourne, I felt curious to know whether any such disturbance was noted here in the Government Observatory, and if so, whether the time corresponded with that given in your paper. On my application the gentleman in charge of the Observatory put into my hands, the inclosed abstract for the whole month of August, which I herewith forward to you. It contains, as you will see, not only the magnetic observations with the unifilar, bifilar and balance, but also the meteorological data for the necessary correction, &c. The reference throughout the paper is to the local time, which may be easily reduced to the Greenwich time, as the longitude is given. The observations, I may add, are quite reliable, though made by native agency, and I hope may prove useful on this occasion. But the paper inclosed I fear is too long to find room in your crowded columns, and what I beg you to do is to place it at the disposal of any of your scientific contributors or friends who take an interest in the question of terrestrial magnetism, and may be therefore expected to make use of the material here furnished.

A word more before I close. Your readers might know this observatory, said to be situated near the magnetic equator, was once in a very flourishing condition under the direction of Mr. John Allan Broun. On his retirement to Europe the establishment was reduced and a limited series of observations introduced, which he continued to direct till his recent demise. Since then the observations recorded are lying unused for the want of a scientific chief. If any scientific gentleman or society should generously offer some help in the way of directing the labours of this institution, I venture to think that the Government would gladly avail itself of such help, and the cause of science could then be materially promoted. P. SOONDREM PILLAY

H.H. the Maharajah's College, Trevandrum, Trevancore, South India, May 6

Symbolical Logic

FRESH criticism of my logical writings in a work just published ("Symbolic Logic," by John Venn, M.A., Fellow and Lecturer in the Moral Sciences, Gonville and Caius College, Cambridge) must be my excuse for troubling the editor and readers of NATURE with a third letter on the above subject.

On page 94 of his work Mr. Venn strikes the key-note, as it seems to me, of its whole purport and spirit. "Those who propose a new notation," he says, "commonly, and not unnaturally, assume that it is to supersede all others. But those who approach it as strangers know that the odds are decidedly that it will only prove one more of those many attempts which perplex and annoy the lecturer, historian, and critic. Hence we may fairly use the argument, dear to those in authority, that if we loosen the sanctions of orthodoxy, heresies will multiply. Only those whose professional employment compels them to study a number of different works have any idea of the bewildering variety of notation which is already before the world. . . . No doubt it would be rank intolerance to forbid such new attempts, *but an attitude of slight social repression towards them may serve to check too luxuriant a growth of new proposals.*"

The italics are mine. Alas, how little Mr. Venn appreciates the irrepressible restlessness of that most ungovernable organ, the human brain, if he really thinks that the "attitude of slight social repression" which he recommends would have the desired effect! Amateur logicians, as well as professionals, *will* start theories and invent notations of their own in happy unconsciousness that they are causing any annoyance to "lecturers, historians, and critics," whom indeed they not improbably picture to themselves (when the all-absorbing nature of their occupation allows them to think of them at all), as ardent devotees of science like themselves, who will be delighted with the new instrument of research which they hope to place in their hands. And more provoking still, scientific societies and editors (including a goodly number of the said lecturers, historians, and critics) *will* print in their *Proceedings* and magazines new proposals which they think likely to prove interesting or valuable, without being influenced by any motive whatever beyond a pure and simple desire to further the progress of science.

Mr. Venn professes great admiration for the late Prof. Boole's genius, and I heartily agree with him, though we admire on somewhat different grounds. I ground my admiration on the fact that Boole worked wonders with an unnecessarily complicated and otherwise defective symbolical method of his own invention. Mr. Venn apparently grounds his admiration on the singular supposition that Boole's method is really very simple and very effective, but that its author did not understand very clearly the real principles of its construction, and did not by any means apply it with as much ease and dexterity as he might have done. I am quite sure that this is not the impression which Mr. Venn intended to create in the minds of his readers, but I am no less sure that this is the impression which a perusal of his book *will* create in their minds—at least in the minds of such as have not read Boole himself. One remark of Mr. Venn's surprises me. He says (p. 385) that Boole "justly regarded his problems in Probability as the crowning triumph of his system." Surely I am not mistaken in my impression that I have somewhere seen Mr. Venn quoted as holding an opinion very much at variance with this statement—in fact attacking as erroneous the very principle on which Boole's "General Method in Probabilities" is based. May I ask Mr. Venn this plain question, Does he or does he not agree with Boole's solution of the question which he proposed on pp. 321 and 336 of his "Laws of Thought" as a decisive "test of the sufficiency of received methods," and (by implication) of the efficacy of his own General Method?

The main points on which Mr. Venn and I differ are the following:—

1. Mr. Venn maintains that the sign + in such expressions as $x + y + z$ should in logic, as in ordinary algebra, be always understood in an *exclusive* sense, so that unless we know x, y, z to be mutually exclusive, the above expression should be written in a different and, as he admits, a much less simple form. I hold, on the contrary, in common with Prof. Jevons and several others, that since, on the non exclusive plan, the simple form $x + y + z$ may, without the slightest risk of ambiguity, be substituted at any stage of an investigation for any of its exclusive equivalents (such as $x + x'y + x'y'z$), or be replaced, if necessary, by any exclusive equivalent, the non exclusive interpretation of the symbol + gives us far more mastery over our symbolical expressions, and should therefore be preferred to the needlessly restrictive and hampering exclusive interpretation which Boole attaches to this symbol. How very serious the disadvantages of this interpretation really are is unwittingly illustrated by Mr. Venn himself on p. 262, where he finds himself obliged to admit that certain important simplifications which he discusses are "purely a matter of tact and skill, for which no strict rules can be given." If he had read my third paper in the *Proceedings* of the London Mathematical Society a little more attentively he would have found in my directions for reducing any complex disjunctive expression to its "primitive form" that these simplifications are not at all a pure matter of tact and skill, but may be obtained by a simple, never-failing, and purely mechanical process, which, however, a little tact and skill may do much to abbreviate. On the exclusive interpretation of the symbol + this process would be simply unmeaning. The problem which Mr. Venn discusses (expressed in my notation) is this:

Reduce the expression $(f:g)\ (gl:f)\ (l:f')$ to its simplest form.

By inspection [since any implication $\alpha:\beta$ is equivalent to $\alpha\beta':0$, and any compound implication of the form $(\alpha:x)\ (\beta:x)\ (\gamma:x)$ to a single implication $\alpha + \beta + \gamma : x$] this, on the non-exclusive plan, is seen to be equivalent to $fg' + glf' + lf : 0$.

Reducing the disjunctive antecedent of this implication to its *primitive form* (a purely mechanical process, as already remarked), we get $fg' + gl : 0$, or its equivalent, the compound implication $(f:g)\ (g:l')$, a result which Mr. Venn obtains apparently by a vague tentative process "for which no strict rules can be given."

2. Mr. Venn and I also hold different opinions as to whether or not symbolical logic should have signs to express relations corresponding to those of subtraction and division in mathematics. His opinion is that such signs *should* be introduced, and at once. My opinion (an opinion which I believe I share with most logicians) is that we had better not encumber ourselves with those symbols till they can be proved to subserve some useful purpose. The important question is—not, as Mr. Venn appears to think, whether such symbols can be intelligibly interpreted, but whether they will in any way help us in discovering new truths; in other words, whether they can be turned to any practical use in the solution of logical problems. If Mr. Venn can adduce a single intelligible logical problem which can be solved more simply or easily by the help of these signs than without them, I shall declare myself at once a convert to his views. So far I have come across no such problem, and must therefore for the present remain in the ranks of his opponents. As an illustration of what Mr. Venn calls the inverse method (*i.e.* division) in logic, he gives (p. 266) the equation—

$$(x + \bar{x}y)\, w = x + \bar{x}yz,$$

in which w (which denotes the books *in a certain library*) is to be expressed in terms of x, y, z (which respectively denote *philosophical* books, *divinity* books, and *protestant* books). His result is—

$$w = x + \bar{x}yz + \frac{0}{0}\bar{x}\bar{y},$$

which he translates into ordinary language thus:—

"The library must have certainly contained all philosophy and protestant divinity, and may possibly have contained any kind of works which are neither philosophy nor divinity; this latter constituent being left entirely indefinite."

Mr. Venn's data in the non-exclusive notation would be—

$$(x + y)\, w = x + yz,$$

and my result (much more simply and easily obtained) is—

$$x + yz : w : x + y' + z.$$

According to my definitions of my letter-symbols, we speak throughout of some *one* originally unclassed book, so that w, x, y, z will respectively denote the statements: It is in the *library*; It is a *philosophical* work; It treats of *divinity*; It is a *protestant* work. My result may therefore be read:—

Any work on philosophy or protestant divinity will be found in the library; and every work in the library is either philosophical, secular, or protestant. (By a *secular* work I mean simply a work that does *not* treat of divinity.)

The antecedent of w in my result is equivalent to the first "constituent" in Mr. Venn's result; but the consequent seems to me to give us much more intelligible information about the library than Mr. Venn's latter constituent ("May possibly have contained," &c.), which he truly describes as "entirely indefinite."

3. Another opinion of Mr. Venn's, unless I misunderstand him, is that all logical equations should, as a preliminary to their solution, be expressed in the form $\alpha = 0$, which, of course, is equivalent to my $\alpha : 0$. My opinion is that this, in logic as in mathematics, is sometimes convenient and sometimes not, and that we should not in logic, any more than in mathematics, tie our hands by this or any other unnecessary restriction.

The other points in which I differ from Mr. Venn have been mentioned in my former letters (see NATURE, vol. xxiii. p. 578, and vol. xxiv. p. 5), and need not be here repeated. Mr. Venn however labours under a serious misapprehension if he thinks that I attach any importance to the distinguishing features of my method *as mere barren conceptions*. Their real importance lies in the *use* which I have made of them, and this use cannot be fairly appreciated without examination of my published solutions in the *Proceedings* of the London Mathematical Society, in the *Educational Times*, and in the *Philosophical Magazine*. I must protest against that spirit of criticism which would offer two or three chipped bricks as a fair specimen of a house, and would depreciate the labours and damp the zeal of all scientific workers by unduly emphasising the undeniable fact that all logical and mathematical methods are, after all, mere combinations, developments, or extensions of a few simple truths which are the common property of all mankind. Even Boole's "actual originality," Mr. Venn tells us, though I think he means *priority*, "was by no means so complete as is commonly supposed and asserted" (see Mr. Venn's Introduction, p. 28). According to this method of criticism we might ascribe the invention of the steam-engine to the person (unfortunately unknown) who first discovered the important principle of a revolving wheel, and turned it to practical account by making a wheel-barrow.

In conclusion I must thank Mr. Venn for the compliment which he kindly pays me on p. 372 of his work, but I think it would read better without the parenthesis, "as he assures us is the case." The "assures" is a little too strong for the simple statements which I made, and which it never occurred to me that any one would dream of doubting. The compliment would also please me more if it did not so completely ignore the earliest, the most difficult, and the most important of my papers in the *Proceedings* of the Mathematical Society, namely, that which treats of the limits of multiple integrals. This part of my method (which gave rise to all the succeeding developments) resembles nothing, so far as I know, that has preceded it; and if Mr. Venn had found time to read it, the objectionable parenthesis which I have quoted would scarcely have presented itself to his mind as in any way called for. HUGH McCOLL

Boulogne, May 25

Resonance of the Mouth-Cavity

SINCE communicating to Mr. Sedley Taylor my recent observations on the capabilities of the mouth as a resonator, and forwarded to you, with my permission, for publication in NATURE, I have made the following experiments with perfect success, and believing that they will be interesting to your acoustical readers I send a list of them to you, and hope you will be able to find a place for its insertion in your next issue:—

Experiment I.—While one of the overtones of a loud prime was resounding in my mouth another person heard it distinctly, upon the ear of the latter being held near the source of resonance.

Experiment II.—While a cart—any other noisy vehicle will do as well—was going along the street, I readily tested the composite nature of the noise by the resonant capabilities of my mouth.

Experiment III.—I turned the water-tap on into a basin (the water was running with a good force), and from the noise make by the falling liquid I was able to get different sounds quite easily.

In both these latter experiments I observed, while opening and closing my mouth, that the pitch rose and fell as when one slides a finger up and down a vibrating fiddle-string.

Experiment IV.—I held down in the treble part of a harmonium—with an 8-feet stop out—several consecutive keys together, and while the notes were sounding which composed the horrible discord, I was able to single out any of them separately by the sympathetic resonance of my mouth.

This experiment can be done equally well at the organ.

Experiment V.—I held down four or five of the low keys of the organ with the 8-feet trumpet drawn, and the beats of the overtones resounded very prominently, so that by taking two contiguous ones at a time the result was like the *voix céleste* stop.

Experiment VI.—I tried several notes in the *c*—*c* octave of the clarinet organ stop, and heard the regular order of partial tones by resonance, but the even ones were weak and odd ones very strong.

Experiment VII.—I held down *c'* and *g'* on the harmonium, and heard the first coincident partials beat distinctly—due to equal temperament.

Experiment VIII.—The first five partial tones of an average bass voice were studied. The notes chosen for observation were *e♭, f, g, a♭, b♭*. To each note the vowels *A* as in *hay*, *A* as in *ah*, *E* as in *me*, *I* as in *high*, *O* as in *oh*, *U* as in *you*, were sung.

It was found that not only did the different vowels give different qualities, but that the same vowel had a different quality for almost every one of the five notes sung. The *E* as in *me* and *U* as in *you* had generally weak low overtones. The *I* as in *high*, on the contrary, gave them out well.

It will perhaps be as well to say, for the benefit of those who may not have tried to get a sympathetic resonance of the mouth, that success is likely to be sooner obtained by first practising the mouth in going from the *ou* to the *ah* shape, and from the *ou* to to the *e* shape.

It is also recommended that the ears be stopped by the fingers when doing these experiments, in order to lessen the possibility of mistaking the direct sound for resonance.

JOHN NAYLOR

5, West Park Terrace, Scarborough, June 1

"How to Prevent Drowning"

I FEAR that if persons who cannot swim place reliance on the advice given by Mr. MacCormac in your impression of June 2 (vol. xxiv. pp. 62, 101) they will hardly succeed in saving their lives should they happen to fall into deep water. It is an error to say that the "human frame, bulk for bulk, is lighter than water," for unless that frame be covered with fat beyond the average, it has a greater specific gravity than water. And after all, a tolerably fat body is lighter, bulk for bulk, than water only by virtue of the air in its lungs, and should that air be expelled by the frantic screams of the immersed person, he will soon find, if unable to swim, that the notion of his frame being specifically lighter than the water is a myth. The dead body even of a tolerably fat person being destitute of air in the lungs, sinks at once to the bottom in salt as in fresh water. The average human being, were he to permit his body to sink as far as it will, would soon find himself at the bottom of the sea or river. Besides, even in the case of a person fat enough to be lighter, bulk for bulk, than water, it is necessary that he should assume a certain position in order that he may succeed in keeping his nose and mouth above the water, and unless he learn how to do this in the water itself I doubt if instruction on dry land would ever enable him to float. We all know the story of the Hibernian who, having narrowly escaped drowning, vowed he would never enter the water again until he had learnt to swim, but we are not told if he ever qualified himself for going into the water again. To try and persuade people that by attending to certain rules they may get into deep water without the risk of drowning is to create a false confidence which will rather increase than diminish the number of deaths by drowning. Imagine a terrified person just plunged for the first time into deep water trying to recall all the directions he has read about shutting his lips, swallowing his breath, permitting his body to sink until it shall displace as much water as equals the body's weight, treading the water, and so on. Why he would require, in the midst of his agony of fear, to possess as many contradictory qualities of mind as Macbeth says no man can have. I venture to assert that no one was ever saved from drowning by following such directions as your correspondents here give. It should be stated in the plainest manner that there is no safety for a person in deep water but in a knowledge of swimming. Swimming should be taught to every boy and girl as a necessary branch of education. It has these advantages over much that is taught in schools, that it is a useful, a delightful, and a healthful accomplishment. R. E. DUDGEON

52, Montagu Square, W., June 3

Dust-winds at Hankow

DURING the spring of 1878 my attention was directed to the dust-winds which are not of unfrequent occurrence along the valley of the Yang-tse in the warm and dry seasons of the year. These dust-winds, as I observed them at Hankow, had sometimes the appearance of a dense mist; whilst at other times the air seemed to be penetrated by a fine haze; and in all cases a fine and almost impalpable dust was deposited

which was with difficulty excluded from the interior of houses. Their duration varied from a few hours to two days; and from the fact that one of the dust-winds was simultaneously experienced at Hankow, Kiukiang, and Chinkiang—a portion of the river's course nearly equal to 450 miles—I may conclude that they were not local phenomena, but possessed a considerable horizontal extension. The dust, which in all respects resembles the loam forming the banks and alluvial plains of the Yang-tse, is composed of mineral particles and vegetable *débris* —the former varying from $\frac{1}{3000}$ to $\frac{1}{12000}$ of an inch in size, and being generally siliceous or calcareous in composition.

Three dust-winds came under my observation on March 25, April 21, and May 1; all of them possessed the following meteorological conditions:—During the two or three preceding days the barometer fell, whilst the mean daily temperature rose, and in two instances the winds were light and southerly. During the continuance of the dust-winds the barometer rose, the mean daily temperature ceased to rise, and light winds with a force of 1 and 2 prevailed varying in direction from north to north-east. For a period of a day or two after the dust-winds had ceased the thermometer registered a lower mean daily temperature, the barometer continued to rise, and the wind retained the same northerly direction. In all three cases there was a disturbed electrical condition of the atmosphere: in the first instance a severe thunderstorm accompanied by heavy rain occurred on the day following; the second dust wind was accompanied at its commencement by a little thunder and lightning, but by no rain; whilst during the two days preceding the third dust-wind there was a considerable amount of thunder and lightning, together with heavy rain.

From this comparison of the prevailing atmospheric conditions in connection with these dust-winds, a more probable explanation of their occurrence may be obtained, than that which is often proposed when simply assigning a "sudden breeze" and a "hot day" as the conditions required to give rise to them. H. B. GUPPY

17, Wood Lane, Falmouth, June 1

A Singular Cause of Shipwreck

THE strange loss of the *Phœnix* off the Iceland coast may perhaps lend some interest to the following:—

Last summer was the best the Icelanders had had for long past. I regret that I can give no thermometric readings, as my instrument became useless during the voyage. The weather in the north was nearly as warm as it has been here lately. All the snow-fed rivers were very full. The Jökulá was nearly up to its high water mark quite early in the season, and the Blanda was almost impassable. Icelanders who rode with me said that they had never seen the mountains from Hof's Jökul to Eyrik's Jökul so free from snow.

I append extracts from a letter which I have just received from one of my guides. A. J. HUBBARD

St. Thomas's Hospital, S.E., June 4

(*Verbatim copy*)

"Hjedinstrofda, April 16, 1881

". . . This winter has been so uncommonly strong that none such has existed this century. The frost has been extremely severe—32° R., once at Akureyri, and 36° R. (= 49½° Fahr.) somewhere with Myvatn. . . . Ptarmigans and other birds froze to death. The farmers had spent most of their hay, and their cattle were to be killed or starve to death. All the sea was covered with ice, mostly polar ice, as far as one could see from the tops of the mountains. On the 2nd inst. the weather was mild, and on the 7th we had a real thaw, and every day since very mild and fine weather. But —— it is possible that the ice will not drive away before late in August, and no ship can come to any harbour on the northern coast; this happened in 1869.

"KRISTJÀN JÓNASARSON"

An Optical Illusion

THE optical illusion described in NATURE, vol. xxiv. p. 54, is, as I have already mentioned, referred to by Priestley (History, &c., Vision, Light, and Colours, vol. ii. p. 725). The description is as follows:—"M. Le Cat well explains a remarkable deception by which a person shall imagine an object to be on the opposite side of a board when it is not so, and also inverted and magnified. It is illustrated by Fig. 162, in which D represents the eye and C B a large black board pierced with a small hole. E is a large white board placed beyond it, and strongly illuminated, and *d* a pin or other small object held betwixt the eye and the first board. In these circumstances the pin shall be imagined to be at F on the other side of the board, where it will appear inverted and magnified, because what is in fact perceived is the shadow of the pin upon the retina; and the light that is stopped by the upper part of the pin, coming from the lower part of the enlightened board, and that which is stopped by the lower part coming from the upper part of the board, the shadow must necessarily be inverted with respect to the object." ("Traité des Sens," par M. Le Cat, Amsterdam, 1744, p. 298.)

C. J. WOODWARD

Birmingham and Midland Institute, Birmingham, June 6

THE VISITATION OF THE ROYAL OBSERVATORY

THE Report of the Astronomer-Royal to the Board of Visitors on Saturday last at the annual visitation was listened to with special interest, and indeed the attendance of astronomers and others at the Observatory was very much larger than usual, because it was generally understood that this would be the last occasion of the kind on which the veteran astronomer would be seen at his post. We learn that an appropriate address was made to him by the Board of Visitors when he announced his attention of relinquishing his official duties in order to enable him to devote all his time and energies to the researches he has now on hand.

The astronomical observations, which occupy the first part of the Report, have been carried on with the usual diligence. Most of the routine work of the Observatory seems to have gone on in the usual fashion through the last year.

One of the objects of interest on this occasion was Halley's ancient tombstone, which, after its removal from Lee Churchyard (where it had been replaced by a new stone with a facsimile of the inscription), had been placed in the South Ground, where it had been lying for several years. It has now been carefully restored, and mounted on the east wall of the lobby of the North Dome.

The sun's chromosphere has been examined with the half-prism spectroscope on 29 days during the period to which this report refers. Fourteen sun-spots have been examined on 20 days, with reference to the broadening of the lines in their spectra. The results confirm the remark that some of the lines of iron are broadened in some spots, whilst others are broadened in other spots. Displacements of some of the lines of iron towards the red, and of others towards the blue, have also been noted in the case of one spot. A remarkable spectrum of a sun-spot showing 17 strong black lines or bands, each as broad as b_1, in the solar spectrum, was observed on November 27 and 29, 1880. These bands, to which there is nothing corresponding in the solar spectrum (except some very faint lines), have also been subsequently remarked in the spectra of several spots.

For the determination of motions of stars in the line of sight, 168 measures have been made of the displacement of the F line in the spectra of 43 stars, 87 of the b_1 line in 27 stars, and 8 of the b_4 line in 4 of these stars. Of these 70 stars 16 had not previously been examined, and the total number of stars of which the motions have been spectroscopically determined is now 91. In the case of 6 of the stars observed in the last year, a dispersive power equivalent to that given by 16 prisms of 60° has been used. Ten measures have been made of the relative displacement of the F and b lines in the spectra of the east and west limbs of Jupiter.

Comet 1810 *d* (Hartwig's), and the aurora of 1881, January 31, have been spectroscopically examined.

Between 1880, May 9, and 1881, May 13, photographs of the sun were taken on 140 days, and of these 284 have been selected for preservation. There are only 8 days

out of 149 days on which the sun's disk was observed to be free from spots, whilst in the preceding year there was a complete absence of spots on 64 days out of 145.

The spectroscopic observations of all kinds have been completely reduced to 1881, May 6.

Touching the magnetical and meteorological instruments it was pointed out that the alterations of the photographic cylinders of the magnetical and meteorological instruments, which were in contemplation at the time of the last report, so as to make their time-scales the same in extent and in position on the record-sheets, have been to a great extent carried out. In the case of the declination and horizontal force magnets, two reflecting prisms with convex cylindrical front surfaces have been mounted by Mr. Simms above the registering cylinder, which has been lowered so that each prism receives the light from the magnet opposite to it. By this arrangement the traces of both magnets fall (as regards time-scale) on the same part of the sheet. The cylindrical lenses formerly used have been removed, being replaced by the cylindrical surface of the prism. The new arrangement is found to be perfectly satisfactory, and Mr. Simms is proceeding with a similar change in the case of the earth-current apparatus and of the vertical force and barometer registers.

A modification has been made in the system of determining the time-scales by the substitution of hourly breaks in the register, for the photographed hour-lines. The break at each hour is made automatically by a slight alteration in the apparatus hitherto used for registering the hour-lines. The time-scales for the declination, horizontal force, vertical force, barometer, and electrometer are now laid down in this way.

The unsatisfactory state of the earth-current register has been already noticed. After the change mentioned in a preceding section, it was soon found that the indications of the earth-current wires were disturbed by a continual series of petty fluctuations which almost completely masked the proper features of earth-currents. By cutting off the communications with those parts of the wires called the North Kent East Line and the Ladywell Line these disturbances were checked; but there remains a periodical disturbance at every hour, which entirely destroys the value and credit of the results. It seems not impossible that something may depend on imperfection of earth-communication. If this fault cannot be removed, the Astronomer-Royal proposes to return to the original system of independent wires (formerly to Croydon and Dartford).

The new pressure plate of Osler's anemometer has worked well. The limiting pressure of fifty pounds on the square foot was twice exceeded during the snow-storm of 1881, January 18.

The photographic records of the measures of magnetic earth-currents, in two directions upon the earth's surface nearly at right angles to each other, are maintained with the same regularity as those of the ordinary magnetic forces, and are preserved in readiness for reference or publication when need may require. An extensive confederacy is now organised, principally in Germany, for register of the earth-currents at several stations.

The following are the principal results for magnetic elements in the year 1880:—

Approximate mean westerly declination	18° 32′
Mean horizontal force	3·912 (in English units). 1·804 (in metric units).
Mean dip	67° 34′ 55″ (by 9 inch needles). 67 35 53 (by 6-inch needles). 67 36 3 (by 3-inch needles).

The report proceeds:—

"In respect of diurnal inequalities of magnetic horizontal force and its direction, though all measures are ready, the curves are not yet actually formed. In the last report I adverted to the usual character of these curves. Assuming it to be certain that they originate from the sun's power, not immediately, but mediately through his action on the earth, it appears to me (as I suggested long ago) that they are the effects of the attraction of the red end or north end of the needle by the heated portions of our globe, especially by the heated sea, whose effect appears to predominate greatly over that of the land. I do not say that everything is thus made perfectly clear, but I think that the leading phenomena may be thus explained. And this is almost necessarily the way of beginning a science."

The number of hours of bright sunshine, recorded with Campbell's Sunshine Instrument, during 1880, was 1214, which is about the same as the average of the four years for which we have a record.

The discussion of the electrometer results for the year 1879 shows that the potential of the atmosphere is usually positive; that it is least in summer and greatest in winter, and especially in the colder weather of winter. There is also a definite diurnal inequality, having double maxima and minima, the maxima, on the average of the year, occurring at about 8h. a.m. and p.m. and the minima at about 3h. a.m. and p.m. In character the diurnal curve has a resemblance to the barometric curve, but the points of maxima and minima precede those of the barometric curve by about two hours. On days of magnetic disturbance, when aurora is visible, nothing unusual is remarked in the electrometer indications. Excepting thunderstorms the greatest disturbances are experienced in showery weather, and are probably local disturbances only.

The time work is thus referred to:—

"In the first few years after the strict and systematic examination of competitive chronometers, beginning with 1856, the accuracy of chronometers was greatly increased. For many years past it has been nearly stationary. I interpret this as showing that the effects of bad workmanship are almost eliminated, and that future improvement must be sought in change of some points of construction. One which occurs to me (I mention it principally as a specimen of departure from customary forms) is this. The impact of the escape-wheel upon the pallet of the balance-axis takes place very near to that axis, and must produce considerable friction, though of short duration. I proposed to the late Mr. Charles Frodsham to meet this by use of a broader pallet and a lighter impact of longer duration. The decease of that accomplished horologist prevented the completion of the trials which he had commenced for carrying out my suggestion. Other variations of the established form might be worthy of trial.

"The Greenwich time-ball has been regularly dropped automatically at 1h. on every day throughout the year, with the exception of 6 days when the violence of the wind made it imprudent to raise the ball, and 8 days when the severe frost of last winter prevented its being raised; and of one day when there was accidental failure.

"The Deal ball was not raised (on account of high wind) on 10 days. It was not dropped (through failure in the telegraphic connection) on 7 days, and was erroneously dropped about 5s. too soon by telegraph signals on one day; and on another day it was not dropped at 1h. owing to telegraph signals continuing up to 1h.; on one day the current was too weak to release the trigger. On every other day the ball has been dropped automatically at 1h.

"As regards the Westminster clock, its errors have been under 1s. on 31 per cent. of the days of observation, between 1s. and 2s on 47 per cent., between 2s. and 3s. on 18 per cent., and between 3s. and 4s. on 4 per cent.

"The distribution of time-signals to all parts of the country continues to be made on the same system as in late years, by means of the chronopher at the central

office of the Post Office telegraphs. In connection with this system, I would express the hope that the proposal to establish an hourly signal at the Start Point will be borne in mind.

"Last autumn a telegraphic determination of the longitude of Leiden was made with great care by M. M. Bakhuyzen. The interchange of signals between Greenwich and Leiden occupied nearly four months. I may here remark that the American extension of longitude carried out under Commander Green, U.S.N., to which reference was made in the last report, will be most useful for the transit of Venus in 1882. Cannot a British officer be found to complete the operations for Australia and New Zealand? I lament that this has not been done. Mr. Gill has undertaken the necessary work for the Cape."

The following important general remarks conclude the report:—

"The present meeting may afford a fitting opportunity for the expression of my views on the general objects of the Observatory, and on the duties which they impose on all who are actively concerned in its conduct. Assuming as beyond dispute that these ought to be carried out in a spirit liberal in itself and honourable to the nation, I proceed to state my opinion on the line of action which they suggest.

"The object prescribed to the Observatory is the promotion of "Astronomy and Navigation." And, since the abolition of the Board of Longitude, the second of these objects (which historically gave rise to the introduction of the first) presses upon the directors of the Observatory much more strongly than before. Considering then the claims of astronomy as b aring on navigation and our responsibilities in reference to them, we find that those responsibilities are by no means narrow. Whatever the rest of the scientific world may do or may not do, we are responsible for determinations of the fundamental elements of sidereal, solar, and especially lunar astronomy, with the highest accuracy that modern skill can secure. The same apparatus of instruments and of mathematical treatment which fix the places of fundamental stars will apply to those of other stars; the same which apply to the sun will apply to planets and comets (not unconnected with solar theory, by virtue of perturbation) and even to satellites. And we could hardly consider ourselves as discharging our duty to the more educated portion of the nation, or as maintaining our proper position in the world, if we did not include in our operations these latter offshoots of the first-mentioned objects.

"But new astronomical subjects have arisen of which no one dreamed when our constitution was first fixed. The first of these was the measures of double stars. But this, though important as ever, has almost disappeared from our view when occupied with solar and spectroscopic physics. I yield to no one in the interest which I take in these subjects, and in the admiration with which I regard the positive conclusions and the problematic suggestions which are founded on them. But I still point out that these are not parts of our original system, and their connection with the Greenwich Observatory is at any time liable to question.

"I now advert to the general subject of navigation. And first I remark that magnetism, in its ordinary and nautical form, is indisputably a proper subject for the Observatory. But within the present century there have arisen:—the accurate examination of magnetic irregularities, the partial reduction of daily irregularities to practical laws (still wanting theoretical explanation), and the establishment of the simultaneous co-existence of occasional disturbances covering the whole surface of the earth. Connected with these is the observation of magnetic currents through the terrestrial soil, registered at Greenwich for many years past, and now attracting attention on the Continent. Perhaps no branch of physics bears the same prospective importance as these. Yet I conceive that their continued study in this Observatory requires special authorisation.

"The original views in making astronomy contributory to navigation were limited to observations of the moon. But in the latter part of the last century the possibility of making chronometers subservient to the determination of longitude (a subject to which the late Board of Longitude gave good attention, and to which the Government has always offered liberal rewards) was proved, and in the present century the improvement has been very great. This has been effected by our Hydrographical Office (mainly through the action of the Observatory), partly by specific rewards, partly by careful attention to the accuracy of every chronometer purchased. And the practical value of the chronometric system has been very greatly increased by taking advantage of the galvanic distribution of time currents, and by the galvanic exhibition of ball-drops and other signals. There can be no doubt that all the agencies involved in this system are well employed, and that they are a legitimate part of the Observatory duty as originally contemplated.

"Still I remark that the Observatory operations bearing on chronometric navigation are not carried out to the extent which I could desire. It is known to all persons familiar with chronometers that rates of the chronometers, obtained while the ships are actually in voyage, would possess remarkable value. We possess the power of giving facility for obtaining these to a large part of our mercantile navy, by exhibiting a time-signal at every hour, at Deal (where the necessary apparatus already exists) and on the Start Point. I have several times brought this proposal, as regards the Start, before the Government, but unsuccessfully. But I should have done wrong if I had omitted, in this general survey of the duties of the Observatory, to state my continued conviction that this is a proper and very desirable addition to the other points of assistance which we can give to navigation.

"Next—closely connected however with the subject of navigation—is the knowledge of the longitudes of distant ports, as referred to the Greenwich Observatory. And I approach this subject with grief. We have entirely abandoned the longitudes of the Atlantic, which have been cleared away, before our eyes, by the scientific enterprise of another nation. The Pacific, bearing those vast and important colonies, almost entirely British, is equally neglected; though so much is ready that the mission of a single officer would quickly establish all. The same aspiring nation which has mastered the Atlantic is now bent (as I understand) on adding to its scientific dominion the Pacific. I think this is not honourable to our nation.

"There remains another subject, which occupies no small part of the force of the Observatory, and which I am unable to connect with either of the two great divisions to which I have alluded—the subject of meteorology. It is exceedingly popular in the country, perhaps because it requires little of expense or of science. It is also pursued at many foreign observatories, where vast numbers of observations are produced without attempt at classification or reduction. We at least are not amenable to this accusation, and may appeal to our reduction of more than twenty years' collected observations as giving matter of permanent interest to the more scientific meteorologist, and even to the geologist. Still I call attention to the fact that this is a subject which, though introduced mainly by myself, I regard as foreign to the original 'Astronomy and Navigation' of the Observatory.

"There is still a matter for consideration, not in our observations, but in the mechanism by which they are made available to the world—I mean our printing. I have repeatedly expressed my opinion that the extent of our printing is far too great: not in the full exhibition of reductions, but in the minute details of individual observations. There are printed every year more than 7000 transits or circle readings, each consisting of 6 or 7 indi-

vidual readings, of which only the mean is useful. I do not believe that, since the year 1835 at least, any person in the world except ourselves has actually taken a mean. As each reading contains 3 or 4 figures, there are printed in each year something like 150,000 useless figures. Reliance must be placed somewhere on the skill and fidelity of the observer, and (considering the severity with which every figure of transit-wire and of circle microscope, and of their means, is examined here) this reliance may be placed at least as well on the means as on the originals. I have reason to think that the bulky volume of nearly 900 pages, might be reduced to about three-fifths of its present size by omitting those originals.

"I would submit for the consideration of the Board whether it might not be advantageous that they should hold a special meeting to consider the subjects which I have indicated. The length of time at an ordinary visitation, and the circumstances under which the Board meets, are not sufficiently favourable for the discussion of broad questions of Observatory policy."

HOLTZ'S ELECTRICAL SHADOWS

IN an extremely elegant series of researches Prof. W. Holtz of Berlin has lately brought to light the existence of a new class of electrical phenomena, to which their distinguished discoverer has assigned the name of *Electric Shadow-figures*. Though nearly six months have elapsed since they were described in the Proceedings of the Göttingen *Gesellschaft der Wissenschaften*, no detailed account of them has appeared in any English journal. Yet the shadow-figures are remarkably easy to produce, and the whole research is of extreme simplicity, as very little apparatus is required beyond the simplest odds and ends to be found in every physical laboratory, the only large instrument necessary being one of Holtz's electrical machines.

The fundamental arrangement is that shown in Fig. 1. From the discharging-rods of a Holtz machine the brass balls are removed. To the left rod there is attached in place of the ball a circular disk of some 10 to 20 centimetres diameter, having its front face either flat or slightly concave. To the right rod a point is fixed, and it is drawn back till from 6 to 15 centimetres distant from the disk. A piece of silk or satin of the same size as the front surface of the disk is laid upon it while the machine is in action, it adheres of itself to the surface, and the preparation is now complete. Before the silk is placed over the disk a small "brush" discharge of blueish light is all that can be distinguished at the point of the right-hand discharging-rod: but this now changes to a very faintly glowing star. At the same moment the central region of the silk-covered disk exhibits a peculiar glimmering light over a well-defined circle. The utmost care is needed to shut out all extraneous light from the room, otherwise the delicate appearances which follow cannot be seen. It is upon this circular patch of feeble light that the shadow-figures are thrown. Its pale gleam becomes more vivid when the machine is more energetically worked: it enlarges in area but diminishes in brightness as the point is drawn back from it, and contracts with an accompanying increase in brightness as the point is brought nearer. It is possible to obtain a similar glimmering surface also upon a large metal ball covered with silk and attached to the rod in place of the concave disk, or instead a screen made of two or three folds of silk stretched over an ebonite ring may be placed between the two discharging-rods, the ends of both being furnished with points. In each case it is important that the silk be without crease or wrinkle, otherwise an evenly illuminated disk of light will not be obtained.

If now a body of definite outline of form be interposed between the point and the disk, an electrical shadow of it will be cast upon the luminous circle. These shadows are truly electrical, not optical, for all bodies do not cast them, and, more curious still, different bodies though of the same shape may cast differently shaped shadows. Conductors of electricity cast well-defined shadows, and so do semi-conductors, such as wood and cardboard. True insulators of small dimensions cast no shadows. The insulation or non-insulation of the conducting bodies makes no difference in their shadow-giving power. A cross cut out of cardboard casts (as in Fig. 1) a well-defined shadow at the centre of the field, but the exterior portions are somewhat hazy. An ebonite cross casts no shadow. A cross made up of two strips, one of cardboard, the

FIG. 1.—Electric shadow cast by a cardboard cross.

other of ebonite, fastened with shellac, casts only a single bar of shadow. Rings of tinfoil, cardboard, or wire also cast shadows. Such small objects are conveniently held by attaching them to the end of glass rods. The size of the shadows increases if the objects are displaced from their central position to right or left. A strip of card or thin metal casts the same shadow whether it be held broadside or edgeways in the field. A wire grating having 5 millimetres width between the bars obscures the field like an opaque body. Breathing on a strip of ebonite or glass renders its surface a feeble conductor, and it casts a transient shadow. A glass rod heated at one point casts a shadow at the heated point, the shadow dying out as the

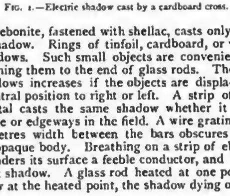


FIG. 2.— uminous figure projected through aperture in cardboard screen.

rod cools. No shadow is cast by a conductor whose surface is completely covered by insulating material, such as a shellac-covered wire or a glass tube containing water, but dry externally. The smoke ascending from a cigar casts moving shadows upon the silken screen. If a small ball be fixed upon the left discharging-rod in place of the point the shadow on the silk is poor, but a second shadow is observed upon the surface of the ball, and this is excessively small, reminding one of the diminished erect virtual optical image in a small polished ball. This one experiment succeeds best if the ball be made the positive con-

ductor. For the other experiments it makes little difference whether the electrification of the point be positive or negative, except that when the point is positively electrified the illuminated surface is a little larger than when negatively electrified. A larger disk of light can also be obtained by working the machine at a greater velocity, but at the same time the shadows are rather smaller.

Prof. Holtz has also obtained the inverse phenomenon of *luminous-figures* by two different means. If two pointed needles are fixed horizontally side by side upon the discharging rod opposite to the disk, there appears a vertical streak of light across the glimmering field. If the needles be replaced by a horizontal strip of metal with its edge directed toward the disk, a vertical bright streak is also produced. A short metallic tube affixed to the discharging rod produces on the disk a dark central spot surrounded by a nebulous bright ring. The second kind of *luminous-figures* is produced in the manner shown in Fig. 2, where a circular disk of metal or cardboard having a central aperture of recognisable form is interposed between the point and the screen. The result is a luminous image of the aperture which, though well-defined at the middle, is marred at the outer regions, the shadowed portions appearing to encroach more upon the illuminated parts as we pass to the peripheral region. The following experiment is curious:—If a square aperture be chosen, the luminous figure projected on to the silken surface shows well-defined corners; but if a small round disk of card be interposed between the discharging point and the square aperture the luminous square on the silken screen at once exhibits rounded corners.

Another interesting point connected with the shadow-figures is that they can be fixed in a temporary manner, like Lichtenberg's figures, by sifting upon the silken surface lycopodium-powder or other fine dust. This collects chiefly upon the contours of the figures, though under certain conditions the luminous and shadowed parts exhibit a contrasted density in the deposit of dust upon them. These dust-figures have an obvious relation with those obtained by Wiedemann from the discharge of Leyden jars through a pointed conductor against the surface of various bodies. It would be interesting to ascertain whether by this process also shadow-figures can be produced.

In explanation of these appearances Prof. Holtz propounds the view that they are due to a rectilinear discharge of electrified particles from the point of the discharging-rod, the discharge taking the general form of a cone, but in which the paths of the outermost particles diverge more widely as they approach the silken disk. The function of the silk he believes to be to retard the discharge, and thereby to increase the electric density on the point. The conducting bodies which are interposed in order to produce shadows act therefore by deflecting the flying particles from their path, either by absorbing or reflecting them. In many cases this action would appear to be a repulsion, since the shadows are always larger than the objects, and suffer more distortion by enlargement nearer the borders of the disk. To explain the production of the double shadows, the rather doubtful hypothesis is advanced that there is a rectilinear discharge of particles in both directions at once.

Quite independently of these observations, the same kind of phenomena have been investigated in America by Messrs. Fine and Magie of the Green School of Science, Princetown, N.J. These experimenters were aware of the existence of shadows on the positive discharging knob, but believed that they were the first to discover the existence of a negative shadow. They found however that non-conductors cast the best shadows, and added the interesting observation that the lines of electrical action were deflected by the presence of a conducting body at the side of the field, and the form of the shadow correspondingly altered. Negative shadows were also observed, they remark, some years ago by Prof. C. A. Young.

These shadow-figures become doubly interesting when compared with the "molecular-shadows" obtained by Crookes from electric discharges in high vacua Further experiments are probably needed before their precise nature is fully known.

S. P. T.

BEN NEVIS OBSERVATORY.

A PROPOSAL was made a year or two ago to erect an observatory on the top of Ben Nevis for meteorological observations, but nothing was done owing to the want of the necessary funds. A committee has been formed, however, within the last few months for the purpose of raising a testimonial to Mr. David Hutchison, who did so much in opening up the West Highlands to tourists with his steamboats, and we observe that it is proposed that the testimonial take the shape of an observatory on the top of Ben Nevis. The committee is a large and influential one, and the proposed scheme has every appearance of being successfully carried out.

In the meantime the Scottish Meteorological Society has commenced daily observations on Ben Nevis, which will be continued during the summer months—the Society having accepted a handsome offer by Mr. C. L. Wragge, who has had experience of such work, to climb to the top of Ben Nevis every morning in time to make observations there at 9 a.m. A complete set of the best instruments has been procured. The barometer (a *Fortin*), is an excellent instrument, and is constructed to read as low as 23·000 inches, in the procuring of which Mr. Scott of the Meteorological Office kindly gave his assistance. On Tuesday, May 31, Mr. Wragge, with Mr. Livingstone, of the Public Schools, and nine workmen ascended the mountain, and the instruments were fixed and secured in proper positions, and all, including the barometer, were found to be in good working order. The regular observations began on the following day, June 1, Mr. Wragge being at his post on the top of the Ben, 4406 feet above the sea, at 9 a.m. He remains an hour at the top, and makes three observations, viz. at 9, 9.30, and 10 a.m. Even during the stormy weather of Saturday last, the observations were made and the observer back to Fort William at 1.30 p.m., on which occasion the temperature at the top was as low as 28°·0.

Simultaneously with the Ben Nevis observations, a complete series of observations are also made near sea-level by Mrs. Wragge. These observations, together with the observations made at the neighbouring stations of Roy Bridge, Corran, Landale, Airds, Lismore, and Dalnaspidal (1450 feet above the sea), will give the data required in dealing with some of the more important problems in meteorology.

NOTES

AMONGST the few existing institutions for the higher education of women, perhaps none has done better work during the last thirty years in a quiet, unobtrusive fashion than Bedford College, York Place. The recent action of London University in opening its degrees to women has given a fresh impetus to women's education; and Bedford College has set itself the task of providing the training for which London University offers only the test—a task in which it has already achieved such success as to give a sure promise of a brilliant future. The funds, however, at its disposal, derived mainly from the bequest of the late Mrs. Reid, prove inadequate to the strain thus put on its resources, and an appeal for support is now being circulated which deserves the attention of the friends of education. In that appeal details of the work and aim of the College will be found. Space will only allow us to add that subscriptions will

be received by the hon. secretary, Bedford College, 8 and 9, York Place, W.

AMONG the houses of historical interest on which the Council of the Society of Arts have just erected memorial tablets, is No. 35, St. Martin's Street, Leicester Square, in which Sir Isaac Newton lived for some time.

M. LITTRÉ, the eminent philologist and philosophical writer, died on the 1st inst., at the age of eighty years. His celebrated French dictionary is probably the best dictionary ever published in any language; not only is its method thoroughly scientific, but it contains nearly every important scientific term in use in French scientific literature.

LAST night a complimentary dinner was given at Freemasons' Tavern to Dr. Danford Thomas, the new Coroner for Central Middlesex. Over a hundred representatives of the various professions were present. Dr. B. W. Richardson was in the chair.

THE *conversazioni* of the Society of Arts and the Civil Engineers, on Thursday and Friday last, were as usual successful; over 2000 people were present at each.

THE anniversary meeting of the Vienna Academy was held on May 30. R. Maly, Professor of Chemistry at Gratz, and F. Lippich, Professor of Physics at Prague, were elected correspondents.

THE Dundee Naturalists' Society have three dredging expeditions in their programme of excursions this summer—one on June 11 to St. Andrew's Bay, a second on July 20 to the Bell Rock, and a third on August 31 to Lunan Bay.

MR. EDISON has written to M. Georges Berger, asking for 120 horse-power to work the large generator he is sending to Paris for the forthcoming exhibition.

EXPERIMENTS are being made with a view to running trains through the St. Gothard Tunnel by electricity, with motive power obtained from the Reuss and the Tessin. The boring of the Arlberg tunnel proceeded last month at the rate of six and a half metres per day. The making of the lines of access will shortly be undertaken.

AT an examination held by the Sanitary Institute of Great Britain on June 2 and 3, six candidates presented themselves, and the Institute's certificate of competency as Local Surveyor was awarded to Samuel S. Grimley and to Arthur Whitcombe, and the Institute's certificate of competency as Inspector of Nuisances was awarded to John Latein Cowderoy, to Joseph Raine, and to William Wilkinson.

WE have received a copy of a paper by J. H. Collins, F.G.S., published in the *Journal* of the Royal Institution of Cornwall, No. xxiii., which contains analyses of the new minerals Henwoodite, Enysite, Duporthite, Penwithite, as well as of other minerals and rocks. Good analyses of minerals are always useful for reference; and Mr. Collins is rendering a service to science in publishing the results of his work. His paper would have been more valuable had he mentioned the methods employed for separation and determination of the mineral constituents, as, for instance, in the analyses of minerals containing alumina, iron, and phosphoric acid. It would likewise save trouble if the percentage composition of his assumed formulæ for minerals were placed in parallel columns with the quantities obtained. That they agree "fairly well" is a little vague.

THE earthquake shocks on Mount Vesuvius were followed on the 1st inst. by a strong eruption. Broad and active streams of lava ran quickly down the north-east side of the mountain.

A SHOCK of earthquake occurred at Serajewo (Bosnia) on June 2, at 4h. 40m. a.m., duration 2 sec.

ON the 1st inst. the Duke of Edinburgh placed the top stone on the new Eddystone Lighthouse. The stonework of the structure has now been completed after a labour of three years. The lantern has yet to be placed on the top of the structure, and the illuminating apparatus has to be fixed, and all the internal fittings are to be applied, but this is work that can be carried on at any time of the tide, so that the whole of the work will, it is expected, be completed in another year, twelve months within the period stipulated in the contract.

IN a report on the health of Swatow, just published by order of the Inspector-General of Chinese Maritime Customs, Dr. E. J. Scott refers to a plague of caterpillars which visited the neighbourhood last summer, literally covering the fir trees, on which they lived exclusively, and leaving them perfectly denuded of leaves. The hill sides in many places looked as if a fire had passed over the trees and scorched them. The Chinese were very much afraid to handle these caterpillars, as they declared that they were exceedingly poisonous, and Dr. Scott says they are to some extent right, as he knew of two foreigners who were injured by them. When crushed, they exuded a glutinous fluid of a light-green colour, very irritating to the skin, and producing rash, which caused much inconvenience for ten days or a fortnight.

DR. HAHN has recently published a work, in which he gives photographic reproductions of more than one hundred thin sections of meteorites on thirty-two plates. In that class of meteoric stones which bear the name of chondrites, on account of the curious round nuclei they contain, Dr. Hahn believes he can demonstrate the existence of a whole series of organic forms belonging to the animal world. A number of the sections shows a structure closely resembling corals. Dr. Weinland publishes an article on this subject in No. 16 of the *Ausland*, Dr. Hahn having placed the thin sections at his disposal; and he states that he gained the conviction that he was really beholding remains of animals belonging to the family of Favosites, well known among Silurian, Devonian, and Coal fossils. For further information we must refer our readers to the article mentioned.

AN epidemic among the crayfish of all the rivers and rivulets of the Stettin district has led to the complete destruction of that crustacean in those waters.

IN the north of France great devastation is caused by large field-mice (*Arvicola arvalis*) among the crops. The plague is particularly severe in the departments of Seine-et-Oise, Eure-et-Loire, Loiret, Seine-et-Marne, Yonne, Aube, Marne, and Haute Marne.

"LA BELLE JARDINIÈRE," one of the largest clothing establishments in Paris, is employing with success magneto-electric machines for the transmission of power, from the basement to the top of the building. The two machines have been built by Siemens Brothers.

AN opera performed on the stage of the Paris Grand Opera House was heard satisfactorily at the Rue Riche Opera House by a number of French officials a few days ago. The feat was performed with the new Ader telephone, of which the peculiarities have not been made public. The performance will be repeated at the International Exhibition.

M. MEREJKOFFSKY's report on his anthropological journey in the Crimea (*Izvestia*, 1881, fasc. 2) contains interesting measurements of skulls of the Crimean Tartars—the pure Steppe Tartars who do not present a mixture with Greeks, as the south-coast

Tartars do, nor with the Nogai, who are now, however, not numerous. After having measured the skulls of about 200 persons, he finds that the cephalic index is: 0·908 for children from 4 to 7 years old, 0·882 from 8 to 9 years, 0·876 from 10 to 12 years, 0·871 from 13 to 14 years, 0·852 from 15 to 19 years, and 0·845 from 20 to 23, being the average from 82 measurements. When discussing the figures received for 27 very pure representatives of Tartars, he shows that the maximum figures were, in four cases, from 0·886 to 0·903, and the minimum in two cases, 0·789 and 0·800; for the 21 others the index varies comparatively little, namely, from 0·822 to 0·876. M. Merejkoffsky has observed among the Tartar women the use of tattooing in small spots between the eyebrows and on the forehead. As to the staining the nails red, which is spread everywhere among the women, and sometimes also among men, M. Merejkoffsky, after having discussed the same custom among other peoples, arrives at the conclusion that it is a survival from the time when the whole skin was stained with red, originally with the blood of enemies, to inspire dread.

THE geysers of Whakarewarewa, New Zealand, are stated to have lately been in a state of agitation, throwing hot water to a great height. The natives anticipated still further eruptions. The geysers have been dormant for six months.

THE Annual Report for 1879-80 contained in the *Proceedings* of the Norwich Geological Society complains of the little interest taken in the work of the Society by the members; the bulk of the work seems to be done by the members of the Geological Survey stationed in the neighbourhood. The presidential address, by Mr. J. H. Blake, "On the Age and Relation of the so-called 'Forest-bed of the Norfolk and Suffolk Coast,'" is of considerable interest. It has been separately reprinted. Among the other papers is one on "The Subdivisions of the Chalk," by Mr. A. J. Jukes-Browne.

THE Twenty-Third Report of the East Kent Natural History Society contains some of the more important papers read during the year. We regret to see that the interest of some of the members in the welfare and work of the Society is not so great as it might be, and that, as in not a few similar societies, the bulk of the work falls on the shoulders of a few of the more energetic members.

WE are glad to find a decided improvement in No. 1, vol. x. of the *Canadian Naturalist*. The papers are mostly geological, a large proportion are original, or at least of purely Canadian origin, and all of scientific value. Principal Dawson contributes some important Palæontological Notes, and there is a long paper by Mr. R. Chalmers on the Glacial Phenomena of the Bay Chaleur Region, with a map. Dr. G. M. Dawson writes of the Geology of the Peace River Region. Appended are Meteorological Notes for 1880, and a curious statement as to the Niagara Falls having been dry for a day, March 31, 1848.

Naturæ Novitates, the fortnightly list of novelties in scientific literature, started some time ago by Friedlander of Berlin, continues, we are glad to see, to flourish. It is calculated to be of real service to workers in science.

MR. JOSCELINE BAGOT and Mr. Drummond, of the Grenadier Guards, accompanied by Mr. T. Wright, the winner of the International Balloon Contest, went up in a balloon from the Crystal Palace on the 1st inst. at 1 p.m. When the ropes were loosed they ascended to the height of 5000 feet, and travelled slowly in a south-westerly direction for the distance of about eight miles. The balloon then suddenly sank, but ballast being thrown out, it rose again to 8000 feet, and traversed in the direction of Epsom. The aëronauts then descended in a field about a quarter of a mile from the Grand Stand, which they reached in time to witness the race for the Derby.

SIR R. TEMPLE'S lecture on the lake region of Sikkim is given in the current issue of the Geographical Society's *Proceedings*, illustrated by a capital map and some very good engravings from the author's sketches on the spot. The other papers are a translation from the Russian by Mr. Delmar Morgan of Dr. Regel's account of his expedition from Kuldja to Turfan in 1879-80, and Mr. F. C. Selous' notes on recent explorations in Mashuna-land, the latter of which adds something to our knowledge of the hydrography of the Zambesi basin, and is accompanied by a map in the text showing the routes of Mr. Selous and others. The geographical notes relate chiefly to the work of various expeditions on the Congo and other parts of West Africa. There are also notes of some interest on the true name of the Chukches and on Richmond Gulf, Hudson's Bay. Mr. James Stevenson contributes a memorandum of the longitude of Lake Nyassa, which is followed by a full abstract of the proceedings of the Paris and Berlin Geographical Societies.

THE paper by General Pitt-Rivers announced last week will be read at the Anthropological Institute on the 14th, not 7th inst.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. W. Nugent; a Vulpine Phalanger (*Phalangista vulpina* ♂) from Australia, presented by Master H. Berridge; ten Green Lizards (*Lacerta viridis*), European, presented by Mr. H. N. Moseley, F.Z.S.; two Ostriches (*Struthio camelus* ♂ ♀) from Africa, two White-backed Piping Crows (*Gymnorhina leuconota*), a Laughing Kingfisher (*Dacelo gigantea*) from South Australia, deposited; a Prince Albert's Curassow (*Crax alberti* ♀) from Columbia, two Golden Agoutis (*Dasyprocta aguti*) from Guiana, two Common Boas (*Boa constrictor*) from South America, on approval; a Japanese Deer (*Cervus sika* ♂), a Cuming's Octodon (*Octodon cumingi*), born in the Gardens; five Impeyan Pheasants (*Lophophorus impeyanus*), four Peacock Pheasants (*Polyplectron chinquis*), a Ruddy Sheldrake (*Tadorna rutila*), bred in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In a Convocation holden on June 7 a decree was passed, without opposition, to amend the statute relating to the Degree in Medicine. According to the new decree, those who enter their names for the First Examination for the Degree of Bachelor of Medicine, and who satisfy the Regius Professor of Medicine that they have obtained Honours in the School of Natural Science, or that they have passed the Preliminary Honour Examination in the same School, shall not be required to be examined either in Chemistry or in Mechanics and Physics at the First Examination for the Degree of Bachelor of Medicine.

In the same Convocation the statute to enable non-members of the University to pass an examination in lieu of Responsions was also passed without opposition. The first examination in lieu of Responsions will be held towards the end of the ensuing long vacation.

Mr. George B. Ferguson, M.D., Hertford College, has been nominated Examiner in the Natural Science School in place of Prof. Lankester, and Mr. John Watts, M.A., Balliol College, in place of Dr. Odling.

Prof. Sayce announces a public lecture in the Taylorian Institution, June 8, on the ancient Hebrew inscription recently discovered in Jerusalem.

An examination will be held in Exeter College early in October for the purpose of filling up a Natural Science Scholarship, tenable for four years during residence. The age of the Candidates is not limited, but they will be expected to give evidence of sufficient Classical knowledge to be able to pass Responsions. The examination will be in Biology, Chemistry, and Physics. Candidates will be expected to show proficiency in at least two of these subjects, and the Scholar will be

required to read for Honours in Biology in the Natural Science School. The examination will be to a large extent practical, but special weight will be given to comprehension of general principles. Candidates for the Scholarship may obtain further information by application to the Rector, or to W. L. Morgan, Esq., the Lecturer in Biology at Exeter College.

CAMBRIDGE.—Prof. Humphry has given notice of a class in human osteology, to be held by the demonstrator (Dr. Creighton) during the long vacation, commencing July 4; and a class for practical histology (by Mr. Hill), beginning July 7.

Mr. J. W. Clark is continued in the office of Superintendent of the Museums of Comparative Anatomy and Zoology for two years.

The offer of the Cambridge Philosophical Society to make their library available as the nucleus of a general scientific library in the new museums, to be placed under the care of a librarian appointed by the University, has been formally accepted by the Senate.

Inter-collegiate lectures in higher mathematical subjects are offered for the ensuing long vacation by Mr. Allen at St. Peter's, on electro-magnetism; by Mr. Mollison (Clare), on heat; by Mr. Stearn (King's), on hydrodynamics; by Mr. Niven (Trinity), on elasticity; and by Mr. Lewis (Trinity), on vortex motion and viscosity.

NOTTINGHAM.—The following appointments have been made to the professoriate of Nottingham University College:—The Rev. J. F. Blake, M.A., F.G.S., Professor of Natural Science; Dr. J. A. Fleming, B.A., D.Sc. (London), Professor of Mathematics; Dr. Frank Clowes, D.Sc., F.I.C., F.C.S. (London and Berlin), Professor of Chemistry; the Rev. J. E. Symes (Cambridge), Professor of Literature.

AN important memorial to Lord Spencer, Mr. Mundella, and the Committee of Council on Education is now in course of signature, urging the more systematic teaching of science in elementary schools. The suggestions are made in prospect of the fundamental changes which are contemplated in the Code. The memorialists urge that in Standards I., II., and III. systematic Object Lessons should be given which should lead up to the more scientific teaching to be required in the higher standards. These Object Lessons should have reference to three main divisions of knowledge. They should include, first, Shape and Size, and the properties of bodies depending on them; second, Properties of Matter, including a knowledge of the obvious qualities of material and implements; and third, Plants and Animals, with a knowledge of their uses. At present the elementary stages of teaching have to do too little with things and too much with words, and the memorialists suggest that the existing standards favour this. The memorial has already received the signatures of Prof. Max Müller, Dr. Caldicott, Mr. Eve, Prof. Meiklejohn, Prof. Carey Foster, and leading members of many School Boards.

AT the Education Society on June 6, with Dr. Gladstone in the chair, a discussion was held "On Science Teaching in Intermediate Schools." After the opening remarks of the President, Miss Franks, Mr. Lake, Prof. Guthrie, Prof. G. Carey Foster, Mr. Cooke, and other gentlemen, gave the results of their experience or expressed their opinions. The main conclusions were: that natural knowledge should be taught, not from books, but from things themselves; that the lessons should not consist of information committed to memory, but of knowledge acquired by the child's own observation and experience; that by such object-lessons he should be led to observe the natural facts or processes around him and to exercise his powers of comparison as well as of perception, and thus arrive at such generalisations as are within his capacity; that after the first more general knowledge of the common things around him the child should be led along the broad lines of interest to some more special departments of science. In this later stage the reasoning powers of the child will be more called into action, and the knowledge of the teacher will be presented in a more systematised and abstract form, but still in such a way as shall best develop the intelligence of the scholar.

THE promoters of the Stephenson Centenary at Newcastle-on-Tyne have determined to commemorate the occasion by erecting, if funds can be obtained, a building in that town for the use of the College of Physical Science, to be called the Stephenson College. It is estimated that a sum of 20,000*l.* will be required, of which 1000*l.* has been promised by Sir William Armstrong, and 2000*l.* by other friends. The Newcastle College of Physical Science was established and endowed ten years ago by the combined efforts of the inhabitants of the town and the University of Durham.

SCIENTIFIC SERIALS

American Journal of Science, May.—Action of frost on the arrangement of superficial earthy material, by W. C. Kerr.—Dall's observations on Arctic ice and the bearing of the facts on glacial phenomena in Minnesota, by N. H. Winchell.—Projection of lines of equal pressure in the United States, west of the Mississippi, by H. A. Hagen.—Neumann's method of calibrating thermometers, with ways of getting columns for calibration, by T. Russell.—William Hallowes Miller, by J. P. Cooke.—Geology of Peace River region, by G. M. Dawson.—Shadows obtained during the glow-discharge, by H. B. Fine and W. F. Magie.—New form of galvanometer for powerful currents, by C. F. Brackett.—American Jurassic dinosaurs, by O. C. Marsh.

THE *American Naturalist* for May, 1881, contains: George Macloskie, the endo-cranium and maxillary suspensorium of the bee.—R. E. C. Stearns, *Mya arenaria* in San Francisco Bay.—H. L. Osborn, the squid of the Newfoundland Banks in its relation to the American Grand Bank Cod Fishery.—A. S. Packard, jun., the brain of the embryo and young locust.

Journal of the Franklin Institute, May.—On the ratio of expansion at maximum efficiency, by R. H. Thurston.—The Wootton locomotive engine, by J. S. Bell.—The efficiency of the *Anthracite's* engines, by C. R. Roelker.—Experiments in Mulhouse on a Corliss steam-engine, described by Chief-Engineer Isherwood.—Repairing a broken crank with wire-rope, by J. C. Kafer.—Concentration of low grade ores.

Annalen der Physik und Chemie, No. 5.—On transpiration of vapours (2nd part) by L. Meyer and O. Schumann.—On the specific heat of chlorine, bromine, and iodine gas, by K. Strecker.—On volume changes of some metals in fusing, by F. Nies and A. Winkelmann.—Thermochemical researches, by C. v. Than.—On the supposed heating of ice, by A. Wüllner.—On the double refraction of light in frictional liquids in motion, by A. Kundt.—New modification of light by reflection on narrow metallic gratings, by J. Fröhlich.—An apparatus for observation of Newton's rings, by L. Sohncke.—Magnetic researches, by E. Warburg.—On the variability of the capacity of condensers with a rigid insulator, by H. Herwig.—Derivation of the electrodynamic laws of induction, by N. Umow.—On the motion of an electric particle in a homogeneous magnetic field and the negative electrical glow, by E. Riecke.—Measurement of the force of terrestrial magnetism on a linear conductor capable of rotation, by the same.—An acoustical apparatus for lecture purposes, by H. Maschke.—Whether electricity, in changing insulating-plates, penetrates into their mass? by W. Holtz.—An old diving-bell, by G. Budde.

Journal de Physique, May.—Photometric studies, by A. Cornu.—Indices of refraction of water in surfusion, by B. C. Damien.—Optical properties of a plate of metal polarised by an electrical current, by G. Lippmann.—On the passivity of iron, by E. Bibart.—Mechanical inscription of Lissajous' figures, by A. Crova.

Archives des Sciences Physiques et Naturelles, No. 5, May 15.—Review of Marsh's work on the Odontornithes, by A. Humbert.—The chemical composition of albuminoid substances (continued), by Dr. Danilewsky.—*Compte rendu* of meetings of the Geneva Chemical Society, by M. Amé Pictet.

Rivista Scientifico-Industriale, No. 8, April 30.—The Gardini battery.—Velocity of sound in chlorine (continued), by S. Martini.—Geological note on the region of S. Vito (Marni), by G. Terrenzi.

Atti della R. Accademia dei Lincei, vol. v. fasc. 11.—On the functions of the urinary bladder, by A. Mosso.—On the physiological action of apoatropine, by the same.—Crystallographic study of two chloroplatinates of Dr. Ciamician, by L. Valle.—Observations on the horizontal diameter of the sun in 1880, by L. Respighi.—New modifications of the process for extraction of arsenic, by F. Selmi.—New researches on the pathological base, and a saccharifying ferment of the urine of a scorbutic patient, by the same.

Sitzungsberichte der physikalisch-medicinischen Societät zu Erlangen, November, 1879, to August, 1880.—On general theta-functions, by M. Noether.—Preparations of human ear-bones for lecture purposes, by L. Gerlach.—On the occurrence of two ampullæ in the outer (horizontal) arch of the bony labyrinth, by the same.—On the excretion of hippuric and benzoic acid during fever, by T. Weyl and B. v. Anrep.—On section-systems of algebraic curves, by J. Bacharach.—On the work-product of muscles, by J. Rosenthal.—On dichroïtic fluorescence on platino-cyanides of magnesium; experimental proof of the perpendicularity of the light-vibrations to the plane of polarisation, by E. Lommel.—On the phenomena, in polarised light, of a plate of platinocyanide of magnesium cut at right angles to the optic axis, by the same.—On an artificial lung-cavity preparation, by F. Penzoldt.—On gluten, by T. Weyl and H. Bischoff.—On carbonic oxide hæmoglobin, by T. Weyl and B. v. Anrep.—On vagus-stimulation, by J. Rosenthal.—On unipolar nerve-stimulation and false nerve-stimulation by derived currents, by the same.—On fluorescence, by E. Lommel.—On the invariant representation of algebraic functions, by M. Noether.—On the parasitism of *Elaphomyces granulatus*, by M. Reess.—On poisoning with morels, by E. Bostroem.—On adventive formations, by A. Hansen.—Does the ground-air contain ammonia? by L. Rinck.—On oxygen determination, by F. Zeitler.—Contributions to pathological anatomy, by E. Bostroem.

SOCIETIES AND ACADEMIES

London

Geological Society, May 25.—R. Etheridge, F.R.S., president, in the chair.—Rev. Tom Bullock Hardern, M.A., LL.M., was elected a Fellow of the Society.—The following communications were read:—On the discovery of some remains of plants at the base of the Denbighshire grits, near Corwen, North Wales, by Henry Hicks, M.D., F.G.S.; with an appendix by R. Etheridge, F.R.S., Pres. Geol. Soc. Traces of these fossils were first observed in 1875 by the author in Pen-y-glog quarry, about two miles east of Corwen. Further research has resulted in the discovery of more satisfactory specimens, which have been examined by Messrs. Carruthers, Etheridge, and E. T. Newton. Among them are spherical bodies resembling the *Pachytheca* of Sir J. D. Hooker, from the bone-bed of the Ludlow series, supposed to be Lycopodiaceous spore-cases; also numerous minute bodies stated by Mr. Carruthers to be united in threes, and to agree with the forms of the microspores of Lycopodiaceæ, both recent and fossil; and some fragments, which may belong to these plants, and others, probably belonging to plants described by Dr. Dawson from the Devonian of Canada under the name of *Psilophyton*. The above testify to the existence of a very rich land-flora at the time. Mixed up with these however are numerous carbonaceous fragments of a plant described also by Dr. Dawson from the Devonian of Canada, which he referred to the Coniferæ, but which is, according to Mr. Carruthers, an anomalous form of Alga. The former called it *Prototaxites*; the latter renamed it *Nematophycus*. Numerous microscopical sections, showing the beautiful structure of this interesting plant from the specimens found at Pen-y-glog, have been examined by Mr. Etheridge and Mr. Newton, and their conclusions agree with those of Mr. Carruthers. The evidence seems to show that at this mid-Silurian period the immediate area where the plants are now discovered must have been under water, and that the mixture of marine and dry-land plants took place in consequence of floods on rapid marine denudation. The author indicated that the land-areas must have been to the south and west, chiefly islands, surrounded by a moderately deep sea, in which Graptolites occurred in abundance. The position of these beds may be stated to be about 2000 feet below the true Wenlock series, and about the horizon of the Upper Llandovery rocks.—Notes on a mammalian jaw from the Purbeck beds at Swanage, Dorset, by Edgar Willett. Communicated by the President.

Physical Society, May 28.—Prof. Fuller, vice-president, in the chair.—Mr. C. Woodward exhibited apparatus for illustrating wave-motions to a class. This consisted of a number of glass panes of equal size mounted on stands so that they could be ranged in a line or in rank and file. Patches of blue paper were attached to them to represent the moving particle of the wave, the positions being determined by a diagrammatic card which fitted each pane. A machine for showing Fresnel's conception of polarised light consisted of two axles fitted with a number of cranks which supported a roof of rafters bearing at their ridges a number of beads to guide the eye in tracing the wave-motion. By turning the axles the cranks shifted the frame of rafters, and the beads displayed the wave-motion, which was vertical, elliptical, or circular according to the adjustment.—Prof. G. Forbes explained the experiments made by him and Dr. Young to determine the velocity of light. The method employed was that of Fizeau, but instead of having one distant reflector and observing the total eclipse of the reflected ray by a tooth of the revolving wheel, two reflectors, one a quarter of a mile behind the other, were used, and two rays, which were observed when of equal brightness. This method was found more accurate than Fizeau's own plan, and gave curves of brightness. The speeds of the toothed wheel were adjusted until the two rays appeared of equal brightness. The general result was that the velocity of the light of an electric lamp is 187,200 miles per second. Cornu found the light of a petroleum lamp to be 186,700 miles per second, and Michaelson that of the sun to be 186,500 miles per second. The higher number of Prof. Forbes is probably due to the bluer light of electricity, for further experiments made with coloured lights and the spectrum seemed to prove that blue light travels probably over 1 per cent. faster than red light. The experiments were made at Wemyss Bay, in Scotland. Mr. Spottiswoode, P.R.S., said he had followed Prof. Forbes with interest, and these results appeared to modify our ideas of the luminiferous ether. Lord Rayleigh inquired why it was that Jupiter's satellites showed no difference in tint in emerging from eclipse if red and blue rays travelled with unequal velocities? Prof. Forbes believed it due to the gradual character of the emergence of the satellites from behind their primary. According to the new theory variable stars should however seem bluish with an increase of their light. Prof. G. C. Foster pointed out that dispersion of the light in the air would rather have had the effect of retarding the blue rays. Mr. Hall of Baltimore, U.S., then exhibited the experiment in which a current of electricity flowing longitudinally along a thin foil of metal is caused to yield a transverse or lateral current by inserting the foil between the poles of a magnet. The lateral current is observed on a sensitive galvanometer, and care is taken in the first place to find points of connection with the foil, which yield no current before the magnet is applied. The results were that if iron is called + the series is iron +, silver −, gold −, platinum −, tin −. Curiously, nickel, though a magnetic metal like iron, is −; but on inquiry by Prof. Chandler Roberts it proved that the nickel employed was perhaps impure. Cobalt ranges between iron and silver, and is +, like iron. Prof. Perry suggested that the displacement and huddling of the lines of flow of the current by the magnet might cause the current; but Mr. Hall said that an experiment had been tried to test that, and went to prove that it was not due to crowding of these lines.—The Secretary read a paper by Prof. J. H. Poynting on the change of bodies from the solid to the liquid states. There are two types of change exemplified by ice-water and by sealing-wax: in the one a surface melting at the same temperature, in the other a softening of mass and heating. The first was thought by Prof. Forbes to be a limiting case of the second type, but the author gives reasons for supposing that it is rather an exchange phenomenon analogous to what takes place when water evaporates, and the melting-point is reached when the number of molecules passing from the ice to the water is equal to the number passing from the water to the ice. The sealing-wax type is analogous to the change of state in a liquid-gas above its critical point, where it changes gradually from a rather liquid to a certainly gaseous state.

Anthropological Institute, May 10.—Major-General A. Pitt-Rivers, F.R.S., president, in the chair.—Mr. Hyde Clarke exhibited a collection of stone implements collected by Mr. Papadopoulo Keramenes of Smyrna.—Lieut.-Col. R. G. Woodthorpe, R.E., read a paper on the wild tribes of the Naga Hills.—Prof. G. Dancer Thane read a paper on some Naga skulls.

May 24.—Major-General A. Pitt-Rivers, F.R.S., president, in the chair.—Mr. E. H. Man read a paper on the arts of the Andamanese and Nicobarese. After exhibiting and describing the new objects from the Andaman and Nicobar Islands, comprised in the second collection recently presented by him to General Pitt-Rivers, the author gave a slight sketch of the aborigines of the former group; he stated that they are divided into at least nine tribes, linguistically distinguished, and in most, if not all, of these there are two distinct sections, viz. inland and coast men. In confirmation of this

statement Mr. Man read a translation he had made of an account obtained in 1876 from a member of the inland branch of the Awko jûwai tribe, inhabiting a portion of the Middle Andaman, regarding their habits and mode of life, the details of which had since been fully corroborated. In many mental characteristics affinity to the Papuans would appear to exist, and the standard in social and marital relations is shown to be far higher than could be expected from a race so entirely outside the pale of civilisation; the previous accounts of their laxity in this respect are now proved to be erroneous. They have no forms of religion or ideas of worship, and though they have faith in a Supreme Being, the Creator, their belief in the Powers of Evil is much more strongly developed. The habitations of the eight tribes of Great Andaman are of three varieties, partaking almost invariably of the nature of a simple lean-to, while those of the remaining tribe, Jârawa (da), are somewhat similar in form to the huts erected by the Nicobarese. The rights of private property are recognised and respected; there also appears to be a fair division of labour and perfect equality between the sexes in their social intercourse.—Dr. Allen Thomson, F.R.S., read a paper on some bone necklaces from the Andaman Islands. Several of the specimens exhibited by the author were constructed entirely of human bones, while some were composed of bones of various animals, and others were partly made up of pieces of coral.—Mr. J. Park Harrison, M.A., exhibited an incised slate tablet and other objects from Towyn. The figures upon the slate appeared to represent celts, urns, &c.

Photographic Society, May 10.—J. Glaisher, F.R.S., president, in the chair.—Mr. Leon Warnerke read a paper on a new discovery regarding gelatine emulsion. This consisted in the observed fact that when gelatine emulsion has been submitted to the combined action of light and pyrogallic acid, it becomes insoluble in warm water; a gelatine negative is transferred to glass or paper, and from the back, with warm water, all parts not acted upon by light and the developer can be washed away; consequently a solvent of the silver not acted upon, such as hypo-sulphite, becomes unnecessary, and the remaining film or picture is left intact, and from its purity can be reacted upon in many ways hitherto extremely difficult or impossible. This discovery also becomes valuable in its application to the Woodbury printing process, phototype printing, and burnt-in photography on ceramic ware.

Institution of Civil Engineers, May 31.—Mr. Abernethy, F.R.S.E., president, in the chair.—The paper read was on "The Production of Paraffin and Paraffin Oils," by Mr. R. Henry Brunton, M. Inst. C. E.

Paris

Academy of Sciences, May 30.—M. Wurtz in the chair.—The following papers were read:—Memoir on the temperature of the air at the surface of the ground and down to 36 m. depth, also the temperature of two pieces of ground, the one bare, the other covered with grass, during 1880, and on the penetration of frost into these, by MM. Becquerel. The effects of the severe cold receive special attention. The screening influence of snow is shown. *Inter alia*, the propagation of frost is slower in grassy ground than in bare ground. In the latter the rate increases very slightly with the depth, the propagation being very regular. In grassy ground the increase is very notable, and with increasing depth, the rate tends to come near that in bare ground. Each layer of ground is subject to two calorific effects: one due to variations of external temperature; the other to the action of deep layers which tend to give a constant temperature.—On rabies, by M. Pasteur, with MM. Chamberland, Roux, and Thuillier. The seat of the virus is not in the saliva alone; the brain contains it, and the authors have successfully inoculated with brain substance. They have also succeeded in shortening the time of incubation, inoculating directly the brain of a dog with cerebral matter from a mad dog (and having recourse to trepanation).—Nebulæ discovered and observed at Marseilles Observatory, by M. Stephan.—On the theory of motion of celestial bodies, by M. Gyldén.—On a new means of accelerating the service of canal locks, by M. De Caligny.—On the genera *Williamsonia* Carruth. and *Goniolina* D'Orb (continued), by MM. de Saporta and Merion.—Observation and elements of the comet *a* 1881 (L. Swift), by M. Bigourdan.—On Fuchsian functions, by M. Poincaré.—Algebraic relations between the superior sines of a given order, by M. Rouyaux.—On the sines of superior orders, by M. West.—On the discontinuous phosphorescent spectra observed in almost perfect vacuum, by Mr. Crookes. M. Edm. Becquerel recalled his own spectroscopic studies of the light of phosphorescent substances and his excitation of such substances by submitting them to the discharge in vacuum tubes (in which case the rise of temperature and the electric light itself complicated the effects).—New interrupter for induction-coils, by M. Deprez. A claim of priority (to M. Ducretet) on the conical mirror; reply to a communication of M. Pifre, by M. Mouchot.—Discussion of the theory of three fundamental colour sensations; distinctive character of these colours, by M. Rosenstiehl. Certain properties attributed to primary colours do not belong to them exclusively, *e.g.* their producing all perceptible colours when mixed two and two, and the sensation of white arising from the three fundamental sensations being excited equally. The fundamental property of the primary triad is stated to be that colours situated on either side of a primary colour (in the graphic triangle) and equidistant to sight have their complementaries so near together that it becomes difficult to distinguish those which are consecutive.—On the oil of wild thyme, by M. Febve.—On geological microzymas; reply to MM. Chamberland and Roux, by M. Béchamp.—On a vanadate of lead and copper of Laurium, by M. Pisani.—On the existence of the Cambrian formation at Saint Léon and Châtelperron (Allier), by M. Julien.—On the coal-formation of Commentry; experiments made with a view to explain its formation, by M. Fayol. He reproduced the conditions and effects on a small scale by means of basins with a constant level of liquid, receiving currents of water with pebbles, sand, clay, coal, plants (previously immersed some time, so as to sink in quiet water), &c.—Movements of the frog consequent on electric excitation, by M. Richet. Frogs (intact) show great resistance to electric stimuli. (Two Thomson elements were used, with a coil.) The response to a single stimulation of the leg or sciatic nerve was generally more than 0·15 seconds after; the delay was oftener half a second, sometimes as much as ten seconds. With repeated excitations the reaction is sometimes extremely slow. In general the response is more rapid the stronger the excitation. Fatigue comes quickly. Excitations of the sensibility stop voluntary movements. The general movements of flight or defence in intact frogs, on electric excitation, seem to be determined by the bulb. Are they (M. Richet asks) reflex or voluntary?—On symmetrical vaso-motor actions, by MM. Teissier and Kaufmann. Under certain conditions the reverse of the law established by Brown-Séquard and Tholozan holds good; a capillary dilatation on the left side, *e.g.* will produce a vascular constriction on the right side, or *vice versâ*.

Vienna

Imperial Academy of Sciences, June 2.—M. Burg in the chair.—E. Hornstein, contribution to a knowledge of the system of asteroids.—Prof. S. Stricker, on the law of convulsive action.—Dr. Ludwig Langer, on the chemical composition of human fat at different ages.—Prof. E. Zuckerkandl, on the communications of the venæ pulmonales with the bronchial veins and the veins of mediastinum.—Prof. W. Loebisch and Dr. A. Loos, on glycerin-xanthogenates.—Dr. P. Weselsky and Dr. R. Benedikt, on hydroquinonic and orcinic ethers.—Dr. L. Szajnocha, contribution to a knowledge of Jurassic Brachiopoda of the Carpathian rocks.—Prof. T. Finger, on an analogon to Kather's pendulum and its use for measuring gravitation.—Dr. S. Ehrmann, on the determinations of nerves in the pigment-cells of frogs' skin.

CONTENTS

THURSDAY, JUNE 16, 1881

THE STORAGE OF ELECTRIC ENERGY

I AM continuing my experiments on the Faure accumulator with every-day increasing interest. I find M. Reynier's statement, that a Faure accumulator, weighing 75 kilograms (165 lbs.) can store and give out again energy to the extent of an hour's work of one-horse power (2,000,000 foot-pounds) amply confirmed. I have not yet succeeded in making the complete measurements necessary to say exactly what proportion of the energy used in the charging is lost in the process of charging and discharging. If the processes are pushed on too fast there is necessarily a great loss of energy, just as there is in driving a small steam-engine so fast that energy is wasted by "wire-drawing" of the steam through the steam pipes and ports. If the processes are carried on too slowly there is inevitably some loss through local action, the spongy lead becoming oxidised, and the peroxide losing some of its oxygen viciously, that is to say, without doing the proper proportion of electric work in the circuit. I have seen enough however to make me feel very confident that in any mode of working the accumulator not uselessly slow, the loss from local action will be very small. I think it most probable that at rates of working which would be perfectly convenient for the ordinary use of fixed accumulators in connection with electric lighting and electric transmission of power for driving machinery, large and small, the loss of energy in charging the accumulator and taking out the charge again for use will be less than 10 per cent. of the whole that is spent in charging the accumulator: but to realise such dynamical economy as this prime cost in lead must not be stinted. I have quite ascertained that accumulators amounting in weight to three-quarters of a ton will suffice to work for six hours from one charge, doing work during the six hours at the uniform rate of one-horse-power, and with very high economy. I think it probable that the economy will be so high that as much as 90 per cent. of the energy spent in the charge will be given out in the circuit external to the accumulator. When, as in the proposed application to driving tramcars, economy of weight is very important, much less perfect economy of energy must be looked for. Thus, though an eighth of a ton of accumulators would work very economically for six hours at one-sixth of a horse-power, it would work much less economically for one hour at one horse-power; but not so uneconomically as to be practically fatal to the proposed use. It seems indeed very probable that a tramcar arranged to take in, say 7½ cwt. of freshly-charged accumulators, on leaving head-quarters for an hour's run, may be driven more economically by the electric energy operating through a dynamo-electric machine than by horses. The question of economy between accumulators carried in the tramcar, as in M. Faure's proposal, and electricity transmitted by an insulated conductor, as in the electric railway at present being tried at Berlin by the Messrs. Siemens, is one that can only be practically settled by experience. In circumstances in which the insulated conductor can be laid, Messrs. Siemens' plan will undoubtedly be the most economical, as it will save the carriage of the weight of the accumulators. But there are many cases in which the insulated conductor is impracticable, and in which M. Faure's plan may prove useful. Whether it be the electric railway or the lead-driven tramcar, there is one feature of peculiar scientific interest belonging to electro-dynamic propulsion of road carriages. Whatever work is done by gravity on the carriage going down hill will be laid up in store ready to assist afterwards in drawing the carriage up the hill, provided electric accumulators be used, whether at a fixed driving station or in the carriage itself.

WILLIAM THOMSON

University, Glasgow, June 13

THE LIFE OF WHEWELL

The Life, and Selections from the Correspondence, of William Whewell, D.D., late Master of Trinity College, Cambridge. By Mrs. Stair Douglas. (C. Kegan Paul and Co., 1881.)

IT is now about four years since the first instalment of a biography of the late Dr. Whewell was published. These volumes, admirably edited by Mr. Todhunter, give us a brief outline of his history, but consist chiefly of a most valuable analytical account of his writings and a selection from his literary and scientific correspondence. In the preface a more complete memoir of Dr. Whewell's personal and domestic history is announced as in preparation. The present volume, edited by Dr. Whewell's niece, Mrs. Stair Douglas, fulfils the promise then given. The preface explains the long interval—fourteen years—which has elapsed since Dr. Whewell's death. A series of untoward events have continued to retard publication. From various causes much delay occurred before the exact plan of the work was determined and the subjects apportioned. At first it was hoped that what may be called the academic life of Dr. Whewell would be undertaken by Mr. Aldis Wright, the present Bursar of Trinity College. But the pressure of heavy and unavoidable engagements has precluded him from proceeding with the task. Mrs. Douglas then endeavoured to work the materials into the selections from Dr. Whewell's personal correspondence which she had nearly completed, with the assistance of Mr. J. Lemprière Hammond, Fellow of Trinity, and one of Dr. Whewell's executors. Before this was accomplished she was deprived of his invaluable aid by his lamented and untimely death. Thus some portions of the present work are a little incomplete. Still, as these are generally of a rather technical nature, and more interesting to members of the University than to the general reader, their absence probably will not be very widely felt. We may be allowed to express our admiration at the tact and good taste with which Mrs. Douglas has executed her task. She allows Dr. Whewell as far as possible to speak for himself, connecting his letters generally with only such brief biographical paragraphs as are necessary for a connected and intelligible narrative. There is little comment and no attempt at the fulsome praise with which biographies are often disfigured. Her descriptions, though brief, are often graphic, while the letters enable us to see the Master of Trinity as he appeared to the inner circle of intimate friends and loved relations.

Of his vast and varied knowledge it is almost needless

now to speak. Suffice it to say that the letters now published contain additional testimony to the truth of Mr. Todhunter's remark in his preface to the volumes mentioned above: "I do not think adequate justice can be rendered to Dr. Whewell's vast knowledge and power by any person who did not know him intimately, except by the examination of his extensive correspondence; such an examination cannot fail to raise the opinion formed of him by the study of his published works, however high that opinion may be."

The letters, however, in the present volume, as might be expected, bring out their author in a light which is to many new and unexpected. To most persons that broad forehead with its massive brow seemed indicative of intellectual strength almost gigantic; the square shoulders, strong bones and muscles, the swinging gait—with which as he swept along he seemed to shoulder aside weaker men by the very waft of his passing—told of irresistible force of will and energy of purpose; tenderness of heart seemed improbable in one of such Titanic mould; one deemed him a "man of iron," who, had he chosen a field other than literature and science, might also have been one of blood; but, as we shall presently see, underneath that rough exterior there was a warm and affectionate heart concealed.

Of the childhood of William Whewell but few particulars are recorded. A master-carpenter's son at Lancaster, he was on the point of quitting the Blue Coat School in that town to be apprenticed to his father, when —by a mere chance as it seemed—the head-master of the Grammar School entered into conversation with the boy, and was so struck by his abilities that he persuaded the father to let his son come to that school, generously offering to give him both books and instruction. According to Prof. Owen—probably his sole surviving schoolfellow —the lad's indomitable spirit soon manifested itself, as well as his appetite for work. The latter indirectly raised the standard of the school lessons, and the other boys threatened to "wallop" him as the penalty for preparing more than twenty lines of Virgil. Even then however this was more easily said than done, and the "wallopers" got as good as they gave, until public opinion in the school decided that it was unfair for more than two boys to attack him at once—"after the fate of the first pair, a second was not found willing." Once only did Whewell shock the moral feelings of his revered master, and that was when an undergraduate at Trinity. The crime shall be told in the master's own words: "He has gone and got the Chancellor's gold medal for some trumpery poem, 'Boadicea,' or something of that kind, when he ought to have been sticking to his mathematics. I give him up now. Taking after his poor mother, I suppose." (She had occasionally contributed to the "Poet's Corner" of the local newspaper.) Mrs. Owen, to whom this complaint was made, pacified the worthy man by remarking that "young men must have some amusement, and this seemed to be a very innocent one."

Notwithstanding Dr. Whewell's strong frame he appears to have suffered from some constitutional delicacy when a lad. His mother—evidently a woman of ability and culture above her station—died when he was thirteen; his father only survived to see him take a degree. A sister also died young; and of his three brothers two died in infancy and the other at the age of nine. Talent was evidently hereditary in the family, for the little brother, at seven years old, had begun to write English verses, and one of his sisters habitually wrote poems. Of all three of the latter, he says, when referring to his prize poem, in a letter to his father, "I am happy in having sisters who all of them have, I think, a more rational taste for poetry and literature of all kinds than any other girls in the same circumstances."

It is very interesting to note the gradual change in these letters. We see in them not only the unfolding of his great intellectual powers as evidenced by the widening circle of interests, but also the gradual expanding of the moral nature. At first those addressed to his relations and more intimate friends are a little stiff and cold, but as sorrows succeed one another the religious element in Dr. Whewell's character becomes more conspicuous, and the later letters are marked by a depth of tenderness surprising to those who only knew him slightly. He was devotedly attached to his first wife, and almost heartbroken by her loss. In one letter he describes himself as taking no pleasure in success now that she was gone, and tells his niece how, while he sat as Vice-Chancellor in the Senate-house conferring degrees, he felt so lonely and miserable that the tears kept trickling down his face; "so unlikely a thing in a Vice-Chancellor in his chair that probably nobody saw it. I hope so." The writer of this article, who received his degree on that occasion, well remembers some of his friends commenting on the Vice-Chancellor's obvious "sourness" of manner, and wondering whether he was disgusted at the Senior Wrangler being a man of a rival college. We little thought that this was "none other than sorrow of heart." After some time Dr. Whewell married again, his second wife being Lady Affleck, sister of his friend Robert Leslie Ellis. In her companionship he found great happiness, but after about seven years of married life he was again left alone. This time he appeared unable to rally from the bereavement; "the future which intervened between him and the grave dismayed him by its dark desolation." After this he visibly declined; the torpor of age began to steal over his faculties, and many thought that the years of waiting would not be very many; but they were briefer than any expected. While still comparatively vigorous, a fall from his horse caused fatal injury to the brain, and after lingering for a few days, happily without much suffering, he died on March 6, 1866.

"While life was ebbing fast away on that last morning, blinds and curtains were drawn wide apart in compliance with his wish that he might see the sun shine on the Great Court of Trinity, and he smiled as he was reminded that he used to say the sky never looked so blue as when seen fringed with its turrets and battlements. Almost to the last he was conscious, and the last words intelligibly uttered, when the striking of the clock roused him as day dawned, were, 'The Eternal God is my refuge, and underneath are the everlasting arms.'"

The extraordinary comprehensiveness and versatility of Dr. Whewell's mind is fully depicted in the letters published in Mr. Todhunter's volume, but it is brought out no less, perhaps even more, graphically by some of the brief allusions in his familiar correspondence. This, for example, is one taken from a letter to his sister: "Besides my usual employments [as College Tutor and

Professor of Mineralogy] I have to go to London two days every fortnight as President of the Geological Society, and am printing a book which I have not yet written ["The History of the Inductive Sciences"], so that I am obliged often to run as fast as I can to avoid the printers riding over me, so close are they at my heels. I am, in addition to this, preaching a course of sermons before the University; but this last employment, though it takes time and thought, rather sobers and harmonises my other occupations than adds anything to my distraction." He seemed to be able to turn his hand to anything, and, like a dexterous conjuror, play with half-a-dozen balls at once. Pendulum experiments, theories of the tides, mathematical problems, crystallographic formulæ metaphysics, and various subjects in moral philosophy, classics, modern languages, architecture, geology, with plenty of work in general literature, all make up what we may call in the best sense "farrago libelli." These letters also bring out very clearly another characteristic of Dr. Whewell's mind. He was essentially cautious in regard to change—an advocate of reformation rather than of renovation; in science a systematiser rather than a discoverer; like a navigator who explored to the full the uninvestigated coasts of the Old World, rather than one who steered out into the open ocean in the hope of discovering a New World. This was no doubt partly due to his mathematical training and academic habits of life—but it is very rare, perhaps impossible, to find a memory of extraordinary tenacity and a life essentially studious, combined with originality in one of its highest forms. That requires a good deal of mental fresh air, and is apt to droop a little if too much confined to the atmosphere of a library. This is especially evident in Dr. Whewell's remarks upon the "Vestiges of Creation" and in his essay on the "Plurality of Worlds." The same tone of mind is very conspicuous in his attitude towards the question of University Reform. He was a vigorous opponent of the abuses of private tuition, a zealous advocate of progress in every department of learning, deeply anxious for the improvement of the Classical and Mathematical Tripos examinations, and to him more than to any other single man the recognition of the Natural and Moral Sciences as branches of academic study is due. But he was antagonistic—almost bitterly so—to the appointment of the Royal Commission of 1856 and of its successor, and was hostile in many respects to the changes—now almost universally acknowledged to have been on the whole very beneficial—which were introduced by the statutes of 1859–61. His great hope and desire was that the University should be allowed to reform itself, and be spared any interference from without. That he should have entertained this hope after so many years of academic labour is perhaps the strongest proof of his sanguine temperament.

We must now part from this interesting volume. Perhaps—like the portrait prefixed to it—it slightly fails in depicting the characteristic ruggedness of the man, but it does much to show him as he was to those near and dear to him as well as to the world—a man of immense intellectual power, of intense energy and industry, of high purpose and simple piety, a hard hitter in conflict and a lover of the shout of battle, but too magnanimous to bear ill-will, whether in defeat or victory. Besides this he was a munificent benefactor to his College and his University: one to whom both must long be grateful, and of whom both may well be proud, as having filled a great position in the world of science and literature, and especially as being "the man" (in the opinion of a most competent judge) "to whom, more than to any other single man, the revival of philosophy in Cambridge is due." T. G. BONNEY

OUR BOOK SHELF

Inorganic Chemistry. Adapted for Students in the Elementary Classes of the Science and Art Department. By Dr. W. B. Kemshead, F.R.A.S., F.G.S. Enlarged edition, revised and extended. (London and Glasgow: William Collins, Sons, and Co., Limited.)

THIS work is a typical one. While containing much that is useful and fairly satisfactory, especially from an examination point of view, the whole tendency of the book, considered as an elementary treatise on a branch of natural science, must be strongly condemned.

The leading facts concerning the better-known non-metallic elements and compounds are succinctly stated; the principal reactions of formation and decomposition of these bodies—especially those reactions which unfortunately *must* be "got up" for examination purposes—are arranged in the form of equations; and the simpler arithmetical applications of such equations are illustrated by fully worked-out examples. But chemistry is more than this: facts must be connected together by principles; the connection between fact and theory, and between theory and fact, must be revealed; these two must not be regarded as synonymous, but as mutually dependent; and the reasoning by aid of which theoretical conclusions are reached must be clearly indicated. Chemistry is neither a system of dogmatic assertions nor an accumulation of shibboleths, by the skilful use of which an examiner may make havoc among the Ephraimites crowding to the Jordan of Examination, but a living science.

The principle which is most largely used (or rather misused) in Dr. Kemshead's book is that of Valency; but valency in the hands of this author is deprived of its value as a scientific theory, and becomes an accumulation of fanciful speculations. The basis of the present work is evidently Dr. Frankland's "Lecture Notes"; hence probably the success of the book in preparing examinees for South Kensington (the present is a second and enlarged edition); and is not such success after all of more importance than training chemists or disciplining the mental powers of youth?

The theory of valency is based on the wider molecular theory of matter, which was preceded by the atomic theory of Dalton, itself a development from that system of chemical notation which rested on the combining weights of the elementary bodies. Now it is clear, from many passages, that the author of this book has failed to distinguish combining weights from atomic weights, and atomic from molecular weights: thus on p. 13 we read "these proportions by weight [*i.e.* from the context, these proportions in which "substances unite together chemically"] when reduced to their lowest relative value, and expressed with reference to that of hydrogen, which is usually taken as unity, are called the atomic weights, or combining numbers of the elements." Again, on p. 26, "the *combining weight* of hydrogen being 1, that of oxygen becomes 16; of nitrogen, 14; of carbon, 12," &c. But combining weights are *not* synonymous with atomic weights, and the *combining weight* of oxygen happens to be 8, of carbon 3, and of nitrogen 4·66. The formula weights of compounds are constantly referred to as "atomic and molecular weights." We have such formulæ as $(NH_4O)_2$, CuO_2, &c., stated to be molecular formulæ;

molecule is nowhere defined (in a note on p. 57 a casual statement is made as to the meaning of the term); "Avogadro's law," which lies at the basis of the whole modern edifice of chemistry, is conspicuous by its absence; certain statements as to gaseous combination and to "volume weights" are made, it is true (p. 35), but these are incomplete and misleading.

When a theory of valency is raised on so slender and shifting a molecular foundation as is here laid, no wonder that the edifice should be a strange one; the definition of "atomicity" on pp. 54-55 is incomplete, and cannot be upheld by facts; the statement on p. 58, "it is then a law to which there are no real exceptions, that though the equivalence of an element may vary, it does so always by the addition or subtraction of an even number," is simply untrue. As an "important conclusion" from certain "facts" (? fancies) "on equivalence," it is stated that (p. 59) "a formula which possesses an uneven number of bonds or units of chemical affinity cannot possibly represent a molecule"; without minutely criticising the expression "bond or unit of chemical affinity," suffice it to say that such a formula as, according to Dr. Kemshead, cannot possibly represent a molecule, unfortunately does represent a molecule. The existence of the molecule NO is a case in point: *à propos* of this compound, there is a charming example of the author's method of treating chemical science as a collection of opinions of various authorities to be found in a footnote on p. 169.

Notes on the Crania of New England Indians. By Lucien Carr. From the Anniversary Memoirs of the Boston Society of Natural History, 1880.

THIS is one of the numerous contributions now being made towards our knowledge of the fast-disappearing race of North American Indians. The author, Mr. Lucien Carr, holds the office of Assistant Curator to the valuable Museum of American Archæology and Ethnology at Cambridge, Mass., an institution owing its foundation to the liberality of Mr. Peabody, so well known in England by his benefactions to the London poor, and its scientific excellence to the zeal and organising power of its first curator, the late Dr. Jeffries Wyman, and of his successors.

The object of the present memoir is to collect together such information as is still to be obtained regarding the cranial characters of the native Indians of the New England States, the celebrated "five nations" of the early historians of America, who in consequence of their geographical position were among the first of the race to succumb to the inroads of European immigration. Measurements are given of 67 crania, of which 38 are assigned to males and 29 to females. The averages of these measurements give the following results:—A medium cranial capacity, *i.e.* 1436 cubic centimetres for the males and 1319 for the females. A latitudinal index of ·759, showing mesaticephalism verging upon dolichocephalism. The altitudinal index exactly the same. The principal facial indices show orthognathism, with a strong tendency to mesognathism, a mesorhine nose (index 50), and slightly megaseme orbits (index 88 in the males, and 91 in the females). Although these are the average characters of the whole collection, very few, if any, of the individual crania are to be found presenting them. There is indeed no such uniformity among these skulls as may be seen in certain races, such as Eskimos, Bushmen, Fijians, Andamanese, or even Australians. Perhaps it could scarcely be expected in inhabitants of a large continent, presenting great diversities of climatic and other conditions, and with no natural barriers to free migration and intercourse. The examination of these skulls therefore confirms what has been often remarked before, that although in a broad sense the American Indians present a certain community of type, there is great diversity in detail among them, the result probably of a long series of repetitions of the process of breaking up into distinct groups or tribes and reuniting in various combinations.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Conservation of Electricity

IN a recent communication to NATURE (vol. xxiv. p. 78) Prof. Silvanus P. Thompson very kindly mentions my latest memoir on "The Conservation of Electricity," and, as I am glad to find, confirms my views on this subject by stating that he has independently arrived at the same conclusions with myself.

As regards however the question of priority moved by Prof. Thompson, I think I ought to add that an earlier paper of mine, published five years ago, must have escaped Prof. Thompson's attention. This was printed as an abstract in the *Comptes rendus* of the Paris Academy of Sciences for June 19, 1876, under the title, "Extension du principe de Carnot à la théorie des phénomènes électriques. Équations différentielles générales de l'équilibre et du mouvement d'un système électrique réversible quelconque." I there enunciated the law of the Conservation of Electricity in the same terms as now, and also gave the same analytical method for applying it. I beg leave to quote as a proof the following explicit passage from this extract:—"L'équation $\int dm = 0$ a une autre signification plus simple; elle signifie que de l'électricité peut se déplacer, mais ne peut jamais varier en quantité. Ce principe de la conservation de la quantité d'électricité à été admis par les physiciens dans tous les cas connus jusqu'ici, influence, frottement, etc. . . . . Pour que $\int dm = 0$ pour tout cycle fermé, il faut que dm soit une différentielle parfaite." This method I had already applied in 1875 to the phenomena presented by mercury electrodes (*vide Annales de Chim. Phys.* 1875). In fact my latest memoir is merely a renewed attempt to draw, by means of new applications, the attention of physicists to a fact which I cannot help considering as important for the future, viz. that the principle of the Conservation of Electricity is, *as far as analytical applications are concerned*, the exact analogue to Carnot's Principle for Heat.

Paris, Faculté des Sciences, June 5 G. LIPPMANN

Apparent Decomposition of Sunlight by Intermittent Reflecting Surfaces

IT occurred to me that light might be decomposed by interrupting, with a reflecting surface, a ray of light in such a manner that the interruptions may be proportional to the wave-length period of any particular ray forming a part of a composite ray. The experiment is effected in the following way:—

A wheel having bright spokes (the large wheel of a bicycle answers well) is caused to revolve between an observer and the sun, so that a ray of light is reflected to the observer by a bright spoke; then, when 120 spokes pass before the observer per second, violet light shines out vividly; when 65 pass, red appears, and different rates of revolution give different colours. There seems to be a marked relationship existing between the number of spokes which pass by and the wave-length of the two colours mentioned, that of the violet being $\frac{1}{60000}$ inch, and that of the red $\frac{1}{37000}$ inch.

I am now investigating this apparent relationship between spoke-interruption and wave-length for the other colours of the spectrum of white light, and I hope to be able to make known the results shortly. FREDERICK J. SMITH

Taunton, June 4

Symbolical Logic

I AM sorry that Mr. MacColl should have thought that there was any intention on my part to suggest a doubt as to his having

written his papers without having read Boole's "Laws of Thought." I knew that he was very anxious that the fact should be known, and I called attention to it. I could not state it as a fact known to me. His own assurance was the only ground I had, or could have, to go upon, and in assigning this it never occurred to me to doubt his statement, or to think that I was suggesting doubts to others.

As regards my half humorous suggestion that an attitude of slight social repression was desirable towards novelties of mere notation—not towards new conceptions or methods—I feel sure that almost every one who has not a private scheme of his own to protect will agree with me. Few things can be more perplexing to students of any subject than to find one author after another making use of a new notation to express old results (I mean no special reference to Mr. MacColl here, who does not seem to me one of the worst offenders in this way). At the time of writing my "Symbolic Logic" I had between twenty and thirty such schemes before me. Some of these, of course, express really distinct conceptions, or effect improvements in procedure, but most of them do not; we find half-a-dozen different signs standing for the same meaning, and half-a-dozen different meanings assigned to the same sign. I cannot but think that much of this confusion would be avoided if the various authors would take the trouble to inquire what had been already written upon their subject. The only "repression" I should like to see introduced consists in the remonstrances of reviewers and students generally against the mere substitution of a new symbol for one which was already in use for expressing precisely the same process or conception. So far from wishing to discourage any attempts to improve on the results of Boole and others, I rejoice to see them, and think that Mr. MacColl himself has done some good work in this way. It would have been better still if he had not disfigured it by a notation which I think makes him regard his results as more original than they really are.

I need not seriously discuss those parts of Mr. MacColl's letter which give his opinion as to the impression which will be produced in other persons by a perusal of my book, and his "impression" that he has "somewhere seen Mr. Venn quoted as holding an opinion very much at variance with" a statement which he misquotes.[1] (By the way, I heartily agree with his "protest against that spirit of criticism which would offer two or three chipped bricks as a fair specimen of a house," &c., and think the chipping of the bricks a happy turn.) The rest of his letter contains criticisms upon my conclusions on a variety of rather intricate speculative questions. Having stated my own views as fully and accurately as I conveniently could only a few weeks ago, in a systematic work, I really must decline to be drawn into repeating them again, in a condensed form, in the columns of a scientific journal, even if the editor would consent to accept them. J. VENN

Cambridge, June 12

[1] What I spoke of was "those problems in Probability which Boole justly regarded as the crowning triumph of his system." What Mr. MacColl puts between inverted commas is that Boole "justly regarded his problems in Probability as the crowning triumph of his system," and challenges me to say whether or not I agree with Boole's solution of a certain well-known example. This considerably distorts the meaning of what I said.

Telephones in New Zealand, &c.

OBSERVING your paragraph on this subject in NATURE, vol. xxiv. p. 88, it occurs to me that the following may be of interest:—When in Wellington and Dunedin, N.Z., at the end of December last, my opinion was asked by the Government Telegraphic officials there upon a pair of ordinary "Edison-Bell Telephones" (not Edison *bell-telephones*, as they are too frequently called) which they had just received from the United States for purposes of experiment. A careful trial under various conditions showed me that they were very good average instruments of ordinary delicacy, such as I had seen hundreds of previously in England and the States.

With these instruments, however, Dr. Lemon, the Superintendent of the Postal and Telegraph Service, was able to converse clearly between Wellington and Napier, over an ordinary land line 232 miles in length, while battery currents were passing over the wires on the same posts.

In New Zealand, Telegraphic communication is, and Telephonic communication will be, entirely in the hands of the Government. In Melbourne the telephone-exchange is worked by a private company, but the erection and maintenance of wires is carried out by the Victorian Government at the annual rate of 5*l*. per subscriber. In Sydney, I regret to say, nothing was being done in this matter. In Honolulu I found (last January) telephonic communication all over the town, but no telegraphs at all. The King of the Sandwich Islands however, Alii Kalakaua, who is shortly expected in England, told me that he greatly needed submarine cables between the various islands. On my return to England I had the pleasure of sending to Sydney materials for a private telephonic line on sugar plantations in the Fiji Islands, and my friend Mr. Frederick Cobb, manager of the Falkland Islands Company, tells me that the line he took out there at my suggestion is a great success.

At Wellington, where the central N.Z. telegraph office is, I was very much struck by the extreme ease with which duplex circuits were worked. Dr. Lemon informed me that it was scarcely necessary to alter the resistances once a week. He showed me a simple little carbon rheostat of his own invention which appeared to answer admirably; it consisted essentially of two pieces of carbon, the closeness of whose contact was regulated by a screw.

On my way home I paid a hurried visit to the central office of the Western Union Telegraph Company in New York (just at the critical time of the absorption by it of the other two companies and the consequent creation of a monopoly), and was greatly surprised to see the extent to which the 16,000 cells in the battery room were being replaced by Siemens's dynamo-machines. I was told that one of them would "drive" about fifty wires, and was shown a number of plaster-of-paris cylinders, about five inches long and one inch diameter, which were put into circuit to diminish, when necessary, the intensity of the current. It may be remembered that as a rule American lines are less perfectly insulated than ours, and hence require stronger currents. WM. LANT CARPENTER

6, York Buildings, Weymouth, June 1

Implements at Acton

MR. PERCEVAL'S letter in NATURE, vol. xxiv. p. 101, is an interesting one, but the occurrence of Neolithic implements at and near Acton has been known (if not published) for many years past. In the Pitt-Rivers' collection may be seen Neolithic scrapers and flakes from the Acton district. I have found Neolithic stones in the neighbourhood of Acton and Willesden for many years past; and only a few weeks ago I picked up a beautiful and perfect knife of black flint made from a large flake, five and a half inches long, and one and three quarter inches wide, in the field on the east of Acton Station of the North London Railway. Many of the Neolithic flints from this position are white. A considerable number of Neolithic implements and flakes have at different times been dredged up from the Thames to the West of London, and some of these have been quite recently exhibited. I do not attach importance to the quartzite pebble, as pebbles of quartzite are extremely common in the glacial deposits at the North of London, and very common in the gravels of the Thames and its northern tributaries. They also occur *in situ* at the north of Willesden.

Will Mr. Perceval kindly furnish the heights at the Hammersmith position, and say whether he is positive that the gravel he has in view was dug on the spot, and whether the implements occur there (as his letter implies) in "remarkable abundance"? I have repeatedly examined the low gravels about Hammersmith, Fulham, and Chelsea, but with no result. For more than three years I have never missed an opportunity of looking over the low gravels belonging to these places, together with the positions at West Brompton and Kensington, where thousands of tons of gravel have been excavated. My result has been one dubious flake, probably washed down from one of the higher terraces. I however have heard of two Palæolithic implements having been found—one at Kensington and the other at West Brompton—but whether from the local gravel or not I am uncertain.

I by no means wish to imply that because I have been unable to find implements in the lower gravels therefore some one else may not have found them. Some one may have been always before me and picked them up, or I may have constantly looked over unproductive patches.

The places mentioned by Mr. Perceval are, it must be remembered, frequently ballasted with gravel brought from a distance by the Thames, by the Grand Junction Canal, and by the Great Western Railway. I know of at least five different localities whence the Acton and Hammersmith gravel is brought, one

locality being in Kent. It is therefore of the highest importanee that one should know for eertain whence the gravel has been derived that one sees on the roads.

I live in an implementiferous district, and find Palæolithic implements in the Highbury and Clapton gravels; but a visitor would make a fatal mistake if he supposed that all the gravel on the roads about here belongs to the distriet. Sometimes many tons of gravel are brought here from Walthamstow; at other times from Ware or Hertford; sometimes from Dartford, and from other places. Unless, therefore, the greatest possible care is taken in ascertaining the exact locality whence the ballast comes, mistakes are eertain to occur.

The lowest gravels about here are unproductive of the works of primæval man, with the exception of, at times, a stray flake or two, probably derived from a higher level. The evidence that I have seen in the lower gravels round London points to the correctness of the eonjecture made by General Pitt-Rivers, that the Palæolithic age had passed away before the lower parts of the Thames Valley were excavated.

WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

How to Prevent Drowning

I HAVE read with some interest Dr. MacCormac's letters on the subject of water-treading as means of preventing drowning.

I am sorry that I cannot agree with him, as it would be decidedly a matter of congratulation if some practical means of diminishing the number of casualties from drowning were found. Personal experience, however, prevents my agreeing with Dr. MacCormac.

I am a tolerably good swimmer, can swim in all the different fashions, but I can neither float nor tread water.

Shortly after Dr. MacCormac's first letter appeared I went to swimming baths with a view of putting the matter to the test. I had carefully read Dr. MacCormac's letter, and determined to give it a fair trial. I minutely observed all his directions, and invariably sank every time I tried his plan.

Now it must be remembered that I am a swimmer, and so far as swimming goes, perfectly at home in the water. Moreover, I was not in the least flustered. When I sank I made no attempt to rise again by swimming; I remained in what Dr. MacCormac would call the orthodox position for treading water, only opening my eyes in order to see whether I was ascending or descending. As however I found that I continued to do the latter until I reached the bottom of the bath, and there seemed to be no probability that I should rise without some further effort, I was at last compelled to make this effort.

This was the course of affairs every time I made the attempt. Moreover, whenever I essayed to float on the surface, although I carefully assumed the correct position, threw my head well back, and took the deep inspiration, the resnlt was the same.

Arguing from these faets, it seems to me pretty clear that it is not everybody who can tread water or float. Why this is so, appears to me to lie in the fact that the human body is not always lighter, bulk for bulk, than water. Perhaps with plump children and others with plenty of adipose tissue about their frame this may be the case, but with spare people who consist mainly of muscle and bone, the specifie gravity must be greater than that of water. The body of a fish when the animal is dead will sink until decomposition sets in and causes it to float.

For these reasons I fear that Dr. MacCormac's sнggestion will not be found of so much practical use as he hopes. The apparent ease of the process described by Dr. MacCormac may in itself be the cause of rash proceedings by those who cannot swim, and may so lead to greater loss of life, the very evil which the suggestion is intended to diminish.

W. HENRY KESTEVEN

401, Holloway Road, N., June 7

ON the Continent the facilities are greater than in England, where factories and steam-boats spoil the pleasure of swimming, and everybody is well aware that *all can float upon fresh water without assistance from their hands and feet.* It is what in the Paris swimming-schools is called "faire le mort."

Anybody—stout, lean, eripple, halt—is able to do so, and I taught, myself, a poor little hnnchback how to perform this easy feat; but his deformity placing him in a state of unstable equilibrium, he was obliged to keep his arms stretched at an angle from 45° to 60°.

Some minutes are sufficient in fresh water to make a proficient and a live "mort." The way to do it is very simple, and Mr. MacCormac described it very exactly, with the omission of some particulars relating to the way of breathing, which had no direct reference to his chief and beneficial topie, "treading water."

He who wishes to "faire le mort" must first draw a deep breath, and keep it, then put himself on his back, with his head thrown backwards, as recommended by Mr. MacCormac, and allow his limbs to droop slackly without any stiffness, no matter in what position.

The body will sink at first under water, but it will immediately rise nearly on a level with the surface, the only parts quite free from water being the chest and the nose and mouth, around which the water describes an oval, whilst the eyes are at times over, at times under, water.

The "mort" can remain floating in this way as long as his breath allows, though it is better not to wait longer than two or three seconds, to avoid fatigue; then he must quiekly emit it, draw another deep breath, and keep it again.

The body sinks as before, rises immediately, regaining its floating position, nose, mouth, and chest emerging again from water.

This ean be continued for hours together without the least motion of legs or arms, as your readers will be able to verify for themselves, either at the Pont Royal or Ligny swimming schools, on their visit to the Paris Exhibition of Electricity.

Jersey, June 5 CHATEL

P.S.—I ought to add that whilst floating on fresh water the body is not quite on a level with the surface, but from the chest, that is out of water, to the toes, which are about six or eight inches under water, figures an inclined plane, the slope of which varies with everybody, and that any attempt to bring the toes on a level with the surface makes the body sink. On the contrary, the deeper the head is sunk backwards under water the more the body emerges.

Auroric Light

JUNE 6, faint lights, especially to the northward, between 10 and 12; smart frost.

June 7, at 10, masses of purplish light rising from the north-east and congregating about the zenith; pencils of greenish yellow and white rising to the north; these continued up to 12, after which no observations were made; very smart frost, which bit the potato-stalks.

Jnne 7, from 10 to 12, well-marked and at times brilliant columns, pencils, and masses of red rising all round the heavens at intervals, and congregating at the zenith; a most severe white frost that burnt up all the potatoes on the valley flats and on the uplands. At 5.30 on the 8th the frost was so thick that the ground had the appearance as if it had snowed during the night.

Ovoca, Ireland, June 10 G. H. KINAHAN

A Singular Cause of Shipwreck

IN NATURE, vol. xxiv. p. 106, you mention a "singular case of shipwreck" caused by waves and spray freezing on a steamer and sinking it by its weight. Cases of this kind caused by frozen spray alone are known near the east coast of the Black Sea. North of 44°, where the mountains are not very high, an exceedingly strong and sudden north-east wind is frequent, quite similar to the Dalmatian Bora, and called alike. It descends at a certain angle to the sea, raising a great quantity of spray. In winter this spray immediately freezes, and ships may sink by its weight. On January 25, 1848, a war-ship, anchored in the middle of the Bay of Noerrossiisk, sank in this manner. As the weather was fine before, a great part of the crew were ashore, and the storm arrived with such suddenness that the ship sank from the weight of the frozen spray. On account of the bora this coast is avoided by merchant-ships in winter, and visited only by a line of steamers subventioned by the Government.

St. Petersburg, Jnne 8 A. WOEIKOF

OBSERVATIONS ON THE HABITS OF ANTS

ON Thursday (June 2) Sir John Lubbock read a further paper on this subject at the meeting of the Linnean Society. He said that in one of his former papers

(Linnean Soc. *Journ.* vol. xiv. p. 278) he had given a series of experiments made on ants with light of different colours, in order if possible to determine whether ants had the power of distinguishing colours. For this purpose he utilised the dread which ants, when in their nest, have of light. Not unnaturally, if a nest is uncovered, they think they are being attacked, and hasten to carry their young away to a darker, and, as they suppose, a safer place. He satisfied himself, by hundreds of experiments, that if he exposed to light the greater part of a nest, but left any part of it covered over, the young would certainly be conveyed to the dark portion. In this manner he satisfied himself that the different rays of the spectrum act on them in a different manner from that in which they affect us; for instance, that ants are specially sensitive to the violet rays. But he was anxious to go beyond this, and to attempt to determine how far their limits of vision agree with ours. We all know that if a ray of white light is passed through a prism, it is broken up into a beautiful band of colours—the spectrum. To our eyes it is bounded by red at the one end and violet at the other, the edge being sharply marked at the red end, but less abruptly at the violet. But a ray of light contains besides the rays visible to our eyes others which are called, though not with absolute correctness, heat rays and chemical rays. These, so far from being bounded by the limits of our vision, extend far beyond it, the heat rays at the red, the chemical rays at the violet end. He wished under these circumstances to determine if possible whether the limit of vision in the case of ants was the same as with us. This interesting problem he endeavoured to solve as follows:—If an ant's nest be disturbed the ants soon carry their grubs and chrysalises underground again to a place of safety. Sir John, availing himself of this habit, placed some ants with larvæ and pupæ between two plates of glass about one-eighth of an inch apart, a distance which leaves just room enough for the ants to move about freely. He found that if he covered over part of the glass with any opaque substance the young were always carried into the part thus darkened. He then tried placing over the nest different coloured glasses, and found that if he placed side by side a pale yellow glass and one of deep violet the young were always carried under the former, showing that though the light yellow was much more transparent to our eyes, it was, on the contrary, much less so to the ants. So far he had gone in experiments already recorded; but he now wished, as already mentioned, to go further, and test the effect upon them of the ultra-violet rays, which to us are invisible. For this purpose, among other experiments, he used sulphate of quinine and bisulphide of carbon, both of which transmit all the visible rays, and are therefore perfectly colourless and transparent to us, but which completely stop the ultra-violet rays. Over a part of one of his nests he placed flat-sided bottles containing the above-mentioned fluids, and over another part a piece of dark violet glass; in every case the larvæ were carried under the transparent liquids, and not under the violet glass. Again, he threw a spectrum into a similar nest, and found that if the ants had to choose between placing their young in the ultra-violet rays or in the red they preferred the latter. He infers therefore that the ants perceive the ultra-violet rays, which to our eyes are quite invisible.

Now as every ray of homogeneous light which we can perceive at all appears to us as a distinct colour, it seems probable that these ultra-violet rays must make themselves apparent to the ants as a distinct and separate colour (of which we can form no idea), but as unlike the rest as red is from yellow or green from violet. The question also arises whether white light to these insects would differ from our white light in containing this additional colour. At any rate, as few of the colours in nature are pure colours, but almost all arise from the combination of rays of different wave-lengths, and as in such cases the visible resultant would be composed not only of the rays which we see, but of these and the ultra-violet, it would appear that the colours of objects and the general aspect of nature must present to them a very different appearance from what it does to us.

Similar experiments which Sir John also made with some of the lower Crustacea point to the same conclusion, but the account of these he reserved for a future occasion. He then proceeded to describe some experiments made on the sense of direction possessed by ants, but it would not be easy to make these intelligible without figures. After detailing some further experiments on the power of recognising friends, he gave some facts which appear to show that ants by selection of food can produce either a queen or a worker at will from a given egg. Lastly he stated that he had still some ants which he had commenced to observe in 1874, and which are still living and in perfect health; they now therefore must be more than seven years old, being therefore by far the oldest insects on record.

THE WEATHER AND HEALTH OF LONDON[1]

TO the statistician London affords materials for the prosecution of many inquiries such as could not be obtained from the statistics of any other city either in ancient or modern times. Among the more important of these inquiries are those which relate to questions suggested by the enormous aggregation of human beings over a limited area which London presents on a scale absolutely unparalleled in the world's history. It is one of these questions we bring before you this evening, viz., the influence of the climate on the health of the people of London.

The relation of weather to health is a question which has engaged the attention of Dr. Arthur Mitchell and myself for many years. In an early stage of the inquiry our attention was mainly directed to Scotland, and more particularly to the data supplied by its eight large towns; but it was soon found that, owing to the sparseness and other conditions of the population, and to the fact that the division of time into months only, adopted by the Registrar-General for Scotland, they were not sufficiently minute to show the true relations of weather to the fluctuations of the death-rate through the year. In truth it was only after not a little unsuccessful labour, and what could at best be characterised as no more than partially successful work, that we resolved eight years ago to open the discussion of the whole subject by an exhaustive examination of the meteorological and vital statistics of London and London alone. More specifically our reasons for the selection of London were that it afforded data from (1) an enormous population spread over an area so limited that it might be regarded as having one uniform climate during each of the seasons of the year; (2) full weekly reports of weather and the deaths from the different diseases; and (3) returns extending over a sufficiently long period.

In the case of diseases such as diarrhœa and bronchitis, which seem to be directly and immediately under the influence of temperature, and such epidemics as scarlet fever and whooping cough, the rate of mortality from which is largely determined by season and weather, a comparatively small number of years is required to give a satisfactory approximation to their true weekly curve of mortality. But as regards the great majority of diseases, it quickly became apparent that a thirty years' average was required in the construction of curves which could be accepted as true "constants" for the diseases to which they refer. The thirty years beginning with 1845 were therefore adopted. An examination of the curves shows that some of their striking features, particularly those

[1] Substance of a Lecture delivered at the Royal Institution, March 25.

showing the complications of special diseases and their connections with each other, which the weekly averages disclose, would entirely disappear if monthly averages only were employed.

The curves of the more prominent and interesting of the diseases are shown on the accompanying woodcuts, the straight black line in each figure being drawn to represent the mean weekly death-rate on an average of the fifty-two weeks of the year, and the figures on the margin the percentages above or below the average. With this general average the mean death-rate of each week is compared and the difference above or below calculated in percentages, which, when *plus, are placed* above the mean line of the figure, and when *minus*, below it. Thus as regards scarlatina (Fig. 1), the mean of the fifty-two weeks is 49·6; on the first week of January it is 7 per cent. above the mean, from which time it continues to fall to the annual minimum, 35 per cent. below the mean in the middle of March, thence rises to the mean in the end of August; to the annual maximum, 60 per cent. above the mean, in the end of October, and thereafter steadily falls. The portion of the curve above the mean line thus shows the time of the year when, and the degree to which, the mortality from scarlatina is above its average and the portion below the line when it is under it.

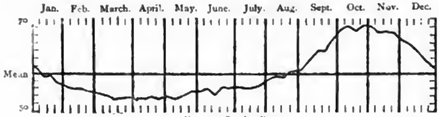


FIG. 1.—Scarlet Fever.

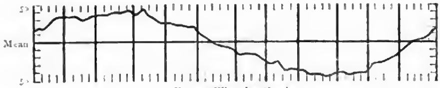


FIG. 2.—Whooping Cough.

FIG. 3.—Small-pox.

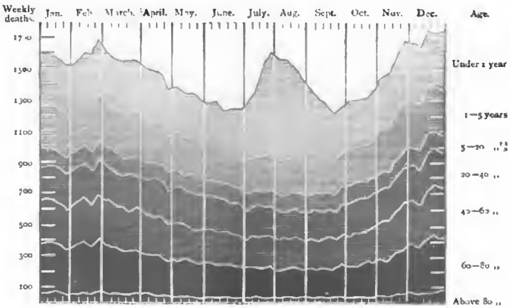


FIG. 4.

Fig. 2 shows similarly the distribution of the mortality from whooping-cough through the weeks of the year, and Fig. 3 the distribution of the mortality from small-pox. It is seen at once that the mortality curve from scarlatina is precisely the reverse of the curve of whooping-cough, the maximum death-rate period of the one corresponding to the minimum period of the other, and *vice versâ*. It is also seen that the mortality curve for small-pox (Fig. 3) is quite distinct from the other two curves.

In order to ascertain the degree of steadiness of these curves, a curve was calculated and drawn for each of the seven epidemics of scarlatina and for each of the

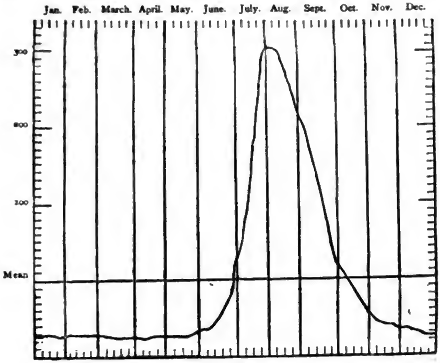


FIG. 5.—Diarrhœa.

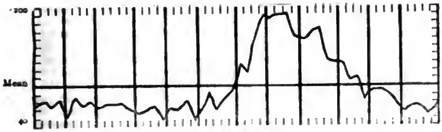


FIG. 6.—Thrush.

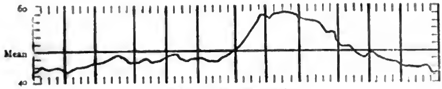


FIG. 7.—Tabes Mesenterica.

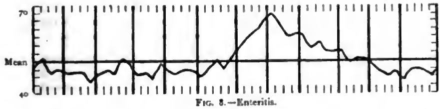


FIG. 8.—Enteritis.

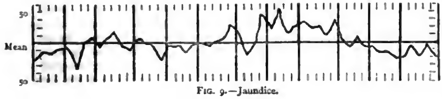


FIG. 9.—Jaundice.

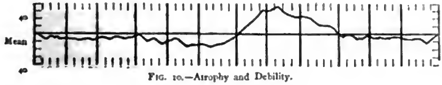


FIG. 10.—Atrophy and Debility.

eight epidemics of whooping-cough during the thirty years, with the instructive result that the curve for each of the separate epidemics was substantially identical with the general curve for the whole thirty years' period, each of the four prominent phases of each curve occurring all within a week of each other. As regards the small-pox curve, if the deaths during the epidemic of 1870-72, by far the most fatal of all the epidemics during the thirty years, be deducted from the general result, we obtain a curve which is substantially the same curve as that for the whole thirty years, but only less pronounced. From these results it follows, and the remark is of general application to all the curves, that the mortality curves for the different diseases arrived at in this inquiry may be regarded as true constants of these diseases for London.

The climate of London, looked at as influencing the health of the people, may be divided into six types of weather according to the season of the year. These are respectively—

Period 1.—Damp and cold, fourth week of October to third week of December.

Period 2.—Cold, fourth week of December to third week of February.

Period 2.—Dry and cold, fourth week of February to second week of April.

Period 4.—Dry and warm, third week of April to third week of June.

Period 5.—Heat, fourth week of June to first week of September.

Period 6.—Damp and warm, second week of September to third week of October.

The outstanding features of the death-rate in its relation to the varying types of weather through the year are shown by the top curve of Fig. 4, which represents the total mortality for all ages. This curve shows two maxima in the course of the year: the one, by far the larger of the two, extending over six months from November to April, and the other embracing the period from about the beginning of July to the autumnal equinox. It will be also observed that the comparatively short-continued but strongly-pronounced summer maximum is restricted to mere infants, whereas the larger winter maximum is a feature of the curves for all ages.

Figs. 5 to 10 are representative curves of those diseases which go to form the summer maximum when "*heat*" is the chief characteristic of the weather. The direct relation of the progress of mortality from diarrhœa to temperature is strikingly seen in the startling suddenness with which the curve shoots up during the hottest months of the year, and the suddenness, equally startling, with which it falls on the approach of colder weather. The curves for dysentery, British

cholera, and cholera are substantially the same as the curve for diarrhœa, all showing the same close obedience to temperature. It is a noteworthy circumstance that these four curves group themselves into pairs—diarrhœa and British cholera on the one side, and dysentery and Asiatic cholera on the other. The chief points of difference are that dysentery and Asiatic cholera begin markedly to rise considerably later than the other two allied diseases, attain their maximum a month later, and fall more rapidly than they rose, the annual phases being nearly a month later than those of diarrhœa and British cholera.

The peculiarly malignant character of summer diarrhœa among young children under five years of age may be shown by the weekly mortality from diarrhœa, rising from 20 in the middle of June, to 342 in the first week of August, 1880, when the mean temperature of July and August was about the average. In July, 1876, when the temperature was 3°·6 above the average, the weekly mortality from diarrhœa among children rose to 502 on the last week of that month. On the other hand, during the cold summer of 1860, the diarrhœa mortality for all ages did not in any week exceed ninety.

Of the British large towns the lowest mortality from summer diarrhœa is that of Aberdeen, which has the lowest summer temperature. The diarrhœa mortality of

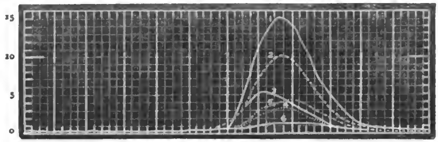


FIG. 11.—Weekly Deaths from Diarrhœa calculated on the Annual Mortality per 1000 of the population.

each town is found from year to year to rise proportionally with the increase of temperature, but the rate of increase differs greatly in different towns, thus pointing to other causes than mere weather, or the relative temperatures and humidities of these towns, as determining the mortality. Fig. 11 shows the weekly death-rate from diarrhœa for six of the large British towns, viz., Leicester, curve 1; Liverpool, 2; London, 3; Bristol, 4; Portsmouth, 5; and Edinburgh, 6; from which it is seen that though the summer temperature of London is hotter than that of Liverpool and Leicester, its diarrhœa mortality is very much less. In this respect London contrasts very favourably with the great majority of British large towns, showing its sanitary conditions generally are at least fairly satisfactory; but inasmuch as it is somewhat in excess of a few of the towns whose summer temperature is scarcely lower, London offers problems in this field to the sanitary reformer for his solution.

Figs. 6 to 10 give the curves for thrush, tabes mesenterica, enteritis, jaundice, and atrophy and debility, all of which have their maximum fatality during the hottest period of the year, and all of which, it will be noted, are bowel complaints. Indeed with the apparent exception of one or two nervous diseases, all those diseases which indicate an increase in their death-rate during the summer months are bowel complaints. ALEXANDER BUCHAN

(To be continued.)

NOTES

THE British Association having decided to hold its annual meeting this year at York, where, fifty years ago, its first meeting was held, it has been thought that advantage should be taken of this jubilee meeting to show, as far as possible, the progress which has been made during the past half century in the construction of instruments of scientific research, and, with this view, it has been decided to invite men of science, scientific societies, and manufacturers to exhibit, at the meeting, instruments of the latest patterns, and tools used in their construction, and if the science be fifty years old, the instruments used in 1831; otherwise specimens of the earliest patterns that can be found. The Exhibition will be for the week of the meeting only, viz. from August 31 to September 8. To ensure that specimens intrusted for exhibition shall be as advantageously placed as possible, a special sub-committee, called "The Museum Sub-Committee," has been formed at York, who will be happy to afford every possible information. The sub-committee includes several gentlemen who have had considerable experience in the arrangement of exhibitions, and they will give their personal attention to the unpacking, arrangement, and care of objects intrusted to them, so that the risk of injury will be reduced to a minimum. The articles exhibited will be insured against fire, and provision will be made for police protection; beyond this the committee does not hold itself responsible. It is requested that exhibitors will put a value on their exhibits, in order that the necessary insurance may be made. The proposed Exhibition will also include apparatus and specimens illustrative of papers to be read at the Association meeting, which the authors may be willing to allow to be examined at leisure, as well as instruments constructed for the prosecution of special researches which have not yet become articles of ordinary commerce. It is very desirable that such instruments and apparatus should be exhibited in action, if arrangements can be made for the purpose.

THE thirtieth meeting of the American Association for the Advancement of Science will be held at Cincinnati, Ohio, commencing on Wednesday, August 17, and following days. As it is generally believed that the Cincinnati meeting will be the largest and most important scientific meeting ever held in the West, every effort will be made by the Local Committee to prepare, in advance of the meeting, a satisfactory general programme for the week. At the Boston meeting several changes in the constitution were proposed, which will come up for action at Cincinnati, the object of the changes being the reorganisation of the sections, and also to extend the scope of the Association. Should these changes be adopted, the Association will embrace eight sections of equal standing, each presided over by a vice-president, and having its own secretary and sectional committee. The following is the division proposed, and upon which final action will be taken at Cincinnati:—Sec. A. Physics; Sec. B. Astronomy and Pure Mathematics; Sec. C. Chemistry, including its applications to Agriculture and the Arts; Sec. D. Mechani-

cal Science; Sec. E. Geology and Geography; Sec. F. Biology; Sec. G. Anthropology; Sec. H. Economic Science and Statistics. Also I. A Permanent Subsection of Microscopy, which shall elect its own officers and be responsible directly to the Standing Committee. Several excursions will be arranged for by the Local Committee, and will be announced on their circular. Special excursions will be arranged for the Anthropological Section to Fort Ancient, Madisonville, and other places of interest. The Permanent Secretary of the Association is Prof. F. W. Putnam, Salem, Mass.

THE Paris Academy of Sciences elected, on June 13, M. Fouqué, Professor of Mineralogy to the Collége de France, a Member of the Section of Mineralogy, to fill the seat vacated by the death of M. Delesse.

AT the first General Meeting of the Members and the Honorary Council of the Sanitary Assurance Association on Friday last, Prof. Corfield (Chief Sanitary Officer) and Mr. Judge (Surveyor) related to the meeting the progress of the Association, and reported on the work of sanitary inspection that had been done. The property which had been placed on the Assurance Register varied in value from houses rated at 60*l.* a year, in which the total fee to subscribers for report, supervision of work, and certificate, is two guineas, to houses rated as high as 700*l.* a year, with proportionately increased fees. The Association undertakes the inspection of the smallest class of property, and no fee is charged to subscribers for a single house rated at 20*l.*, while the fee is only half a guinea for houses rated at 40*l.* In the discussion which followed, Sir Richard Temple, Capt. Douglas Galton, and Mr. Whichcord spoke strongly in support of the objects of the Association, and the Council were requested to take steps to make the Association as widely known as possible, and particularly to call the attention of the proprietors of large building estates to the advantages which would accrue if the certificate of the Association was made essential to the granting of leases.

THE prize programme of the Belgian Academy of Sciences for 1882 consists of the following subjects (briefly stated):—Distribution between acids and bases, in mixture of solutions of salts which, by their mutual reaction, do not produce insoluble substances; *exposé* of present knowledge of torsion and improvement of it; extension of knowledge of the relations between the physical and chemical properties of simple and compound bodies; description of Belgian Tertiary strata of the Eocene series; influence of the nervous system on regulation of temperature in warm-blooded animals; relations of the pollinic tube to the ovum in one or more Phanerogams. Medals of the value of 600 francs are offered in connection with each question, except the third, for which the medal is valued at 1000 francs. The time-limit is August 1. For 1883 the following three questions are adopted:—1. Establish by new experiments the theory of reactions presented by substances in the nascent state. 2. Prove the truth or falsity of Fermat's proposition: To decompose a cube into two other cubes, a fourth power, and generally any power, into two powers of the same name, above the second power, is impossible. 3. New spectroscopic researches are required, showing especially whether or not the sun contains the essential constituents of organic compounds. A gold medal of 800 francs value is offered for solution of any one of these. The time-limit is August 1, 1883. Memoirs must be written in French, Dutch, or Latin, and sent in with motto and sealed envelope to the secretary.

WE learn that M. Planté, the inventor of the electrical accumulator, intends to organise a factory for the sale of his instruments. M. Planté considers himself obliged to take this step in order to show that the principles of his original apparatus are sufficient to work them with advantage. It appears that the Faure accumulators with oxide of lead cannot be loaded except by a battery, and that the original lead can be worked by a magneto-electric machine, by taking some precautions which will be described shortly. M. Plante is constructing for M. Tissandier an accumulator on his original system, which will be used to direct a small elongated balloon. It is intended to exhibit it in the nave of the Palais de l'Industrie in August next.

OUR Paris Correspondent writes: On June 10 an interesting experiment took place in Paris. A little after midnight a tram-car belonging to the Omnibus Company conveyed forty persons from the Place du Trone to the Boulevard Richard Lenoir and back at a velocity of six miles an hour. The motive power was supplied by 160 Faure accumulators, weighing 18 lbs. each. An interesting feat was accomplished, but not quite such as was anticipated. The work could have been done by two horses. The experiment lasted about one hour, and the power of the motor, although not exhausted, was much diminished.

M. JOSÉ CUSTODIO, Marinha Grande, Leiria, Portugal, writes to say that the centenary of the death of the great Portuguese Minister, the Marquis of Pombal, is to be celebrated on May 8 of next year. In connection therewith it is desired to obtain information about Williams Stephens, who founded the first royal manufactory of glass in Portugal, under the patronage of Pombal. Any information whatever concerning Stephens will be welcomed.

WE would draw the attention of our readers to the announcement of the first general meeting of the Society of Chemical Industry on the 28th and 29th inst., at the Institution of Civil Engineers, with Prof. Roscoe in the chair.

THE Portfolio of Drawings of Living Animals and Plants issued by Mr. Thomas Bolton for June, 1881, is a very creditable production, and we are glad to call our readers' attention to the opportunity there is afforded to them by the labours of Mr. Bolton, of investigating fresh and living specimens of very many interesting forms of animal and vegetable life—for the most part of quite microscopical size—and at the same time of having, by the drawings which accompany these forms, an excellent sketch of what they are to expect to find, and a short but authentic history of what is known about them.

M. MASCART, the director of the French Meteorological Service, is devising a new registering magnetometer, which is intended to have all the indications recorded on one roll of paper.

AN earthquake shock was felt in Switzerland on Thursday morning last. The shock occurred at 12.35 a.m. The direction was south-west to north-east at Geneva, and north-east to to south-west at Lausanne, Martigny, and Bex. Prof. Morel of Morges describes it as having been for one region very intense; it was felt from Martigny and Bex, in the Valais, to the valley of Joux, in Vaud; at Geneva, Chamounix, and all round Lake Leman. Its centre was probably in the valley of the Upper Rhone, where seven or eight oscillations were distinctly perceived, accompanied in many places by subterranean thunder, bells were rung, walls cracked, slates dislodged, and chimneys overturned. It was also felt at Osmondo, notwithstanding the great height of the village above sea-level. A second shock was felt some hours later in the same locality.

INTELLIGENCE received at Constantinople on June 9 from Van states that an earthquake has devastated thirty-four villages in that district. Another shock of earthquake occurred at Chios at half-past nine on Saturday morning, causing the fall of a Turkish minaret and of several ruined houses in the town. Oscillations of the ground are constantly noticed in Croatia. Thus on May 19 at 2 a.m. a violent shock, lasting three seconds, and accompanied by subterranean noise, was observed at Glina.

The shock was felt at Agram also, and at several other Croatian towns. On May 23, at 8.21 p.m., an earthquake was observed at Metkovich (Dalmatia). It lasted eight seconds, its motion was wave-like, and in a south-westerly direction; at 9.45 p.m. a second one followed. At Janina no less than seven different shocks were noticed on the same day; they varied considerably in strength, the first one occurred at 10.15 a.m., the last one at 10.57 p.m. All were accompanied by subterranean noise. At Stagno two violent shocks occurred at 8.23 and 9.3 p.m., and at Slano a moderate one at 8.35 p.m. On May 22 at 6.15 p.m. an oscillation of the ground was observed at Zwickau (Saxony); the direction was from north-east to south-west. On May 21 a moderate shock was noted about 11 p.m. at Copenhagen and in the vicinity. It lasted six seconds.

MR. W. SOWERBY, writing from the Botanical Gardens to the *Times*, states that the fresh-water jelly-fish described in NATURE a year ago by Professors Allman and Ray Lankester, has reappeared in the Victoria Regia tank in the Gardens. It is a curious fact that the date of its first discovery (June 9, 1880) should be so near the day of its reappearance—viz. June 12; as during the winter the tank is cleaned out and remains for some months empty.

THE numbers of the present year's issue of our northern namesake, *Naturen*, under the recent editorship of the eminent Norwegian geologist, Hans F. Reusch, continue to provide well written popular expositions of scientific questions. Dr. Leonhard Stejneger returning to a subject which he had treated of in early numbers, considers the causes which influence the migration of birds, which he is disposed to seek principally in the necessity originally imposed on earlier generations to seek food by change of locality, when the cold in one region, and the heat in another, destroyed the smaller animals, or the plants, from which these birds sought their nourishment, while the sense once developed became in process of time an hereditary instinct. The editor describes the working of the telephone system in Christiania, where, since June, 1880, a central station, in which the work is done by women, has been established in connection with Dr. Bell's Company in New York, and under the direction of Herr Hugo Ullitz. The apparatus used is the so-called Blake's microphone. Herr Geelmuyden draws attention to the expediency of adopting one mean time, viz., that of Christiania, for all Norway. The difficulty of establishing one normal time for the whole country is especially great in Norway, where, for instance, some districts—as Vardö and Vadsö—lie further east than Constantinople, while the west coast has nearly the same W. long. as Marseilles. As one of the curious results that would follow the adoption of the time of Christiania as the normal standard he mentions that the midnight sun at the North Cape would have to be looked for at 11 P.M. A colossal pine which was lately uprooted by an inundation at Pÿhajoki in Oulais, Finland, was found to have 1029 annual rings. The Norwegian Arctic Expedition has yielded a new fish bearing affinity with the Ophidiidæ, but presenting sufficient differences to justify its recognition as a hitherto unknown northern form, for which Dr. R. Collett has suggested the name *Rhodichthys regina*. The entire yield of fish in the trawl-nets at great depths (from 1300 to 1400 fathoms) was 234 individuals, belonging to thirty-two different genera, of which seven had been previously unknown to science.

THE deaths are announced of Dr. Jakob Bernays, Principal Librarian at the Bonn University, and of Dr. Richard Ladislaus Heschl, Professor of Pathological Anatomy at the University of Vienna (the successor of Rokitanski). Both were fifty-seven years of age, and both died on May 26.

THE Highbury Microscopical and Scientific Society gave a *conversazione* at Harecourt Hall, Canonbury, on Thursday, the 9th inst., which was numerously attended.

THERE is a regular mania in Paris at present for publishing periodicals connected with electricity. A new electrical weekly paper called the *Telephone* has issued its first number; it is the fifth in existence. We are told moreover that the first number of another, the *Electrophone*, will be issued in a very few days.

MR. HENRY WALKER has issued a useful little "Guide to the Popular Natural History Societies of London." In London and suburbs there are twenty such associations.

THE *conversazione* to commemorate the fiftieth anniversary of the Harveian Society of London will be held on Wednesday, June 29, at the South Kensington Museum.

EXPERIMENTS have been made during the past few days in lighting the House of Commons by means of the electric light.

SEVEN solar lamps were lighted by electricity about a week ago in Paris by a Siemens machine, situated in the *mairie* of rue Drouat. These lamps, which are perfectly regular, and placed in the most crowded part of the Paris Boulevards, near Passage Jauffroy, have created a sensation.

THE concerts of the Palais Royal will be resumed in a few days. The gardens will be lighted by no less than eighteen Jablockhoff lamps. It is intended to place a miniature electric boat on the basin manned by a little girl.

THE annual Congress in connection with the French Society of Archæology will be opened on June 28 at Vannes (Morbihan). A long and interesting programme has been prepared for the meeting.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*) from South Africa, presented by Miss Agnes Robertson; a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mr. Hamilton Kerr; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr. H. Aylesbury, steam yacht *Albion*; a Common Ocelot (*Felis pardalis*) from America, presented by Mr. P. Leckie; two Common Peafowls (*Pavo cristatus*) from India, presented by Mr. George Stevenson; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from the Moluccas, presented by Miss Rose Hubbard; three Waxwings (*Ampelis garrulus*), European, purchased; a Cape Buffalo (*Bubalus caffer*), a Japanese Deer (*Cervus sika*), born in the Gardens; nine Summer Ducks (*Aix sponsa*), a Jameson's Gull (*Larus jamesoni*), bred in the Gardens.

GEOGRAPHICAL NOTES

AT its last meeting the Russian Geographical Society announced the nomination of M. Yurgens as Director of the Polar Meteorological Station on the Lena, and of M. Eichner and Dr. Bunge as his assistants. The Society also voted sums of money for sending M. Kouznetsoff for the anthropological exploration of Tartarian tribes and for M. Malakhoff, who goes to the provinces of Vyatra and Oufa for the exploration of caverns and of remains of former settlements.

WE find in the *Izvestia* of the Russian Geographical Society the following information as to the geodetical work which was done by the Russian officers on the Balkan peninsula during the last war. The whole of Bulgaria and Eastern Roumelia was covered with a net of trigonometrical triangles, as well as the portion of Turkey between Adrianople, Dede-agatch, and Rodosto, and from Yambol, through Adrianople, to Constantinople and Bourgas. The net goes also into Servia and along the Danube, the total number of geodetically-determined spots being 1289; for all these spots there were also made determinations of heights. The highest determined summits on the Balkan mountains are Youmroutchkal (7791 feet), and Vajan (6217 feet); and in the Rhodope Mountains: Karlyk (9846 feet), Karlyk-moolah and Snika (both 7189 feet high). The longitudes of eleven principal towns (Rouschouk, Sistova, Tirnova, &c.) were determined with great accuracy, and those of fifty-seven others either by telegraph or by chronometers, and

they were chosen in such a manner as to determine the influence of the Balkan chain on the deviation of the pendulum. As to the topographical work, no less than 133,750 square versts were mapped during the war, of which 110,500 square versts were mapped on the scale of $\frac{1}{42000}$, and the heights of more than 113,000 spots were determined, so that there are all necessary data for making an embossment-map of the whole of the mapped parts of Roumelia and Bulgaria.

DR. RAE sends us the following extract from a letter to him from Capt. Howgate, dated May 23, 1881:—"Our Arctic work here is progressing finely, so far as our fitting is concerned. The *Jeannette* search vessel is to get off early in June, if she fills up her complement of men. For our Lady Franklin Bay work the steam sealer *Proteus* of St. John's, Newfoundland, has been secured. She is a vessel of 688 tons burden, and contracts to deliver, with the colony and supplies, one hundred tons of coals at Lady Franklin Bay, which will guard against failure in the item of fuel, should the coal-seam not turn out so well as expected. The complement of men has been made up, and the shipment of stores to St. John's actually commenced, so there is every reason to expect that the expedition will sail from that port on July 4, as originally intended. The Point Barrow party is nearly filled up, and will be finished this week, I believe."

A SHORT time back it was stated that Mr. James Stevenson of Glasgow had offered to contribute 4000*l*. on certain conditions for the construction of a road between Lakes Nyassa and Tanganyika. The Foreign Missions Committee of the Free Church of Scotland have resolved to do their part by establishing a station among the Chungus at Maliwanda, a place about fifty miles on the proposed line of road from Lake Nyassa. The London Missionary Society have agreed to open a station at Zombé, twenty miles to the south-east of Lake Tanganyika. In order to found the Livingstonia Mission's new stations and superintend the construction of the road, Mr. James Stewart, C.E., left England on May 13 with three artisans, and another is to follow. In the autumn also it is probable that another medical missionary will go out to Lake Nyassa.

BY the aid of a correspondent at the Gaboon who wrote on March 30 a contemporary has received the startling intelligence that M. de Brazza "got to Stanley's Pool from the Ogowé and came down the Congo." Some people however may be aware that this information was made public at the meeting of the French Geographical Society on January 21, when M. Duveyrier tried to make quite clear what is evidently not yet known at the Gaboon, viz. that after he had founded the Ogowé and Stanley Pool stations and descended the Congo, the mission confided to M. de Brazza by the French branch of the International African Association ceased, and the two stations, it is well known, are to be taken charge of by M. Mizon and another Frenchman. M. de Brazza is now engaged on an expedition for which the French Chambers have made a liberal grant, and in which he will be accompanied by his former colleague, Dr. Ballay. These two are to descend the Alima to the Congo in a steam launch, and then to make a thorough examination of the valley of the great river, part of their object being to divert trade to some extent to the Ogowé. The writer of the letter from the Gaboon believes that "Stanley will find de Brazza established there [? Stanley Pool] when he gets up." This of course is a matter of chance, as M. de Brazza has now a sort of roving commission on the Congo, but, no doubt, Mr. Stanley will find some one at the Ntamo station (now called Brazzaville), as Messrs. Crudgington and Bentley in February found a French sergeant and two soldiers there, and by this time possibly M. Mizon or some one else will have arrived to take charge.

IN consequence of the success of the preliminary journey which Mr. Crudgington and his companion have just made along the north bank of the Congo to Stanley Pool, the Baptist Missionary Expedition will now definitely adopt this route into the interior. As the result of a long conversation with Mr. Stanley on the subject, the party consider that it will be best to take advantage of his road as far as Isangila, and then to place a steel boat on the river above the falls there. Afterwards there will be no insuperable difficulty in the navigation of the river, except perhaps in two or three places where the boat will be taken to pieces and carried past the cataracts. A boat is now being built for the expedition in London from the plans and drawings of Mr. Stanley, who has willingly afforded the party the benefit of his advice and assistance. The adoption of this plan will obviate the necessity for passing through the country of the troublesome Basundi, and will materially hasten the progress of the expedition.

HERR ERNST VON HESSE WARTEGG, the well-known traveller, has just returned to Europe from Africa, where he went up the Nile, and then crossed the desert between that river and the Red Sea, making important excavations and discoveries of ancient Egyptian remains, among which were a very interesting sarcophagus, pottery, statuary, &c. He recently gave a lecture before the Geographical Society of Alsace-Loraine at Metz, exhibiting several hundred photographs and ethnographical objects. Some time ago Herr von Hesse Wartegg was elected Honorary Member of the Royal Belgian Geographical Society and Corresponding Member of the Geographical Society of Metz. His travelling companion, Dr. Theodor Hoerner, has gone from Suakin to Kassalla, and from there through the Kunama country to Massawah.

IN the *Colonies and India* we find some particulars respecting a projected expedition from New Zealand to New Guinea for the purpose of exploration and eventual colonisation. The promoters two years ago made a preliminary voyage there in the *Courier*, which for various reasons was not particularly fortunate, but from their past experience they now feel certain of success. The *Courier* then visited Astrolabe Gulf, on the north-east coast, and the natives were found very tractable and disposed to trade. Scented woods were met with in abundance, and tobacco and sugar were seen under cultivation. Mr. R. Mills, who was with this expedition, has brought away with him numerous views taken on the spot, which give a good idea of the natives and the aspect of the country.

LETTERS have been received at Vienna from the African traveller, John Freiherr von Müller. He intends to penetrate into the district south of Fazoglu and Fadazi, which hitherto have never been visited by any European. The geographical problems to be solved in these parts are the discovery of the bifurcation of the Sobat River (a tributary on the right bank of the White Nile), which was suspected by Karl Ritter, and also the discovery of the problematical Zamburu and Baringo lakes. The general circumstances in these districts do not justify the hope of success being oversanguine; yet Freiherr von Müller hopes safely to reach the Indian Ocean at Mombassa or Bagamoyo on his return journey.

ON Monday last week Dr. Ave-Lallemant delivered a lecture to the members of the German Athenæum, Mortimer Street, on the Orinocco River. The lecturer spoke mainly from personal observation, and the lecture was a highly interesting one.

A GENERAL Congress of German geographers, presided over by Dr. Nachtigal, met in Berlin last week. The second volume of Dr. Nachtigal's work on the Sahara and Sudan is expected shortly.

LIEUT. BOVE has just returned from the Argentine Republic, where he has been making arrangements for the projected expedition to the Antarctic regions. The Geographical Institute of the Argentine Republic has unanimously voted 2000 scudi for the enterprise. As soon as the Italian Government has arranged the diplomatic affairs of the expedition with the Argentine Republic, Lieut. Bove will return to Buenos Ayres.

HEFT VI. of *Petermann's Mittheilungen* commences with an interesting article on the Greatest Quantity of Rainfall in One Day, by Dr. H. Ziemer. Letters from Dr. Junker give interesting details concerning his sojourn in the Niam-Niam country, and an article, with map, on East Griqua Land and Pondo Land brings together recent information on these regions. Another article gives the leading results of some recent journeys in Arabia.

NO. 4 of the *Mittheilungen* of the Vienna Geographical Society contains an account, by Dr. Emin Bey, of his journeys in the Upper Nile Region; and Joh. Ritter Stef. v. Vilnovo has a long paper on the side-courses of rivers. In No. 5 Dr. Holub has a useful paper on the industrial aspect of Austrian exploration; Dr. Jettel writes on the scientific exploration of Bosnia and Herzegovina; and Lieut. Kreitner on the Ainos.

THE murder is reported of an Italian exploring party in the Danakil country. According to the latest advices from Aden, the party was composed of the traveller Giulietti, and an escort furnished by the commander of the vessel stationed at Assab. The party, whose object is stated to have been scientific and

commercial, left Beilul last April to explore the source of the Gualima. Four days distance from that town they were attacked and slain by the natives. Signor Giulietti was well known for the difficult journey he accomplished from Zeila Hazar. He was asked by the Geographical Society to explore the interior of the west coast of the Red Sea. At first a journey to Lake Aussa was contemplated, but obstacles arising, the plan was changed for an expedition into the Assab Gallas country.

AT the meeting of the Geographical Society on Monday last Capt. W. J. Gill, R.E., read some extracts from a long account of his explorations in Western Szechuen, which has lately been sent home by Mr. E. Colborne Baber, now Chinese Secretary of H.M. Legation at Peking. The extracts chosen dealt chiefly with the amusing side of Mr. Baber's journey, but the paper, nevertheless, contains abundance of solid information respecting the extreme west of China, and, as Lord Aberdare stated in his anniversary address, is considered by competent judges to be a noteworthy contribution to our knowledge of Asiatic geography. The most valuable part of the extracts read is probably that respecting the almost unknown Lolo country, in the neighbourhood of Ning-yüan-fu. Mr. Baber sent home copies of some pages of a Lolo manuscript, no specimen of which, we believe, has ever been seen in Europe before. These have been submitted to the well-known scholar, M. Terrien de la Couperie, who gave the meeting a brief account of the results of his examination of them. Mr. Baber's paper will be published by the Society, together with the valuable cartographical matter which accompanied it.

M. AND MADAME UJFALVY were to leave Simla for Kashmir, *viâ* Kangra, on June 6. From Kashmir they hope to penetrate into Thibet and Central Asia.

THE death is announced of Mr. Andrew Wilson, author of a well-known book of travel in the Himalayas, "The Abode of Snow."

SOLAR PHYSICS—CONNEXION BETWEEN SOLAR AND TERRESTRIAL PHENOMENA [1]

II.

IN my last Lecture I alluded to the complicated periodicity which sun-spots exhibit. It is right here to quote the remark of Prof. Stokes, that until we have applied to solar phenomena a sufficiently rigid analysis we are not certain that this apparent periodicity will bear all the marks of a true periodicity. It cannot however be denied that solar phenomena are roughly periodical, and this apparent periodicity has influenced observers in their attempts to search for a cause. There have been two schools of speculators in this interesting region, consisting of those who imagine a cause within the sun, and of those who imagine one without. The former *may* be right, but apparently they cannot advance our knowledge much. We know very little of the interior of the sun, and no one has yet ventured on any hypothesis regarding the *modus operandi* by which these strangely complicated and roughly periodical surface phenomena may be supposed to be produced by the internal action of the sun itself.

Those who maintain the hypothesis of an internal cause are apparently driven to it by the *à priori* unlikelihood of any cause operating from without. No doubt we have around the sun bodies, the motions of which are strictly periodical, such as planets, comets, and meteors, but they are relatively so small and so distant, that it seems difficult to regard them as capable of producing such vast phenomena as sun-spots.

There is however this difference between the two hypotheses—those who assert internal action cannot convert their views into a working hypothesis. On the other hand, those who look to external sources can take the most prominent planets, for instance, and endeavour to ascertain whether as a matter of fact the behaviour of the sun with regard to spots is apparently influenced by the relative positions of these. Attempts of this nature have been made by Wolf, Fritz, Loomis, Messrs. De La Rue, Stewart, and Loewy, and others. These attempts have been of two kinds. In the first place observers have tried whether there appear to be solar periods exactly coinciding with certain well-known planetary periods. By this means the following results have been obtained by the Kew observers (Messrs. De La Rue, Stewart, and Loewy):—

(1) An apparent maximum and minimum of spot energy approximately corresponding in time to the perihelion and aphelion of Mercury.

(2) An apparent maximum and minimum of spot energy approximately corresponding in time to the conjunction and opposition of Mercury and Jupiter.

(3) An apparent maximum and minimum of spot energy approximately corresponding in time to the conjunction and opposition of Venus and Jupiter.

(4) An apparent maximum and minimum of spot energy approximately corresponding in time to the conjunction and opposition of Venus and Mercury.

Mr. De La Rue and his colleagues make the following remarks upon these results:—

"There appears to be a certain amount of likeness between the march of the numbers in the four periods which we have investigated, but we desire to record this rather as a result brought out by a certain specified method of treating the material at our disposal, than as a fact from which we are at present prepared to draw conclusions. As the investigation of these and similar phenomena proceeds it may be hoped that much light will be thrown upon the causes of sun-spot periodicity."

I may here mention that within the last month I have, in conjunction with Mr. Dodgson, applied a method of detecting unknown inequalities with the view of seeing whether there are any indications of an unknown inequality in sun-spots having a period near that of Mercury, and I find there are indications of such an inequality having a period which does not differ from that of Mercury by more than about three-hundredths of a day. Besides the four periods above mentioned the Kew observers have, they think, detected evidence of a periodicity in the behaviour of spots with regard to increase or diminution depending apparently on the positions of the two nearer planets, Mercury and Venus. The law appears to be, that as a portion of the sun's surface is carried by rotation nearer to one of these two influential planets, there is a tendency for spots to become less and disappear, while on the other hand when it is carried away from the neighbourhood of one of these planets there is a tendency for spots to break out and increase.

The Kew observers regard this latter species of evidence as being well worthy of a more exhaustive discussion when the sun-spot records are more complete. I have already mentioned that the chief difficulty in attributing solar outbreaks to configurations of the planets is the comparative smallness and great distance of these bodies, so that when we reflect on the enormous amount of energy displayed in a sun-spot we cannot but have great difficulty in supposing that such vast phenomena can be caused by a planet like Venus, for instance, that is never as near to the sun as she is to the earth. But this difficulty depends very much on what we mean by the word "cause." If we mean that the planets cause sun-spots in the way in which the blow of a cannon-ball or the explosion of a shell causes a rent in a fortification, the hypothesis is certainly absurd. But if we only mean that the planets act the part of the man who pulls the trigger of the gun, the hypothesis may be unproved, but it is no longer absurd. For we have reason to believe that there may be great delicacy of construction in the sun's atmosphere, in virtue of which a small cause of this kind may produce a very great effect.

We may therefore believe it possible that planets may act in this way on the sun—the energy displayed in a spot being however not derived from the planets, but from the sun itself, just as the energy of a cannon-ball is not derived from the man who pulls the trigger, but from the explosion in the gun.

All this is chiefly historical, and it leads to a very interesting query. If there is such an action of a planet on the sun, must not this have a reaction? If the earth influences the sun, must not the sun simultaneously influence the earth? Perhaps so; nevertheless it is not an influence of this kind which I shall now bring before you. The sun is periodically stirred up—no matter how—and being stirred up there is an increase in the light and heat which are radiated to the earth. This affects the meteorology of the earth, and also its magnetism, after a method which, if we do not fully understand it now, we may ultimately expect to comprehend. It is this kind of influence, and not an occult action, of which I shall now bring the evidence before you

[1] Lecture in the Course on Solar Physics at South Kensington; delivered by Prof. Balfour Stewart, F.R.S., April 27. Continued from p. 117.

And first of all let me speak of the sun's influence on the magnetism of the earth.

Suppose that the chief observatories of the world have each a vault, and that in this vault a magnetic needle is delicately suspended. We may imagine the sun to be shut out altogether, the only light being that of a lamp which enables us to record, either photographically or otherwise, on a magnified scale any small oscillations of the needle. The vault may be supposed to be sufficiently deep down to be practically uninfluenced by the heat of the sun, so that it will exhibit no difference in temperature between noon and midnight. Finally there must be no iron or steel about the place, or anything which might affect the needle. Now under these circumstances you would naturally imagine that the needle would be perfectly stationary, always pointing in the same direction. Such, however, is not the case—it does not move very greatly, but nevertheless it does move, and its position depends on the hour of the day, or, in other words, upon the sun. The sun cannot heat the chamber in the least, nevertheless it can influence the magnet, and we might even tell in a rough way the hour of the day by noting the position of the needle. In this country the needle attains one extreme in its daily progress about five or six in the morning, and the other about one or two in the afternoon, and the difference in position of these two extremes is called the diurnal range of magnetic declination. Here then we have a magnetical phenomenon which depends upon the sun, and which does not take place simultaneously at the various observatories of the earth, inasmuch as the sun travels from east to west, so that when it is six in the morning at one place it may be midnight at another.

In the next place, we have abrupt magnetical changes analogous to the well-known abrupt meteorological changes, and bearing the appropriate name of magnetic storms.

A magnetic storm is not a mere local outbreak, but is felt simultaneously at all the various points of the earth's surface. The various needles in the various vaults of which we have now been speaking will all be affected at the same moment of time, and will be found to be oscillating backwards and forwards in a disturbed state. It thus appears that diurnal ranges and magnetic storms are two distinct phenomena.

To begin with diurnal declination ranges. These have, as their very name implies, a connexion with the hour of the day, and hence with the position of the sun. Again, in middle latitudes declination ranges are greatest in summer when the sun is most powerful, and least in winter. Lamont was the first to observe the signs of a long period inequality in the yearly means of the Munich diurnal declination ranges, and in 1852 Sir Edward Sabine succeeded in showing that this inequality followed that of sun-spots previously discovered by Schwabe, maximum ranges corresponding to years of maximum sun-spots, and minimum ranges to years of minimum sun-spots. In the same year Dr. Wolf and M. Gautier independently remarked the same coincidence.

But there is more than a mere general correspondence between these two phenomena, for it is believed that all inequalities of sun-spots, whether of long or short period, are accompanied by corresponding changes of declination range, a large range invariably accompanying a large number of spots. Perhaps I ought to say a large range following a large outbreak of spots, for the solar phenomenon leads the way and the magnetic change follows after it at a greater or less interval of time. I may add, likewise, that we have some evidence which leads us to suspect that particular states of declination range, like particular states of weather, have a motion from west to east, the magnetical weather moving faster than the meteorological. From a preliminary investigation which I have made, I even think there may ultimately be a possibility of forecasting meteorological weather by means of magnetic weather five or six days before. It will be noticed, that as far as declination range is concerned, we have no evidence of a direct magnetic action of the sun upon the earth, but we have, on the other hand, evidence that the magnetic effect, like the meteorological, lags behind the cause in such a way that we are inclined to attribute the magnetic as well as the meteorological phenomena to the heating effect of the sun's rays.

Let us next take magnetic storms. These, as we have seen, affect the various stations simultaneously, so that the magnetism of the earth appears to change as a whole, and in this respect they are very different from the ordinary diurnal oscillations of the needle. Nevertheless, equally with declination ranges, magnetic storms appear to depend on the state of the sun. In 1852 Sir Edward Sabine showed that in those years when there are most sun-spots there are most magnetic storms, while, on the other hand, years of minimum sun-spots correspond to a minimum number of such storms. The late John Allan Broun, an eminent magnetician, has given reasons for believing that the greater magnetic disturbances are apparently due to actions proceeding from particular meridians of the sun; this when verified will be a fact of the greatest importance.

Again, Prof. Loomis of America, from a discussion of 135 cases of magnetic disturbance, concludes that great disturbances of the earth's magnetism are accompanied by unusual disturbances of the sun's surface on the very day of the magnetic storm. It might at first sight be thought from this last observation that a magnetic storm is due to some direct magnetic influence propagated from the sun to the earth, and accompanying a rapid development of spots, the influence being thus very different from that which may be supposed to cause variations in the magnetic range. But I do not see that this result follows from Prof. Loomis' observations. There is, I think, evidence that the earth before a magnetic storm is in a critical magnetic state—out of relation to its surroundings—and hence a sudden solar outburst may be the immediate occasion of its starting off. But I fail to see any evidence that the influence received from the sun on such occasions is different in kind to that which affects magnetic ranges. For we know that magnetic storms occur most frequently about the equinoxes, or at those times when the sun is crossing the equator. Now were a magnetic storm produced by a magnetic influence immediately proceeding from the sun, it would be difficult to understand why there should be any marked reference in magnetic storms to certain months of the year.

When the magnetism of the earth is in a disturbed state this may of course be rendered visible by means of the oscillations of a delicately suspended magnetic needle. Nevertheless there are associated phenomena of a very conspicuous character which vividly impress us with the reality of the occurrence. One of these is the aurora—displays of which invariably accompany considerable magnetic storms, on which occasions they may be witnessed over a large portion of the globe.

Another of these is the earth currents which on such occasions affect all telegraphic lines connected with the earth. These earth currents are automatically registered at Greenwich by the Astronomer Royal, and their peculiarity is that during magnetic storms they are very violent, passing rapidly and frequently backwards and forwards between positive and negative.

We come now to the meteorological effects produced by the variable state of the sun's surface. More than ten years ago Mr. Baxendell of Manchester pointed out that the convection currents of the earth were apparently altered by the state of the sun's surface, and since that time this peculiar connexion between the sun and the earth has been investigated on an extensive scale by observers in various portions of the globe. Dr. Charles Meldrum of the Mauritius Observatory was one of the first pioneers in this important branch of inquiry. In 1872 he showed that the rainfalls at Mauritius, Adelaide, and Brisbane were greater generally in years of maximum than in years of minimum sun-spots. Shortly afterwards Mr. Lockyer showed that the same law held for the rainfalls at the Cape of Good Hope and Madras. Dr. Meldrum afterwards took twenty-two European observatories, and found that the law held in nineteen out of the twenty-two. It would however appear from the observations of Governor Rawson that at Barbadoes, and perhaps other places, the maximum rainfall does not coincide with the maximum sun-spot years. As locality has a very great influence upon rainfall, it might be supposed that by measuring the recorded depths of water in large rivers and lakes we should be able to integrate the rainfall over a large area, and thus avoid irregularities due to local influence. This too has been attempted. In 1873 Gustav Wex examined the recorded depths of water in the Elbe, Rhine, Oder, Danube, and Vistula for the six sun-spot periods from 1800 to 1867, and came to the conclusion that the years in which the maximum amount of water appeared in these rivers were years of maximum sun-spots, while the minimum amounts of water occurred during the years of minimum sun-spots.

In 1874 Mr. G. M. Dawson, in America, analysing the fluctuations of the great lakes, came to a similar conclusion. This leads me to a very practical and important part of the subject. In countries such as ours we often suffer from excessive rainfall, and are rarely incommoded by excessive heat; but in hot countries such as India a deficient rainfall means a dearth, or even a famine.

This has been brought prominently before us of late years by Dr. Hunter, Director-General of Statistics in India, who has shown that famines are most frequent at Madras about the years of minimum sun-spots—years which were likewise associated with a diminished rainfall.

In summing up the rainfall evidence we ought to bear in mind that the direction as well as the intensity of the earth's convection currents is no doubt altered by solar variability. And if we at the same time reflect how very local rainfall is, we cannot expect that the same rule regarding it should hold for all the various stations of the earth's surface. But on the whole there appears to me to be evidence that we have most rainfall during most sun-spots. Of course we know little or nothing of variations in the rainfall at sea.

I have already mentioned that the magnetic storms of the earth are most frequent during years of maximum sun-spots, and the very same thing may be said of wind-storms. Dr. Meldrum has found that there are more cyclones in the Indian Ocean in years when there are most sun-spots, and fewest cyclones in years when there are fewest sun-spots. M. Poey has proved a similar coincidence between the hurricanes of the West Indies and the years of maximum sun-spots, and I believe that a similar conclusion has been arrived at with regard to the typhoons of the Chinese seas.

In 1877 Mr. Henry Jeula of Lloyds and Dr. Hunter found that the percentage of casualties on the registered vessels of the United Kingdom was 17½ per cent. greater during the maximum two years than during the minimum two years in the common sun-spot cycle.

We may therefore imagine that the wind as well as the rain of the earth is most violent during years of maximum sun-spots.

We come now to the pressure of the air. If there were no sun the pressure of the air would ultimately distribute itself equally where it is now unequal. This inequality is no doubt caused by the sun, and we should expect it to be most pronounced when the sun has most power. It is also different in summer and winter. In summer we generally find a low barometer in the centres of great continents, and a high barometer over the sea; while during winter we have the converse of this, or a high barometer over continents and a low barometer at sea. I think it likely that the true relation between the variations of sun-spots and of barometric pressure will ultimately be discovered by means of the admirable weather-maps of the United States; meanwhile, however, especially in India, something has already been done in this direction.

If we regard the distribution of isobaric lines, that is to say of lines of equal barometric pressure, we shall find that the Indo-Malayan region is one which for the mean of the year has a barometric pressure probably below the average. Now during years of powerful solar action we might imagine that this peculiarity would be increased. But this is precisely what all the Indian observers have found for years with most sun-spots.

On the other hand Western Siberia in the winter season has a pressure decidedly above the average, and we should therefore imagine that during years of powerful solar action the winter pressure would be particularly high. This again is the state of things that Mr. Blanford has found in his discussion of the Russian stations to correspond with years of most sun-spots.

It therefore appears to me that the barometric evidence as far as it goes is favourable to the belief that years of maximum sun-spots are years of greatest solar power.

I come now to consider the question of temperature. Mr. Baxendell was the first to conclude that the distribution of temperature under different winds, like that of barometric pressure, is very sensibly influenced by the changes which take place in solar activity. In 1870 Prof. Piazzi Smyth published the results of observations made from 1837 to 1869 with thermometers sunk in the rock at the Royal Observatory, Edinburgh. He concluded from these that a heat wave occurs about every eleven years, its maximum slightly lagging behind the minimum of the sun-spot cycle. In 1871 Mr. E. J. Stone examined the temperature observations recorded during thirty years at the Cape of Good Hope, and came to the conclusion that the same cause which leads to an access of mean annual temperature at the Cape leads equally to a dissipation of sun-spots. Dr. W. Köppen in 1873 discussed at great length the connexion between sun-spots and terrestrial temperature, and found that in the tropics the maximum temperature occurs fully a year before the year of minimum sun-spots: while in the zones beyond the tropics it occurs two years after the minimum. The regularity and magnitude of the temperature wave is most strongly marked in the tropics.

The temperature evidence now given appears at first sight to be antagonistic to that derived from the other elements, both of magnetism and meteorology, and to lead us to conclude that the sun heats us most when there are fewest spots on its surface. This conclusion will not, however, be strengthened if we discuss the subject with greater minuteness. Scientifically, we may regard the earth as an engine, of which the sun is the furnace, the equatorial regions the boiler, and the polar regions the condenser. Now this engine works in the following manner. Hot air and vapour are carried along the upper regions of the atmosphere from the equator to the poles by means of the anti-trade winds, while in return the cold polar air is carried along the surface of the earth from the poles to the equator, forming what is known as the trade winds. Now whenever the sun's heat is most powerful, both trades and anti-trades should, I imagine, be most powerful likewise. But we live in the trades rather than in the anti-trades—in the surface currents, and not in the upper currents of the earth's atmosphere. When the sun is most powerful, therefore, is it not possible that we might have a particularly strong and cold polar current blowing about us? The same thing would happen in the case of a furnace-fire—the stronger the fire the more powerful the hot draught up the chimney—the more powerful also the cold draught from without along the floor of the room. It might thus follow that a man standing in the furnace room near the door might be chilled rather than heated when the furnace itself was roaring loudest. In fact temperature is a phenomenon due to many causes. Thus a low temperature may be due

(1) To a deficiency in solar power.
(2) To a clouded sky.
(3) To cold rain.
(4) To cold winds.
(5) To cold water and ice.
(6) To cold produced by evaporation.
(7) To cold produced by radiation.

Now Mr. Blanford, the Indian observer, has recently shown that a low temperature of the air and soil is accompanied in the stations which he has examined by a copious rainfall and by a large number of clouds. If therefore we regard a high rainfall as the concomitant of many sun-spots, we must not be surprised if this is sometimes accompanied with a low temperature, nor hastily conclude from this lowering of temperature that the sun is less rather than more powerful. Considerations of this nature have induced me to think that the true connexion between sun-spots and terrestrial temperature is more likely to be discovered by a study of short-period inequalities of sun-spots than by that of the eleven-year period in which there is time enough to change the whole convection system of the earth. I have accordingly discussed at some length two prominent sun-spot inequalities of short periods (about twenty-four days), and endeavoured to see in what way they affect the terrestrial temperature. From this it appears that a rapid increase of sun-spots is followed in a day or two by an increase of the diurnal temperature range at Toronto. Now an increase of diurnal temperature range surely denotes an increase of solar energy, and we are thus led to associate an increase of solar heat with a large development of spots.

I have thus brought before you a quantity of evidence, chiefly indirect, tending to prove that the sun's rays are most powerful when there are most spots. But you will naturally ask why I have not given you any direct evidence on this point. Is it not possible, you ask, to measure the direct heating effect of the sun's rays, so as to decide the question without further circumlocution? Now, strange to say, this has not been done.

We call an instrument that measures the sun's direct influence an actinometer, and I will now briefly allude to two such instruments, one for measuring the chemical effect of the sun's rays devised by Dr. Roscoe, and another for measuring the heating effect of the sun's rays, devised by myself. (The lecturer here described the mode of action of these actinometers.)

But the use of such instruments is rather a problem of the future than of the past. Hitherto it cannot be said that we have determined by actual observation whether the sun's rays are more powerful or less powerful at times of maximum sun-spots. I may, however, quote the actinometrical observations made in India at Mussooree and Dehra by Mr. J. B. N. Hennessey as confirming, so far as the evidence goes, the hypothesis of greater solar energy at maximum than at minimum epochs.

My trust is that for the future India will throw great light upon the problem we are now discussing. We have a distinguished meteorologist, General Strachey, as member of the Council of India, we have General Walker and the trigonometrical survey staff, and we have Mr. Blandford and the various meteorological and magnetic observers of India, and I am glad to think that neither solar nor actinometric observations are likely to be forgotten.

Let me now briefly recapitulate the conclusions we have come to.

In my first lecture I endeavoured to bring before you theoretical grounds for imagining that the sun is most powerful when there are most spots on its surface.

This has been supported by the evidence of a meteorological nature derived from these observations of rainfall, wind, barometric pressure, and temperature which have now been discussed, and likewise from such actinometric observations as have been made in Mussorree and Dehra. With regard to magnetical observations, we have the fact that diurnal declination ranges are largest in times of maximum sun-spots, and that on such occasions we have likewise a great number of magnetic storms, accompanied with earth currents and displays of the aurora. In fine we have most magnetic activity when there are most spots. There may perhaps be some doubt as to the exact method by which solar phenomena affect the magnetism of the earth, but we have already hypotheses from two distinguished physicists, the late Prof. Faraday and Prof. Stokes, while others have likewise been engaged in similar speculations.

Thus we may hope that eventually the truth will be attained. Meanwhile however we may conclude that the earth is most active both meteorologically and magnetically when there are most spots on the sun's surface. And if this be so, who will say that this is not a problem of great practical as well as of great theoretical importance?

ON GAS SUPPLY BOTH FOR HEATING AND ILLUMINATING PURPOSES [1]

WHEN, within the memory of living men, the gas-burner took the place of the time-honoured oil-lamp, the improvement, both as regards the brilliancy of the light and the convenience of the user, was so great that the ultimate condition of perfection appeared to have been reached. Nothing apparently remained for the engineer to effect but improvements in the details of the works and apparatus, so that this great boon of modern times might be utilised to the largest extent. It is only in recent years that much attention has been bestowed upon the utilisation of by-products, with a view of cheapening the cost of production of the gas, and that the consumer has become alive to the importance of having a gas of high illuminating power and free from nauseous constituents, such as bisulphide of carbon, thus providing a gentle stimulant for steady progress on the part of the gas-works manager.

This condition of steadiness and comfort has been somewhat rudely shaken by the introduction within the last year or two of the electric light, which, owing to its greater brilliancy and cheapness, threatens to do for gas what gas did for oil half a century before. The lighting of the City of London and of many public halls and works furnishes indisputable proof that the electric light is not an imaginary, but a real and formidable competitor to gas as an illuminant, and it is indeed time for gas engineers and managers to look seriously to their position with regard to this new rival; to decide whether to meet it as a foe, and contest its progress inch by inch, or to accept at once the new condition of things, conceding the ground that cannot reasonably be maintained, and to look about in search of such compensating fields as may be discovered for a continuation or extension of their labours.

For my own part I present myself before you both as a rival and as a friend; as a rival, because I am one of the promoters of electric illumination, and as a friend, because I have advocated and extended the use of gas for heating purposes during the last twenty years, and am by no means disposed to relinquish my advocacy of gas both as an illuminating and as a heating agent. Speaking as a gas engineer, I should be rather disposed to regard the electric light as a welcome incentive to fresh exertion, confidently anticipating achievements by the use of gas which would probably have been long postponed under the continued *régime* of a monopoly. Already we observe, both in our thoroughfares and in our apartments, gas-burners producing a brighter and more powerful light than was to be seen previously; and although gas will have to yield to the electric light the illumination of our lighthouses, halls, and great thoroughfares, it will be in a position, I believe, to hold its own as a domestic illuminant, owing to its great convenience of usage, and to the facility with which it can be subdivided and regulated. The loss which it is likely to sustain in large appliances as an illuminant would be more than compensated by its use as a heating agent, to which the attention of both the producer and the consumer has latterly been largely directed.

Having in the development of the regenerative gas-furnace had exceptional opportunities of recognising the many advantages of gaseous over solid fuel, I ventured, as early as 1863, to propose to the Town Council of Birmingham the establishment of works for the distribution of heating gas throughout the town, and it has occurred to me to take this opportunity (when the gas managers of Great Britain hold their annual meeting at the very place of my early proposal) to place before them the idea that then guided me, and to suggest a plan of operation for its realisation which at the present day will not, I venture to hope, be regarded by them as Utopian. The proposal of 1863 consisted in the establishment of separate mains for the distribution of heating gas, to be produced in vertical retorts, that might be shortly described as Appold's coke oven heated by means of "producer" gas and "regenerators." The heat of the retorts was to be increased beyond the ordinary limit in order to produce a coke suitable for locomotive and other purposes; and the gas produced being possessed of less illuminating but of the same heating power, and being, with a view to cheapness, less thoroughly purified than ordinary retort gas, was to be distributed through the town as a heating agent, to be applied to the small boilers and furnaces of the numerous little factories peculiar to the district, as well as for domestic purposes. The Corporation applied for an Act of Parliament, but did not succeed in obtaining it, owing to the opposition of the existing gas companies, who pledged themselves to carry out such an undertaking if found feasible by them. I am ready to admit that at the time in question the success of the undertaking would have involved considerable practical difficulty, but I feel confident that the modified plan which it is my present object to bring before you would reduce those difficulties to a minimum, and open out on the other hand a new field of vast proportions for the enterprise and energy of those interested in gas-works, and of great benefit to the public.

The gas-retort would be the same as at present, and the only change I would advocate in the benches is the use of the regenerative gas-furnace. This was first successfully introduced by me at the Paris Gas-works in 1863, and has since found favour with the managers of gas-works abroad and in this country. The advantages that have been proved in favour of this mode of heating are economy of fuel, greater durability of retorts, owing to the more perfect distribution of heat, the introduction of an additional retort in each bed in the position previously occupied by the fire-grate, and above all, a more rapid distillation of the coal, resulting in charges of four hours each, whereas six hours are necessary under the ordinary mode of firing. The additional suggestion I have now to make consists in providing over each bench of retorts two collecting pipes, the one being set aside for illuminating, and the other for a separate service of heating gas. I shall be able to prove to you from unimpeachable evidence that the gas coming from a retort varies very greatly in its character during progressive periods of the charge; that during the first quarter of an hour after closing the retort, the gas given off consists principally of marsh gas (CH_4) and other occluded gases and vapours, which are of little or no use for illuminating purposes; from the end of the first quarter of an hour, for a period of two hours, rich hydrocarbons, such as acetylene (C_2H_2) and olefiant gas (C_2H_4) are given off; whereas the gases passing away after this consist for the most part again of marsh gas possessing low illuminating power.

M. Ellissen, the late chief of the experimental department of the Paris Gas-works, and actual President of the French Society of Gas Engineers, has favoured me with the result of a most interesting series of experiments, which he carried out in connection with the late M. Regnault, the eminent physicist, some years ago, the object of the experiments being to discover the proper period of time to be allowed for each charge.

The results of these experiments are given in a diagram showing in a striking manner that although the average illu-

[1] Paper read before the British Association of Gas Managers at Birmingham, June 14, by C. W. Siemens, D.C.L., F.R.S., Civil Engineer.

minative power produced by the distillation of the coal did not exceed 1·35 Carcel burners, or 13·5 standard candles, according to our English mode of measurement, the gas given off from the end of the first quarter of an hour, during a period of two hours, possessed an illuminating power of 1·616 Carcel burners, or 16·16 standard candles. According to the figures given in the valuable experiments of M. Ellissen, it appears that nearly two-thirds of the total production of gas takes place in the above period, whilst the remaining third is distilled during the first quarter of an hour and the last hour and three quarters. It hence follows that by changing the direction of the flow of gas at the periods indicated, allowing the first results of distillation to flow into the heating gas-main, then for two consecutive hours into the illuminating gas main, and for the remainder of the period again into the heating gas-main, one-third volume of heating and two-thirds of illuminating gas would be obtained, with this important difference, that the illuminating gas would be of 16·16 instead of 13·5 candle power, and that the heating gas, although possessed of an illuminating power of only 11·05, would be preferable to the mixed gas for heating purposes, in being less liable to deposit soot in its combustion upon heat-absorbing surfaces, and in giving, weight for weight, a calorific power superior to olefiant gas.

These experiments not having been made for the particular objects I have in view, no account was taken of the quantity or quality of the gas coming from the retort during the first quarter of an hour. Judging by the nature of the curves given by M. Ellissen, it is reasonable to suppose that during the first quarter of an hour a considerable quantity of gas of very inferior illuminating power is given off, which, if taken into account, would still further improve the result given in favour of separating the illuminating from the heating gases.

It will be observed that although the candle-power of the illuminating gas would be raised to only 16·25 if two-thirds of the gas were set apart for this purpose, *i.e.* after the first 25 minutes of distillation up to 2h. 35m. from the commencement of the charge, a gas equal to 18·04 candles would be obtained if the proportionate quantity of heating and illuminating gas were reversed, which might be effected by continuing the distillation for illuminating purposes from 0·25m. to 1h. 27m. after the commencement of the charge, whilst if equal quantities of heating and illuminating gas were produced, which would result from allowing the illuminating gas to flow into its receiver from 0·25m. to 2h. 0m., the candle-power of this portion of the gas would be raised to 16·78 candles, as shown in the figures given below.

	Cubic Feet.		Candle-power.
Total gas produced from ton of coal...	10573·20	of	13·50

	Illuminating Gas.		Heating Gas.
	Cubic Feet.	Candle-power.	Cubic Feet.
Illuminating gas passing into its main 25 minutes after commencement of charge:—			
If two-thirds the quantity used for illumination from 0h. 25m. to 2h. 35m.	7048·8	16·25	3524·4
If half the quantity used for illumination from 0h. 25m. to 2h. 0m.... ...	5286·6	16·78	5286·6
If one-third the quantity used for illumination from 0h. 25m. to 1h. 27m...	3524·4	18·04	7048·8

These important results are borne out by a series of photographic observations which were made some years ago by Mr. Sugg, which Mr. Sugg has further supplemented verbally in stating that the average illuminative power obtained by the distillation of Newcastle coal might be taken at 14 candle-power, whilst two-thirds of the quantity, if separated in the manner I propose, would produce an average of 16 candles.

The working out of this plan would involve the mechanical operation of changing the direction of the gas coming from each bench of retorts at the proper periods of the charge; this could be accomplished by means of a simple reversing valve similar to that applied for many years in reversing the current of the regenerative gas-furnace, and a sand-glass may be placed in front of each bench of retorts for the guidance of the man in charge as to the time when the reversal should be made. In order to distribute the two gases a double set of gas-mains would certainly be required; but these exist already in the principal thoroughfares of many of our great towns, where at one period or another competing gas companies have been established, and it would not be difficult, I think, to utilise these services for the separate supply of illuminating and heating gas, the latter being taken into such houses and establishments only where asked for by the occupiers.

The public could well afford to pay an increased price for a gas of greatly increased illuminating power, and the increase of revenue thus produced would enable the gas companies to supply heating gas at a proportionately reduced rate. It would not be necessary to employ upon the heating gas the same expense and trouble in purification as is required for illuminating gas, because the products of combustion of the heating gas would not as a rule enter the apartments, but be conducted into the atmosphere through the ordinary chimneys. Heating the retorts by means of the regenerative gas-furnace would, as already indicated, lead to an increased production of gas from each bench of retorts, and thus compensate for the reduced amount of illuminating gas in each operation. The heating gas might without inconvenience be sent through the pipes at a greater pressure than the illuminating gas, in order to make a given plant of mains transmit an increased quantity.

The question may fairly be asked whether a demand would be likely to arise for heating gas similar in amount to that for illuminating gas, and I may state that I am decidedly of opinion that although at the present moment the amount of gas supplied for illuminating purposes exceeds that for heating, the diminution in price of the latter would very soon indeed reverse these proportions. Already gas is used in rapidly increasing quantities for kitcheners, for the working of gas-engines, and for fire-grates. As regards the latter application, I may here mention that an arrangement for using gas and coke jointly in an open fire-place, combined with a simple contrivance (with a view of effecting the combustion of the gas by heated air), has found favour with many of the leading grate builders and with the public; although this arrangement was suggested by me only last winter, several hundred of these grates are already in use in London, Manchester, Leeds, Glasgow, and other towns, showing how fully alive the public are at the present time to that great crying evil, "the smoke nuisance."

It may be as well for me to mention here, that neither the regenerative gas-coke fire-grate just alluded to, nor the plan I here advocate of separating the produce of gas-retorts, has been made by me subject-matter of letters patent, my time being already too much occupied in other directions to give that amount of constant attention to these subjects which the working of a patent necessitates.

As regards the use of illuminating gas, I have one more suggestion to make, which I feel confident will be viewed by you not without interest. The illuminating effect produced in a gas flame depends partly upon the amount of carbon developed in the solid condition in the body of the flame, and partly upon the temperature to which these particles are heated in the act of combustion. Having already shown how by separation a gas of greater luminosity may be supplied, it remains to be seen how the temperature of combustion may be raised. This may be effected to an extent that seems surprising by certain mechanical arrangements, whereby a portion of the waste heat produced by the flame itself is rendered available to heat the gas and air sustaining the combustion of the flame, say to 600° F., or even beyond that point.

The arrangement I have adopted for this purpose is represented on the sectional diagram, and I have also the pleasure to place the burner itself before you to enable you to test its efficiency by actual trial. The burner is of the ordinary Argand type, mounted in a small cylindrical chamber of sheet copper connected with a vertical rod of copper projecting up and through the centre of the burner, and terminating in a cup-like extension at a point about four inches above the gas orifices, or on a level with the top of the flame. A small mass of fire-clay fills the cup, projecting upwards from it in a rounded and pointed form. The copper vessel surrounding the burner is contracted at its upper extremity with a view of directing a current of air against the gas jets on the burner, and on its circumference it is perforated for the admission of atmospheric air. The bottom surface is formed of a perforated disk covered with wire gauze, and wire gauze also surrounds the circumference of the perforated cylinder. The external air is heated in passing through these "regenerative" surfaces, and the flame is thus fed with air, heated to the point above indicated, which by more elaborate arrangements might be raised to a still higher degree. The ball of fire-clay in the centre of the burner, which is heated to red-

ness, serves the useful purpose of completing the combustion of the gas, and thus diminishes the liability to blackening of the ceiling.

This arrangement for transferring the heat from the tip of the flame to the air supporting its combustion is applicable also to an open batswing burner; but I have not yet had time to ascertain accurately the amount of increase of luminosity that may be realised with this class of burner.

I may here mention that another solution of the problem of heating the incoming air by the waste heat of the products of combustion has lately been brought under public notice by my brother, Frederick Siemens, which differs essentially from the plan I have suggested, inasmuch as he draws the flame downwards through heating apparatus, and thence into a chimney. Experiments made officially and with great care have proved that by these methods the luminous effect of gas can be practically doubled. In practice both these methods of intensifying a gas-flame will probably find independent application according to circumstances, the cause of increased luminous effect being in both cases the same.

From a purely theoretical point of view it can be shown that of the calorie energy developed in the combustion of gas, a proportion probably not exceeding 1 per cent. is really utilised in the production of luminous rays, and that even in the electric light nine tenths of the energy set up in the arc is dispersed in the form of heat, and one-tenth only is utilised in the form of luminous rays. It would lead us too far here to go into the particulars of these calculations, but it is important to call attention to them, in order to show the large margin for practical improvements still before us.

By the combined employment of the process of separation of the illuminating from the heating gas with the arrangement for intensifying the luminosity of the gas-flame, the total luminous effect produced by a given consumption of coal gas may be nearly tripled, thus showing that the deleterious effects now appertaining to gas illumination are not inseparably connected with its use.

My principal object in preparing this communication has been to call your attention generally to the important question of an improved gas illumination, and more particularly to the subject of a separate supply for heating gas, which, if carried into effect, would lead, I am convinced, to beneficial results, the importance of which, both to gas companies and to the public, it would be difficult to over-estimate.

APPENDIX

Paris, June 4, 1881

DEAR SIR,—I send you herewith the result of my experiments, together with tables and curves; the very ingenious proposal that you have made would permit such a division of the total production of gas, that two-thirds could be employed for lighting and one-third for heating purposes, resulting in splendid illumination and much more rational heating.

I am, dear sir, &c.,

Dr. C. William Siemens A. ELLISSEN

Experiments on the Variation of Production of Gas, and of its Illuminating Power at different Periods of the Distillation

Tables I. and II. contain the results of experiments made in a bench of seven retorts of the type of the Compagnie Parisienne, each retort being charged respectively with 100, 110, and 120 kilogrammes (220, 242, 264 pounds).

Table I. corresponds to a distillation of 4 hours.

Table II. corresponds to a distillation of 4h. 48m.

The period of distillation has been divided into intervals of fifteen minutes, and the results recorded on each horizontal line refer to the gas produced during the quarter ending the time mentioned on each line.

In each of the two tables the case of a charge of 110 kilos. (242 lbs.) has been chosen as the standard, and the results have been graphically represented by means of two curves, one in red for the gas produced, and the other in blue for the illuminating power.

The line of abscissæ being divided into equal parts, each representing fifteen minutes, each ordinate of the red curve gives the gas produced during the preceding quarter of an hour, and the corresponding ordinate of the blue curve indicates the illuminating power of this same gas.

The production of the gas been further divided into two portions, the one destined for illumination, and the other for heating and motive power.

The gas produced during the first quarter of an hour is generally of low illuminating power, and varies besides with the hygrometric condition of the coal; it has, in the following calculation, been accordingly classed with the heating gas, and the gas produced during the interval from 0h. 15m. to 2h. 15m. of the working has alone been reserved for illuminating purposes.

Distillation in four hours. Charge of 110 *kilos.* (242 *lbs.*)

I. Gas produced per 100 kilos. of coal distilled—

	Cubic metres.	Per ton, cubic feet.
1. From 0h. 15m. to 2h. 15m. ...	18·062	... 6502·32
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 0m. ...	11·308	... 4070·88
Total	29·370	... 10573·20

II. Gas produced per 100 cubic metres obtained—

	Cubic metres.
1. From 0h. 15m. to 2h. 15m. ...	61·502
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 0m. ...	38·498
Total	100·000

III. Mean illuminating power of the produced gas—

	Litres.	In English standard candles.
1. From 0h. 15m. to 2h. 15m. ...	87·7	... 16·16
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 0m. ...	128·2	... 11·05
Mean of the total mixed gas as per calculation	103·3	
Illuminating power of mixed gas as per direct trial...	105·7	... 13·50

Distillation in 4 *hours* 48 *minutes. Charge of* 110 *kilos.* (242 *lbs.*)

I. Gas produced per 100 kilos. of coal distilled—

	Cubic metres.	Per ton, cubic feet.
1. From 0h. 15m. to 2h. 15m. ...	20·388	... 7339·68
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 48m. ...	9·741	... 3506·76
Total	30·129	... 10846·44

II. Gas produced per 100 cubic metres obtained—

	Cubic metres.
1. From 0h. 15m. to 2h. 15m. ...	67·673
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 48m. ...	32·327
Total	100·000

III. Mean illuminating power of the produced gas—

	Litres.	In English standard candles.
1. From 0h. 15m. to 2h. 15m. ...	101·1	... 14·02
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 48m. ...	132·4	... 10·07
Mean of the total mixed gas	111·2	... 12·77

It is not proposed to stop at the results obtained by distillation in 4h. 48m., that is five charges per twenty-four hours; experience has proved that the best conditions of working are found in the use of active charges rapidly distilled by raising the temperature of the furnaces.

From these experiments it results that it would be possible to divide the products of distillation of coal into illuminating gas, and gas for heating purposes and motive power.

Thus in place of producing, as is generally done, by means of a distillation of four hours and 110 kilos. (242 lbs.) per retort, a mean result per 100 kilos of coal distilled, of 30 cubic metres of normal gas, which corresponds to an expenditure of 105 litres, to produce the light of a Carcel burner consuming 42 grammes of oil per hour, there may be produced:—

1. About 18·5 cubic metres of illuminating gas of an illuminative power of 87 litres; and

2. About 11·5 cubic metres of heating and motive-power gas of an illuminative power of 128 litres; or per 100 cubic metres

of gas produced, 61·50 cubic metres of illuminating gas, and 38·50 cubic metres of heating and motive-power gas.

This result would be obtained by receiving into separate reservoirs the gas produced during the first fifteen minutes, and during the last 1h. 45m. of the distillation, and in reserving for illuminating purposes the gas made in the interval of 0h. 15m. to 2h. 15m. of the charge from the commencement of the distillation.

STORAGE OF ELECTRIC ENERGY

THE following correspondence on this subject has appeared in the *Times*. By help of this and the communication in our issue of to-day from Sir W. Thomson, the reader will be able to understand the present position of this important question.

THE marvellous "box of electricity" described in a letter to you, which was published in the *Times* of May 16, has been subjected to a variety of trials and measurements in my laboratory for now three weeks, and I think it may interest your readers to learn that the results show your correspondent to have been by no means too enthusiastic as to its great practical value. I am continuing my experiments to learn the behaviour of the Faure battery in varied circumstances, and to do what I can towards finding the best way of arranging it for the different kinds of service to which it is to be applied. At the request of the Conseil d'Administration of the Société de la Force et la Lumière, I have gladly undertaken this work, because the subject is one in which I feel intensely interested, seeing in it a realisation of the most ardently and unceasingly felt scientific aspiration of my life—an aspiration which I scarcely dared to expect or to hope to live to see realised.

The problem of converting energy into a preservable and storable form, and of laying it up in store conveniently for allowing it to be used at any time when wanted, is one of the most interesting and important in the whole range of science. It is solved on a small scale in winding up a watch, in drawing a bow, in compressing air into the receiver of an air-gun or of a Whitehead torpedo, in winding up the weights of a clock or other machine driven by weights, and in pumping up water to a height by a windmill (or otherwise, as in Sir William Armstrong's hydraulic accumulator) for the purpose of using it afterwards to do work by a waterwheel or water pressure on a piston. It is solved on a large scale by the application of burning fuel to smelt zinc, to be afterwards used to give electric light or to drive an electro-magnetic engine by becoming, as it were unsmelted in a voltaic battery. Ever since Joule, forty years ago, founded the thermodynamic theory of the voltaic battery and the electro-magnetic engine, the idea of applying the engine to work the battery backwards and thus restore the chemical energy to the materials so that they may again act voltaically, and again and again, has been familiar in science. But with all ordinary forms of voltaic battery the realisation of the idea to any purpose seemed hopelessly distant. By Planté's admirable discovery of the lead and peroxide of lead voltaic battery, alluded to by your correspondent, an important advance towards the desired object was made twenty years ago; and now by M. Faure's improvement practical fruition is attained.

The "million of foot pounds" kept in the box during its seventy-two hours' journey from Paris to Glasgow was no exaggeration. One of the four cells, after being discharged, was recharged again by my own laboratory battery, and then left to itself absolutely undisturbed for ten days. After that it yielded to me 260,000 foot pounds (or a little more than a quarter of a million). This not only confirms M. Reynier's measurements, on the faith of which your correspondent's statement was made; it seems further to show that the waste of the stored energy by time is not great, and that for days or weeks, at all events, it may not be of practical moment. This, however, is a question which can only be answered by careful observations and measurements carried on for a much longer time than I have hitherto had for investigating the Faure battery. I have already ascertained enough regarding its qualities to make it quite certain that it solves the problem of storing electric energy in a manner and on a scale useful for many important practical applications. It has already had in this country one interesting application, of the smallest in respect to dynamical energy used, but not of the smallest in respect to beneficence, of all that may be expected of it. A few days ago my colleague, Prof. George Buchanan, carried away from my laboratory one of the lead cells (weighing about 18 lbs.) in his carriage, and by it ignited the thick platinum wire of a galvanic *écraseur* and bloodlessly removed a nævoid tumour from the tongue of a young boy in about a minute of time. The operation would have occupied over ten minutes if performed by the ordinary chain *écraseur*, as it must have been had the Faure cell not been available, because in the circumstances the surgical electrician, with his pharaphernalia of voltaic battery to be set up beforehand, would not have been practically admissible.

The largest useful application waiting just now for the Faure battery—and it is to be hoped that the very *minimum* of time will be allowed to pass till the battery is supplied for this application—is to do for the electric light what a water cistern in a house does for an inconstant water supply. A little battery of seven of the boxes described by your correspondent suffices to give the incandescence in Swan or Edison lights to the extent of 100 candles for six hours, without any perceptible diminution of brilliancy. Thus, instead of needing a gas engine or steam engine to be kept at work as long as the light is wanted, with the liability of the light failing at any moment through the slipping of a belt—an accident of too frequent occurrence—or any other breakdown or stoppage of the machinery, and instead of the wasteful inactivity during the hours of day or night when the light is not required, the engine may be kept going all day and stopped at night, or it may be kept going day and night, which will undoubtedly be the most economical plan when the electric light comes into general enough use. The Faure accumulator, always kept charged from the engine by the house supply wire, with a proper automatic stop to check the supply when the accumulator is full, will be always ready at any hour of the day or night to give whatever light is required. Precisely the same advantages in respect of force will be gained by the accumulator when the electric town supply is, as it surely will be before many years pass, regularly used for turning lathes and other machinery in workshops and sewing-machines in private houses.

Another very important application of the accumulator is for the electric lighting of steam-ships. A dynamo-electric machine of very moderate magnitude and expense, driven by a belt from a drum on the main shaft, working through the twenty-four hours, will keep a Faure accumulator full, and thus, notwithstanding irregularities of the speed of the engine at sea or occasional stoppages, the supply of electricity will always be ready to feed Swan or Edison lamps in the engine-room and cabins, or arc lights for mast-head and red and green side lamps, with more certainty and regularity than have yet been achieved in the gas supply for any house on *terra firma*.

I must apologise for trespassing so largely on your space. My apology is that the subject is exciting great interest among the public, and that even so slight an instalment of information and suggestions as I venture to offer in this letter may be acceptable to some of your readers. WILLIAM THOMSON.

The University, Glasgow, June 6.

ALTHOUGH agreeing with every word of Sir William Thomson's letter in the *Times* of to-day, and entirely sympathising with his enthusiasm as regards the marvellous box of electricity, still I feel that it would have been desirable if in pointing out the importance of this new discovery Sir William Thomson had guarded against a very probable misconstruction of the purport of his letter.

The means of storing and re-storing mechanical energy form the aspiration not only of Sir William, but of every educated mechanic. It is, however, a question of degree—of the amount of energy stored as compared with the weight of the reservoir, the standard of comparison being coal and corn. Looked at in this way one cannot but ask whether, if this form of storage is to be the realisation of our aspirations, it is not completely disappointing. Large numbers are apt to create a wrong impression until we inquire what is the unit. Eleven million foot pounds of energy is what is stored in 1 lb. of ordinary coal. So that in this box, weighing 75 lb., there was just as much energy as in 1¼ oz. of coal, which might have been brought from Paris or anywhere else in a waistcoat pocket, or have been sent by letter.

When we come to the question of the actual conveyance of energy for mechanical purposes, this view is of fundamental importance. The weight of the same amount of energy in the new form is 800 times greater than the equivalent amount of coal; and as a matter of economy, supposing that energy in this

form might be had at a certain spot and no capital were required for its conversion or storage, and that the energy were directly applicable it could not be carried ten miles—that is to say, such energy cannot be economically useful ten miles from its source, although coal had to be carried 100 miles to the spot. This limit, in truth, falls far short of what has been already attained by other means. By wire ropes and by compressed air or steam energy may be economically transmitted from ten to twenty miles. So that if this is the utmost of what is to be done by means of the storage of electricity this discovery adds another door to those which are hopelessly closed against the possibility of finding in Niagara or other water power a substitute for our coal, even when the object is motive power, and much more for that purpose for which five-sixths of our coal is used—the production of heat.

It is very important that the people of this country should not shut their eyes to the fact that, so far from there being a greater prospect of the solution of the problem than when, about twenty years ago, Prof. Jevons raised the alarm, the prospect is now much smaller. In the meantime the capabilities of steel ropes, fluids in pipes, and electricity along conductors have been not only investigated, but practically tested, and found altogether wanting. And now it would seem that the storage of electricity must be added to the list. OSBORNE REYNOLDS

Owens College, June 9

YOUR leading article in the *Times* of yesterday, on the storage of electricity, alludes to my having spoken of Niagara as the natural and proper chief motor for the whole of the North American Continent. I value the allusion too much to let it pass without pointing out that the credit of originating the idea and teaching how it is to be practically realised by the electric transmission of energy is due to Mr. C. W. Siemens, who spoke first, I believe, on the subject in his presidential address to the Iron and Steel Institute in March, 1877. I myself spoke on the subject in support of Mr. Siemens's views at the Institution of Civil Engineers a year later. In May, 1879, in answer to questions put to me by the Select Committee of the House of Commons on Electric Lighting, I gave an estimate of the quantity of copper conductor that would be suitable for the economical transmission of power by electricity to any stated distance; and, taking Niagara as example, I pointed out that, under practically realisable conditions of intensity, a copper wire of half an inch diameter would suffice to take 26,250 horse-power from water-wheels driven by the Fall, and (losing only 20 per cent. on the way) to yield 21,000 horse-power at a distance of 300 British statute miles; the prime cost of the copper amounting to 60,000*l.*, or less than 3*l.* per horse-power actually yielded at the distant station. WILLIAM THOMSON

The University, Glasgow, June 9

IF you do me the honour to publish a letter which I wrote to you yesterday regarding the electric transmission of energy it will be seen that I thoroughly sympathise with Prof. Osborne Reynolds in his aspirations for the utilisation of Niagara as a motor, but that neither Mr. Siemens nor I agree with him in the conclusion which he asserts in his letter to you, published in the *Times* of to-day, that electricity has been tried and found wanting as a means for attaining such objects. The *transmission* of power was not the subject of my letter to you published in the *Times* of the 9th inst., and Prof. Reynolds' disappointment with M. Faure's practical realisation of electric *storage*, because it does not provide a method of *porterage* superior to conduction through a wire, is like being disappointed with an invention of improvements in water cans and water reservoirs because the best that can be done in the way of movable water cans and fixed water reservoirs will never let the water-carrier supersede water-pipes wherever water-pipes can be laid.

The 1½ oz. of coal cited by Prof. Osborne Reynolds as containing a million of foot-pounds stored in it is no analogy to the Faure accumulator containing the same amount of energy. The accumulator can be re-charged with energy when it is exhausted, and the fresh store drawn upon when needed, without losing more than 10 or 15 per cent. with arrangements suited for practical purposes. If coal could be unburned—that is to say, if carbon could be extracted from carbonic acid by any economic process of chemical or electric action, as it is in nature by the growth of plants drawing on sunlight for the requisite energy—the result would be analogous to what is done in Faure's accumulator. WILLIAM THOMSON

The University, Glasgow, June 11

DR. MIKLUCHO MACLAY'S ANTHROPOLOGICAL AND ANATOMICAL RESEARCHES IN MELANESIA AND AUSTRALIA [1]

AFTER I had left Sydney in March, 1879, I visited the following islands: New Caledonia, Lifu; of the New Hebrides: Tana, Vate, Tongoa, Mai, Epi, Ambrim, Malo, Vanua Lava; the Admiralty Islands; the groups—Luh (or Hermit), Ninigo (Echiquier), Trobriant, the Solomon Islands, the islands at the south-east end of New Guinea, and the islands of Torres Straits.[2]

Only a very few of the results of the journey can be comprehended in a short *résumé*, of these the first two of the following appear to me to be the most important:—1. Many islands of Melanesia[3] (especially some of the islands of the New Hebrides, of the Solomon Group, of the Louisiades, New Ireland, &c., &c.) possess a well-marked brachycephalic population (the breadth-index of many heads exceeds eighty, and sometimes even eighty-five), which circumstance is assuredly not ascribable to a mixture with another race, and proves that brachycephalism has a much wider range in Melanesia than has been hitherto supposed. This is a result of numerous careful measurements of heads and skulls[4] of the aboriginals of different islands of Melanesia. 2. Although in some villages of the southern coast of New Guinea there is noticeable a Polynesian admixture, yet this circumstance by no means permits of the aboriginals of the south-eastern peninsula (who are a branch of the Melanesian stock) being called a "yellow Malayan race," as has been frequently done of late years. 3. An acquaintance with the languages of the group Luh (or Hermit) and the dialects of the northern coast of the large island of the Admiralty Group, as well as the native traditions of the former, has shown that the population of the group Luh emigrated from the Admiralty Islands. Further acquaintance with the natives of Luh proved that there is among them a Polynesian admixture, which has resulted from the carrying off of the women of the group Ninigo, and from a frequent intercourse with the inhabitants (also a Melano-Polynesian race) of the smaller group Kaniet or Kanies (or Anchorites). My stay among the inhabitants of the Admiralty Islands has afforded me a glimpse of many interesting customs of the islands; but an account of these observations and researches cannot be condensed within the compass of a few sentences. To this series of results belong also the observations which I never neglected to make during the journey in Melanesia whenever the opportunity presented itself—especially observations on their customs, such as the deformation of the head, tattooing, perforation of the septum narium, alæ nasi, lobes and margins of the ears. I have also succeeded in making further observations, and obtaining more information, on the macrodontism in the Admiralty and Luh islands.

On my way back from the islands of Torres Straits I visited Brisbane, where I at first only intended to remain a few days. Here however a favourable opportunity presented itself of acquiring some interesting anatomical material for my anthropological researches, which circumstance induced me to prolong my stay for several months. I found, namely, that there was a possibility of continuing my researches on the comparative anatomy of the brain of the different varieties of the genus *homo*, which were commenced in 1873 in Batavia and resumed in Sydney in 1878. Although the material in question consisted only of three brains, yet I find that this new contribution to our knowledge of race-anatomy supports the view which I may briefly summarise as follows:—The investigation of the brains of representatives of different races of men shows that there occur peculiarities of by no means trifling import, which one cannot regard as individual variations. To this category belong differences in the development of the corpus callosum of the pons varolii, of the cerebellum; differences in the volume of the cranial nerves, and so forth; also the arrangement of the convolutions of the cerebrum is different, and I believe that in

[1] From a paper read before the Linnean Society of New South Wales February 23, 1881, by Dr. N. De Miklucho-Maclay. Revised and transmitted by the author.

[2] A more detailed account of the route, of the time spent at the different places, with sketch-maps of the routes and other details, will be found in my communication to the Imperial Russian Geographical Society, in the Jsweslija of the Society.

[3] By the name "Melanesians" I designate exclusively the frizzly-haired inhabitants of the South Sea Islands.

[4] In order to eliminate any doubt as to the correctness of the cranial measurements on living individuals, I have not neglected to collect a considerable number of undoubtedly authentic skulls from New Caledonia, New Guinea, the Admiraltys, Ninigo, and Solomon Islands.

course of time it will probably be discovered that there exist certain definite types of cerebral convolutions corresponding to the principal varieties of mankind. In order to discover those types much material will require to be conscientiously examined; and I hope that my investigation will induce other anatomists to work in this direction to prove or to disprove this statement, which in the present state of our knowledge can only be more or less hypothetical.

On my way from Thursday Island I let slip no opportunity of examining, measuring, and photographing the remnant of the Australian aboriginals; and hearing it stated in various quarters that there were living in the interior of Queensland certain natives described as devoid of hair, I thought the problem of a possible occurrence of a hairless stock among the aboriginals worthy of a personal investigation. I have written to Prof. Virchow of Berlin at length concerning my examination of this hairless family, which I found at Gulnarber Station, near St. George, on the Balonne River. This was made considerably easier for me by the kind assistance of Mr. G. M. Kirk of Gulnarber Station. As regards this instance of natural, and in this case hereditary *atrichia universalis* among the Australian aboriginals, I will only remark that it forms an interesting antithesis to the well-known cases of excessive hypertrichosis.

With a view of pursuing comparative anatomical researches on the brain of the marsupials, I went to Pikedale, near Stanthorpe, where I succeeded during a stay of almost six weeks in acquiring for my cerebral investigations some material which is almost impossible to obtain in the cities, such as Brisbane or Sydney, and which, as I have learnt by my own experience, cannot be obtained even in the bush with great ease and quickness. I succeeded, however, in obtaining a number of brains of some species of the genera—Macropus, Osphranter, Halmaturus, Petrogale, Phascolarctos, as well as a few brains of Ornithorhynchus and Echidna.

At the end of December last year, still availing myself of the kind hospitality of Mr. Donald Gunn, I went on to his other station, Clairvaul, near Glen Innes, with the intention of collecting some fossils, and without great trouble I got a series of interesting remains of *Diprotodon Australis, Nototherium Mitchellii, Phascolomys gigas, Macropus titan*, &c., &c.

When I received in May, 1880, in Thursday Island, a letter from my friend Mr. William Haswell, informing me that the Zoological Station in Sydney was not established, I determined not to leave Australia before the scheme had been carried out. Detained in Queensland by the work already referred to, I only arrived in Sydney in January of this year, and now, after a stay of one month, I have the pleasure to announce that I have every reason to believe that the Zoological Station at Watson's Bay will be opened in a short time. My stay in Brisbane has once more caused me to feel the *necessity* of such an institution for the biologist. I could expatiate at length on the advantages of a zoological station, but I content myself with remarking that, in spite of my great dislike to waste my time, I was obliged to spend many days, even weeks, in Brisbane and Sydney without the possibility of working, on account of the want of a suitable place.

I repeat again my conviction, grounded on long experience, that "the immediate need is not of apparatus or libraries, but of a place for undisturbed work."[1] I hope to be able, not later than in two months, to work in the Zoological Station in Watson's Bay. I am convinced that many men of science will avail themselves of it in future years; and I am satisfied to leave for future generations such a memento of my stay in Sydney as the first zoological station in Australia.

[1] *Vide Proceedings* of the Linnean Society of New South Wales, August 25, 1878.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIGE.—The Physiological Laboratory (Dr. Foster's) will be open during the Long Vacation, and a series of repetitions of lectures and demonstrations will be given by Mr. Waters, the Assistant Demonstrator, in Elementary Biology, Histology, and Physiology.

The Cavendish Laboratory will be open during July and August, and the Professor of Experimental Physics or one of the Demonstrators will attend daily.

Prof. A. C. Haddon, of the Royal College of Science, Dublin, has been nominated by the Board of Natural Science Studies to study at the Zoological Station at Naples during the ensuing autumn.

The Board of Mathematical Studies has issued a report showing that in the last Mathematical Tripos the total of marks was 33,541, of which the first ten wranglers averaged 8582. In the last five days 11,753 marks were assigned to riders, and 7770 to problems; of which the first ten wranglers averaged 2388 and 936 respectively. The additional examiner stated his satisfaction with the answering; and he considered that much of the time formerly occupied by the study of astronomy, including the Lunar and Planetary Theories, Figure of the Earth, and Precession of Nutation, was now devoted to Heat, Electricity, and Magnetism. Comparing the progressive nature of the latter subjects with the stationary nature of the former, the latter afford the best means of testing the mathematical ability of the candidates.

Prof. Cayley will lecture in Michaelmas Term on Abel's Theorem; Dr. Ferrers (Master of Caius) on the Theory of the Potential; Mr. Niven (Trinity) on Electrostatics; Mr. Glaisher (Trinity) on Definite Integrals and Differential Equations; Mr. Hobson (Christ's) on Rigid Dynamics; Mr. Stearn (King's) on Conduction of Heat and Electricity; Mr. Allen (St. Peter's) on Magnetism; Mr. Dickson on Dynamics of a Particle.

The annual report of Prof. Adams to the Observatory Syndicate states that, notwithstanding the exceptionally unfavourable weather for observing, there had been made 2834 determinations of Right Ascension and North Polar Distance with the transit circle, including 2151 observations of zone stars which were made on eighty nights. Satisfactory observations of the partial solar eclipse, December 31, 1880, were obtained with the Northumberland equatorial, employing the wire micrometer. The observations with the transit circle for nadir point and level have been facilitated and rendered much more satisfactory by an alteration in the mode of illumination of the wires through the Bohnenberger eyepiece. Instead of placing a small hand-lamp on a stand close to the eyepiece, which gave an uncertain image at the best, the illumination is now effected by means of a paraffin lamp placed on a platform at the requisite elevation about ten feet from the eyepiece. The rays for the lamp are rendered parallel by passing through the system of lenses intended for the illumination of the microscopes of the eastern circle, which is not in ordinary use. There is now no difficulty in getting the light properly directed, and the images both of the Right Ascension and Declination wires are dark and very distinct. The observations of standard stars are completely reduced in R.A. and N.P.D. to the end of 1879 and part of 1880, as to the zone stars, the true R.A. and N.P.D. are obtained to the end of 1878, the approximate N.P.D. to the end of 1879. The calculation of reduction of apparent place to mean is completed to the end of 1876, and is far advanced for 1877. Meteorological observations are regularly made. A third assistant in the Observatory is urgently needed.

The following awards have been made by the Master and Seniors of St. John's College for proficiency in Natural Sciences:—To Samways, a Wright's Prize, with 100*l*. for the year; to Weldon, Edmunds, Love, T. Roberts, Foundation Scholarships; to Pagan, Goodman, Exhibitions. The Open Natural Science Exhibition was awarded to H. Wilson of the Leys School, and another Open Exhibition to J. Kerr of Manchester Grammar School.

SCIENTIFIC SERIALS

Trimen's Journal of Botany, June, 1881, contains:—Notes on *Carex flava*, L., by F. Townsend, M.A.—A revision of the Indian species of Leea, by C. B. Clarke, M.A.—Notes on Irish plants, by H. C. Hart, B.A.—Short notes.—Extracts and notices of books and memoirs.—Botanical notes.

THE *American Naturalist*, June, 1881, contains:—The archæology of Vermont, by Prof. Geo. H. Perkins.—On the larval habits of the Bombyliidæ, by C. V. Riley (with a coloured plate).—On the late explorations in the Gaboon, by H. von Kopenfels.—On the Pueblo pottery, by Edwin A. Barber.

Kosmos, Jahrgang v. Heft 2, contains:—Prof. Dr. Fritz Schultze, on the relations of sceptical naturalism to modern natural science, with especial reference to the evolution theory (conclusion).—Henry Potonié, on the relations of morphology to physiology.—Dr. Fritz Müller, *Atyoida Potimirim*, a mud-eating fresh-water shrimp (with twenty woodcuts).

Revue Internationale des Sciences, May, 1881.—M. Debierre, on physical dynamism and biological dynamism (concluded).—A. Charpentier, on the examination of the powers of vision, from a general medical point of view.—J. Morton, the city of Gheel, in Belgium, and its asylums.—H. Müller, on the pretended refutation of Boumer of the theory of flowers (translated from *Kosmos*).

Nyt Magazin for Naturvidenskaberne, Christiania, 1880-1881. Band 26, Heft i.—Herr Leonhard Stejneger continues his contributions to the ornithology of Madagascar, and describes a new *Tylas*, which appears to be closely allied to the *T. madagasc.* of Grandidier.—L. Meinich gives the result of his examination of the quartz and sandstone formations of the Trysilfjeld near Kongsberg, Norway, and Herr H. Rensch, editor of *Naturen*, describes the geological character of the strangely dislocated and fissured fjeld known as the Torghattee, on an island off the Heligoland coast. The same writer occupies nearly all the pages of Heft 2, first in giving the remainder of his observations of the Torghatten caverns and rocks, and next in a comprehensive and elaborate description of the character of the conglomerate sandstones and metamorphosed schists in the Nordfjord and Göndfjord districts near Berglun, to which he adds the analyses and histological results obtained from the examinations of these rocks in the Leipzig mineralogical laboratory. These numbers of the magazine contain, however, some specially interesting communications by Herren Danielssen and Koren of the various new forms of Gephyreæ and Echinodermata obtained in the Norwegian Arctic Expedition. These observers describe a form of Bonellia, to which they have given the name of *Hamingia Arctica*, which approximates closely to *Bonellia viridis*, first found in the northern seas about forty years ago by Herr Koren. Only one specimen was obtained of Hamingia. In regard to echinoderms the expedition has proved more fortunate, and Herren Danielssen and Koren describe several new forms of Asterias, Solaster, and Asterina.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, June 9.—S. Roberts, F.R.S., president, in the chair.—Prof. Mannheim and Mr. T. Craig (United States Coast Survey) were admitted into the Society, and Mr. G. R. Dick, Professor of Mathematics in the Royal College, Mauritius, was elected a member.—Much interest was excited at the meeting by the fact that one of the Society's Foreign Members was present, and proposed to read a paper. M. Mannheim is well known in this country to be a most elegant cultivator of the modern geometry on the lines of Poncelet and Chasles. He has more especially worked at the following subjects :—(1) The method of geometrical transformation, following out in this direction Poncelet's researches in the theory of reciprocal polars; (2) the plane representation of certain space-figures; (3) the wave surface (his early papers form the subject of an article in the *Quarterly Journal* for 1878 by Prof. C. Niven, F.R.S.); but lastly, he has been more particularly engaged upon the study of properties relative to the displacement of figures in space; to this he has given the name of "Géométrie Cinématique" (Dr. Ball in his "Theory of Screws" says, "To M. Mannheim belongs the credit of having been the first to study geometrically the kinematics of a constrained body from a perfectly general point of view"):—his recent work with this title has obtained a warm recognition *propter merita* in this country—on this occasion Prof. Mannheim communicated a paper "Sur les surfaces parallèles," which was characterised by all the clearness and power of exposition so well known to belong to mathematicians of the French school. Dr. Hirst, F.R.S., in proposing a vote of thanks, lightly touched upon the novelties of the communication, and expressed the pleasure it gave him and the meeting to see his fellow-student and friend present in the Society's rooms. A cordial vote of thanks having been carried, M. Mannheim briefly thanked the members present for their kind reception of him.—Other communications were:—On certain symbolic operators, by Mr. J. W. L. Glaisher, F.R.S.—On a system of co-ordinates, by Prof. Genese.—Note on a system of Cartesian ovals passing through four points on a circle, by Mr. R. A. Roberts.—On the Gaussian theory of surfaces, by Prof. Cayley, F.R.S.—On a theorem in the calculus of operations, by Mr. J. J. Walker.—On spherical quartics, with a quadruple cyclic arc and a triple focus, by Mr. H. M. Jeffery, F.R.S.—Note on the wave surface, by Prof. Mannheim.

Chemical Society, June 2.—Prof. Roscoe, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting of the Society (June 16).—The following papers were read:—Experimental researches on the amalgamation of silver ores, by C. Rammelsberg.—On the action of solvents on saponified oils and waxes, by A. H. Allen and W. Thomson. The authors have made many experiments with a view of discovering a correct method for the analysis of mixtures of hydrocarbons with animal and vegetable fatty matters. Two methods are suggested. In both, the sample is boiled with a solution of caustic soda in alcohol, which is, in one case, diluted with water, and then shaken up with ether, to dissolve out the unsaponifiable matter, leaving the soap in the solution; in the other method alcohol is added to the mixture, and then some sodium bicarbonate and ignited sand; the whole is dried and extracted by petroleum spirit in a Soxhlettes apparatus. Some analyses are given in the paper; good results were obtained.—On the sulphides of copper and a determination of their molecular weight, by S. U. Pickering. The author has heated cupric sulphide alone, in a current of hydrogen and in a current of carbonic acid, and concludes that the sulphur is given off in two separate and equal portions at totally different temperatures; therefore the molecule contains two atoms of sulphur. Hydrogen reduced the sulphide to the metallic state.—Chemical examination of the Buxton thermal water, by J. C. Thresh. The author has disproved the extraordinary statements of Playfair and Muspratt that one gallon of this water contains 206 and 504 cubic inches of nitrogen. The water really contains 22·98 c.c. of nitrogen per litre. He points out how the error originated. A complete analysis of the mineral constituents is given; amongst them are molybdic acid, cobalt oxide, &c.—On potable waters; determination of total solids, by E. J. Mills. This determination is made by carefully noting the time required by a glass bulb to rise a given distance through the water.—On the estimation of the value of zinc powder and on a gauge for measuring the volume of gases without calculation for temperature and pressure, by J. Barnes.

Zoological Society, June 7.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—The Secretary called the attention of the meeting to the opening of the Insectarium in the Society's Gardens, which had taken place on April 25, and read a report on the insects that had been reared and exhibited there, drawn up by Mr. W. Watkins, the Superintending Entomologist.—Mr. F. M. Balfour, F.Z.S., read a paper on the development of the skeleton of the paired fins of Elasmobranchs considered in relation to its bearings on the nature of the limbs of the Vertebrata. The object of the investigations recorded in this paper was explained by the author to be twofold—viz., on the one hand to test how far the study of the development of the skeleton of the fins supported the view which had previously been arrived at by the author, to the effect that the paired fins were the specialised and highly-developed remnants of a once continuous lateral fin on each side, and on the other to decide between the views of Gegenbaur and Huxley and Thacker and Mivart as to the primitive type of fin-skeleton. The author pointed out that the results of his researches were entirely favourable to the view that the paired fins were structures of the same nature as the unpaired, and that they gave a general support to the views of Thacker and Mivart. They clearly showed that the pelvic fins retain more primitive character than the pectoral. Conclusions were drawn somewhat adverse to the views recently put forward on the structure of the fin by Gegenbaur and Huxley, both of whom considered the primitive type of fin to be most nearly retained in *Ceratodus*, and to consist of a central multisegmented axis with numerous rays on its two sides. It appeared, in fact, that the development of the skeleton demonstrates that a biserial type of fin like that of *Ceratodus* could not have been primitive, but that it must have been secondarily derived from a uniserial type, by the primitive bar along the base of the fin (the *basipterygium*) being rotated outwards, and a second set of rays being developed on its posterior border.—Mr. W. T. Blanford, F.Z.S., read some notes on a collection of Persian reptiles recently added to the British Museum, amongst which was an example of a new species of lizard, proposed to be called *Agama Persica*.—A communication was read from the Rev. O. P. Cambridge, C.M.Z.S., on a new spider of the family *Theraphosidæ*. The chief interest attaching to this spider was the fact that it had lived in the Gardens of the Society from March to October, 1880. Mr. Cambridge proposed to name the species *Homœomma Stradlingii*, after Dr. Stradling, who had brought the specimen

in question home from Bahia.—Mr. G. E. Dobson, C.M.Z.S., read a paper on the pharynx, larynx, and hyoid bones in the *Epomophori*, indicating some very remarkable peculiarities of structure, in which these bats appear to differ not only from all other Chiroptera, but from all other mammals. Pharyngeal air-sacs were also described in the males of *Epomophori monstrosus*, *franqueti*, and *comptus*.—Mr. J. Gwyn Jeffreys, F.R.S., read the third of the series of his memoirs on the Mollusca procured during the *Lightning* and *Porcupine* expeditions, 1868-70. The present paper contained an account of the families from *Kelliidæ* to *Tellinidæ*. Eleven new or hitherto unfigured species were described. The geographical, hydrographical, and geological distribution of the species enumerated were fully given.—Mr. F. C. Selous read a paper on the South African Rhinoceroses, based upon specimens collected and observations made during nine years' hunting in Southern and South-Central Africa. Mr. Selous had come to the conclusion that in these countries only two well-marked species of *Rhinoceros* existed—namely, the square-mouthed *Rhinoceros simus*, and the prehensile-lipped *R. bicornis*.

Entomological Society, June 1.—Mr. H. T. Stainton, president, in the chair.—Rev. E. N. Bloomfield, M.A., was elected a Subscriber to the Society.—Mr. J. Jenner Weir, on behalf of Mr. J. W. Douglas, exhibited, and read remarks on, various British species of *Aleurodes* and *Orthezia*, one of which was described as new to science under the name of *O. Normani*.—Mr. T. R. Billups exhibited specimens of *Crabro clavipes*, L., and *Molorchus minor*, L.—Mr. J. Sang exhibited some interesting varieties, &c., of British *Lepidoptera*.—The Secretary read a communication from Mr. G. E. Piercey respecting a creature stated to be noxious to travellers in Turkestan. It was suggested that this was probably identical with the well-known *Argas Persicus*.—The Secretary also read a report from the Committee appointed at the last meeting of the Society to inquire into the supposed presence of *Phylloxera* on the vines in Victoria; also a communication from the Colonial Office respecting an insect stated to be destructive to the eggs of locusts in the Troad.—Lord Walsingham read a paper on the *Tortricidæ*, *Tineidæ*, and *Pterophoridæ* of South Africa.—Mr. A. G. Butler communicated a memoir on the genus *Sypna*.—Mr. W. L. Distant communicated descriptions of *Rhynchota* from the Australian and Pacific regions.

PARIS

Academy of Sciences, June 6.—M. Wurtz in the chair.—The Secretary read telegrams from the Emperor of Brazil (of May 31 and June 2) announcing the discovery of a comet.—On the right ascensions of the moon observed at Algiers by M. Trepied, by M. Faye. Hansen's tables, defective for long-period inequalities, are shown to be perfect for ordinary inequalities. Newcomb's correction, instead of being too great, has to be increased 1".—Researches on sulphide of nitrogen, by MM. Berthelot and Vielle. *Inter alia*, it detonates with violence under the hammer, but it is less sensible to shock than fulminate of mercury or nitrate of diazobenzol. In heating, it deflagrates about 207°. The heat of formation is negative. The pressures arising from explosion in a closed vessel are very near those from fulminate, but the velocity of decomposition is very different.—On the report of M. Roudaire, on his last expedition in the Tunisian Chotts, by M. de Lesseps. Further examination confirms the feasibility of the inland sea project, the political advantages of which (with others) are noted.—On the geological results of M. Roudaire's mission, by M. Hébert. Tunisia seems to have emerged during the long periods between the deposit of the Senonian Chalk and that of the Middle Miocene. The basin of the Chotts, with the Cretacean masses bordering it on either side (their strata anticlinal), are like a button-hole, the Chotts forming the aperture.—New analyses of jadéite and some sodiferous rocks, by M. d'Amour. It is proved that beds of jadeite (which is largely used in India and China for ornaments, and found in the form of coins, hatchets, &c., in European dolmens and caves), exist in Asia, especially the Thibet region; also in North and South America. From analysis of some European rocks the author finds reason to suppose that beds of jadeite may also be found in the Alpine chain, or region near; thus the prehistoric articles in Europe would be naturally explained, without migration of Asiatic peoples. MM. Boussingault and Daubrée made remarks on the subject.—Study of electricity on board modern ships; incidental remarks (1) on the influence of the mode of junction in complex electric circuits, and (2) on the principle of an electric hygrometer and a fire-alarm, by M. Ledieu. The iron hulls of fast ships, sheathed with wood, then copper, both fixed metallically, form a complex pile, which the author studied. Experimenting with a moist piece of wood having copper nails in it, he found a battery current had much greater intensity when the rheophores were applied to the nails than when applied to the wood. Using dry wood with the former arrangement, the intensity varied with the atmospheric moisture. This might be applied *e.g.* in measuring dew-formation. For a fire-alarm he would keep the wood slightly moist by means of spongy matter on its surface connected with water. A galvanometer would indicate the degree of dryness of the wood, and if a certain limit were reached the needle would cause a bell to ring.—On the *rôle* of phosphoric acid in volcanic soils, by M. de Gasparin. He controverts Prof. Ricciardi's view that the fertility of the eruptive strata of Etna is due to presence of this mineral. The concomitance of muddy formations, and the climate, hastening the decomposition of lava, are the chief factors.—The vines of Soudan of the late M. Légard, by M. Planchon.—The solar parallax deduced from American photographs of the transit of Venus of 1874, by Mr. Todd.—On the functions of two variables arising from the inversion of integrals of two given functions, by M. Fuchs.—On the expressions of co-ordinates of an algebraic curve by Fuchsian functions of a parameter, by M. Picard.—On a property of uniform functions, by M. Poincaré.—On the liquid state and the gaseous state, by Mr. Hannay. He claims to have proved, more than a year before, for all pressures, what MM. Cailletet and Hautefeuille have lately established for a single pressure: viz., that the continuity of the liquid and gaseous states (Andrews) is only apparent.—Cyanides of sodium and barium, by M. Joannès.—On the combinations of iodide of lead with alkaline iodides, by M. Ditte.—On the *rôle* and the origin of certain microzymas, by M. Béchamp. The microzymas in rocks, earth, mould, street-dust, or the slime of marshes, have no other origin than those forming an integral part of every living organism, and whose *rôle* is the total destruction of this after death; after which they remain in the soil or the air, ready chiefly for transformation of organic matter for vegetation.—On the non-existence of *Microzyma Cretæ*; reply to M. Béchamp, by MM. Chamberland and Roux.—On the mechanism of troubles produced by cortical lesions, by M. Couty. Unilateral and limited cortical lesions involve profound modifications of the various functions of the medulla oblongata and the spinal cord opposite, leaving intact the brain-functions.—On the embryogeny of Ascidians of the genus *Lithonephora*, by M. Giard.—On the stomatorrhiza of *Succulina carcini*, Thompson, by M. Jourdain.—On the morphology of the fœtal envelopes of Chiroptera, by M. Robin.—Contributions to the cryptogamic flora of Banks's Peninsula (New Zealand), by M. Crié.—Mr. Stone's star-catalogue was presented by M. d'Abbadie.

CONTENTS

THURSDAY, JUNE 23, 1881

LECTURES ON TEACHING

Lectures on Teaching, Delivered in the University of Cambridge during the Lent Term, 1880. By J. G. Fitch, M.A. (Cambridge: University Press, 1881.)

OUR review of this new contribution to the now copious and increasing educational literature of the country has been delayed by causes complimentary to its author. The felicity and charm of the style, the freshness of treatment of even hackneyed topics, and the interest and practicalness of the matter, rendered the reviewer's proverbial dipping into a book impossible in this case, and the work had to be read for its own sake as much as for that of criticism. The author has long been known as one of our most earnest practical and enlightened educationists, and though perhaps not a polemical pioneer in the educational field, an advanced, safe, and healthy thinker on the important problems involved. Of this new utterance of her husband, the *uxor dilectissima*, to whom the book is curiously but most appropriately dedicated, has no reason to be ashamed, even though it is not the newest poem or novel, and only a prosaic, but by no means prosy, volume of "Lectures on Teaching"—a title, by the way, much too modest for the quality of the book, which should in future editions be exchanged for one more worthily distinctive and more expressive of its contents.

The occasion of its production is one of no small interest and importance in the history of educational progress in this country. For some years back, there have been made some laudable attempts to secure for education University recognition and standing, by the appointment of Professors of Education. The first practical effort in this direction was made by the College of Preceptors, which had the merit of appointing, in 1873, the first Professor of Education in Britain, the late enthusiastic and enlightened Joseph Payne. This was followed by the establishment of Education Chairs, in 1874, in the Universities of Edinburgh and St. Andrews. It took some time for our greater conservative Universities to adopt, even in part, such an unwonted innovation, though earnestly and repeatedly pressed to do so in Memorials from the Head-masters of our great Public schools, whom they had trained as scholars but neglected as teachers; leaving them to gain what professional skill they have, as they themselves confess, at the expense of their pupils. At length, in 1879, Cambridge came to the conclusion that it would no longer be derogatory to them to patronise to some extent the new Science of Education; and a "Teachers' Training Syndicate" was appointed, which issued a scheme of examination in the history, theory, and practice of education, with lectureships on these branches of the subject. The first course was given by the well-known genial educationist, Mr. Quick, on its history; the second, by Mr. James Ward, on its science; and the third, by Mr. Fitch, on its practical aspects, which we have now before us. The next step for these Universities to take, which they must—shall we say cordially will?—take before very long, will be to do for education what has been done for other subjects—to give it full University status by the appointment of Professors of Education—a step that will do more than aught yet attempted to give teaching and teachers the standing, influence, and emolument to which the importance of their work to national culture and progress justly entitle them from the lowest to the highest; and a step that will more than repay the Universities themselves, which would thus be entering, as Mr. Fitch well observes, "on an honourable and most promising field of public usefulness," that will help to "make the work of honest learning and of noble teaching simpler, more effective, and more delightful to the coming generations."

Though deprecating any claim to be a systematic treatise, or a "manual of method" on the subject, the book traverses the greater part of the general field of inquiry, and to the practical student and teacher will be found more helpful, suggestive, and scientific than more elaborate and pretentious text-books. The work is singularly readable, attractive, sound, sensible, and practical, and altogether free from hobby-horsiness. Though subordinating theoretical treatment, Mr. Fitch is, as a rule, scientific in spirit and suggestion; and when discussing, as he does, controverted questions that generally rouse polemical combativeness, if not bitterness, he does this with so much of the *suaviter*, and with such genial persuasiveness in favour of his conclusions, which are for the most part sound and abreast of recent opinion, that the book is calculated, in an unusual degree, to carry enlightened conviction on many problems into the conservative ranks of University men, still too impervious to change in their traditional views of education. Canvassing, as Mr. Fitch does, so many subjects of controversy, it cannot be expected that his conclusions will be generally accepted—on not a few we deem him in error, and should be prepared to join issue; but they are always so presented as to command, and to gain, the most careful consideration and to disarm opposition—an invaluable element with the class he specially addresses.

The mere headings of the chapters show the interest and extent of the field traversed. We have "The Teacher and his Assistants," in which an inspiring, noble, but far from Utopian, ideal is held up for imitation: "The School, its Aims and Organisation," in which such important questions, as what a liberal education is, what subjects should be taught, and the like, are discussed: "The School-room and its Appliances," where excellent practical suggestions are given towards making our schools the healthy, comely, and educative centres they ought to be in any wise community: "Discipline," in which this vital element in school life is treated with admirable spirit and wise counsel, traversing the various disciplinary influences that ought to be brought to bear on the child, before the *ultima ratio* of corporal punishment is resorted to, which, though not condemning it altogether, he wisely thinks "is almost wholly unnecessary, does more harm than good, and in just the proportion in which teachers understand their business, they will learn to dispense with": "Learning and Remembering," where some sensible practical hints are given as to the use and abuse of that universal school hack, the memory, the treatment of which he rightly considers a very good test of "the difference between skilled and unskilled teaching," though his psychology of this so-called faculty is questionable, savouring

too much of the orthodox Hamiltonian school, and too little of the, at present despised, phrenological, which latter considers memory a mode of action of all the intellectual faculties, and which, on this point at least, contains more truth and practical suggestion than is generally suspected: "Examining," in which the elements of skilful questioning are well put, and not a few fresh suggestions made, as in regard to the use of "Socratic questioning" in school, on which, however, we recommend to the author's considera-tion, the views of his friend, William Ellis, the liberal and philanthropic founder of the Birkbeck Schools, who was the first to adopt and advocate it in its entirety, making it a true Socratic *dialogue, the pupil also stating difficulties and asking questions of the teacher*, as well as answering questions asked: the "Preparatory Training" of the child, in which much sound practical advice is conveyed as to the early stages of the "three R's" and their congeners, but in which, amidst much appreciation of the Kindergarten, this system is on many points—we are sure unconsciously—greatly misunderstood and misrepresented; as that Fröbel did not take "a large, or very sound, view of the purpose of education as a whole," that the system "does little or nothing to encourage reflection," is "apt to mistake means for ends," "does not train to overcome difficulties," and much else in the same strain, regarding which, we may safely and confidently leave Mr. Fitch to Miss Shireff and other wise and fully-informed *Kindergartener*: "The Study of Languages," in which the vexed question of the place of the ancient classics and modern languages in education is very fairly and estimatively stated, and recent broader views advocated, a chapter that would be studied with advantage by both parties in this wordy strife: "The English Language," where he claims a very high—we should be inclined to claim even a higher—place for the native tongue and literature, in intellectual, cultural, and general educative power, when rightly taught, and gives some excellent practical suggestions to help towards this, his conclusion being admirably expressed thus—"If your scholars do not acquire a positive love for reading; if they do not ask to be allowed to read the whole book or poem of which the extract you take as a lesson forms a part; . . . if they do not feel a heightened admiration for what is noblest and truest in literature, and an increasing distaste for what is poor and flimsy and sensational: then be sure that there must be something incurably wrong in your method of teaching, and that all your apparatus of grammar, paraphrase, and logical and grammatical analysis, will have failed to fulfil its purpose."

Then we have two chapters on "Arithmetic," both as an art and a science, on which, as might be expected from the author of a well-known text-book on the subject, he places high value, for both culture and use, and on which he gives very good hints; on "Geography" and "History" he is equally fresh, suggestive, and practical; and the book concludes with two very good chapters on the teaching of "Natural Science" and on "The Correlation of Studies."

On the place of science and scientific teaching in all true education Mr. Fitch speaks with his accustomed candour, fairness, and perspicuity, and pleads in their favour with a quiet but firm and skilled advocacy, which, with its genial non-polemical incisiveness and force, makes it a real acquisition to the growing literature on the scientific side, which will carry conviction into certain scholastic circles that would be, as they have been, deaf to more formal and strenuous pleading. Here Mr. Fitch reveals himself with unwonted power as a skilled, Socratic, but disguised polemic, in the advancing cause of science in education. His way of making the scientist in education put his case "to those who live in the academic world," is admirable: "You are mistaken in supposing that the domain of physical science is a merely material and practical region, while yours is essentially intellectual. There is here a body of truth of the highest practical utility, no doubt, but also of the greatest value for educational purposes. The laws and principles by which the facts of the material world may be explained and co-ordinated, are quite as uniform, quite as beautiful, and as far-reaching in their applications, as any of the laws of language or the truths of mathematics. Moreover, the processes of thought required in the study of these questions are just as vigorous, just as stimulating, stand in just as close a relation to the intellectual needs of a well-instructed man, as those involved in the older studies. You can make the teaching of physical science as fruitful, as thoroughly disciplinal for all the higher purposes contemplated in a liberal education, as the teaching of Greek or of geometry, if you will only first recognise the possibility of making it so, if you will encourage skilled and accomplished men to take up this branch of instruction, and are ready to give them the same status and encouragement as you now give to accomplished teachers of philology or history. Enlarge your conception of what a liberal education means." Mr. Fitch concludes that "of the legitimacy of these claims there can be no doubt," and wishes "some Huxley or Tyndall had enunciated this message before we ourselves went to school." He shortly discusses the utilities of physical truths, their beauty and intellectual attractiveness; the disciplinal value of the inductive process by which they are discovered, and its function as an invaluable corrective and necessary supplement to the one-sided deductive method of the common scholastic studies. His recommendations on the practical teaching of science in schools, and on technical education, are also good, and to the point. Altogether, this chapter on Science deserves perusal by all interested in its teaching, and we wish our space allowed us to take his arguments up in detail.

The last chapter, on "The Correlation of Studies," is an important one, and in great part sound and sensible, though not a few will be inclined to disagree with the author on some points; for the problem of "the conflict of studies" is as yet more crude and unsettled than any other in the educational world. He wisely controverts the plausible maxim, *non multa, sed multum*. His division of the proportionate times that should be given to the different classes of studies, viz. "nearly half to language and literature and subsidiary exercises, and of the remaining half, rather the larger portion to mathematics, and the smaller to experimental science," will provoke controversy, and is surprising in an advocate of science in schools. He would also have never more than two physical sciences studied at the same time. His remarks on the principle of selection amidst the increasing press of studies should be serviceable to distracted teachers, and help them to a

choice; though here, also, he leans overmuch to the linguistic side.

In the curriculum of studies recommended by him, Mr. Fitch has strangely subordinated, if he has not greatly ignored, the studies that prepare the citizen for his duties as a member of the State—the Social and Political Sciences. Surely he has not read, or greatly studied, the admirable pleading in their favour to be found in the works of his friend William Ellis, and also of George Combe, as recently fully presented in his educational contributions, edited by one of his colleagues in the inspectorate. In this respect, there appears to us to exist a serious hiatus in his plan of study. One of the doctrines he enunciates is also enough to raise the old phrenologist from his grave, what he calls "the convertibility of intellectual forces," whereby, he informs us, "every kind of mental power, once worked and applied to a worthy purpose, becomes available for other purposes, and *is capable of being transformed into power of another kind*"—an ancient error in schools, still fruitful of failure and wrong, which we are surprised to find held by a man so generally wise on education. Poor George Combe otherwise fares sadly at the hands of Mr. Fitch, who describes his mission in life as being that of advocating the one doctrine of inherent hereditary aptitudes, and says that "he never could induce his friends seriously to attempt the classification and teaching of a school on his principles, and the experiment yet remains untried"! What of the history of such schools given in the work on Combe's principles just named?

Though not agreeing on many points with the author, as was inevitable in a field so full of controversial matter as the growing science of education, we look upon the book as a valuable contribution to the subject, which, by its unusually attractive style and high tone, will command a wide audience, and, from the auspices under which it is produced, will reach places where sound educational philosophy too seldom penetrates. We cordially recommend it to all interested in education, and specially to teachers; and also to the active Education Society, as, like Prof. Bain's recent work on Education, which they have already taken up, an admirable basis of profitable discussion.

PRACTICAL HISTOLOGY

A Text-Book of Practical Histology, with Outline Plates. By W. Stirling, M.D., Sc.D., F.R.S.E., Regius Professor of the Institutes of Medicine in the University of Aberdeen. (London: Smith, Elder and Co., 1881.)

AT the outset Dr. Stirling informs us that "the purpose of this work is twofold: first, to give plain, definite, and precise directions for the preparation and examination of the animal tissues; and secondly, to ensure that the student executes a drawing of the majority of the microscopic specimens which he makes for preservation. For this purpose a series of Outline Plates is issued with the text."

As regards the first of these objects, there is no doubt that to give "plain, definite, and precise directions" is a desirable and praiseworthy object, which most, if not all "practical" books strive to attain. Those that succeed in this endeavour differ from one another chiefly in the means by which this object is accomplished; in some the author arrives at his object after long-continued patient and diligent work, in other rarer instances he utilises the works of others, and by doing so he may, and sometimes actually does, produce a book which has considerable merits of its own, inasmuch as it gives in plain and simple words valuable and useful extracts of much larger original works full of minute and bewildering details, not easily understood by, and of little practical use to the ordinary student. Dr. Stirling, although his book cannot in any sense claim to be considered other than a book of compilation, has nevertheless succeeded in presenting to the medical student, anxious to acquire the necessary amount of knowledge in practical histology, a work which, conveying in a short and intelligible manner a great deal of information, will, we doubt not, prove of service.

As regards the second object of the book, viz. that the student should for himself make drawings of his microscopic specimens, we fail to see how Dr. Stirling's Outline Plates can advance this object in a satisfactory manner. We always thought that the student drawing the correct outlines of the specimens or of parts of the specimens prepared by himself, has got everything that is essential to guide him in the study of those specimens. To fill in the details in pencil, or, as Dr. Stirling suggests, and what is still more laborious, in colours, in the outline figures drawn for him from somebody else's specimens, appears to us of more than questionable value.

Besides the directions for practical work a considerable part of the book is taken up by the description of the structure of the simple tissues and organs. As far as we can see, these descriptions are in a great measure, to the extent of verbal quotations, borrowed from other books, without even an attempt to mention this fact; by doing so Dr. Stirling has deviated from the accustomed rule, and has proceeded in a rare and unexpected manner. If an author introduces abstracts and verbal quotations from any other work, we believe it will be universally admitted that whatever the aim and nature of the book, the author is bound to mention his source; if he omits to do this, intentionally or unintentionally, he lays himself open to the charge of having committed what in the eyes of every right-thinking man, not to say of every man of science and teacher in a responsible position, must ever be considered a grave offence.

Dr. Stirling has made very extensive use of the "Atlas of Histology" in some chapters, *e.g.* on the salivary glands, the kidneys, the generative organs, and others, making copious extracts therefrom, to the extent of verbal quotations, without in any way indicating that he has done so.

Dr. Stirling's proceeding is greatly to be regretted, since by his numerous and original works in histology he has won the esteem of his *confrères* and has proved himself to be sincerely anxious about the promotion of this science.

The publishers deserve great credit for the handsome style in which the book is brought out.

E. KLEIN

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Electric Railway in Paris

I HAVE within the last few days received a letter from a friend in Paris, who writes that he had last week travelled on the electric railway in that city. There is still much, he adds, to be done before it can be brought into general use; but nevertheless the train moved satisfactorily. There were fifty-four passengers in the carriage, which was propelled by a large Gramme machine and 160 cells of Faure's battery. The experiments are to be recommenced very shortly with a new motor by M. de Méritens, and a Faure's battery. W. SPOTTISWOODE

41, Grosvenor Place, S.W., June 16

Probably New Variable and Red Star

ON May 22 I found, 2° 51'·7 north of α Cygni, a deep red or crimson star, which is not in the Bonn Catalogue. The nearest to it there is + 47° 3167, which in declination corresponds with a white star that I observed at the same time, but not in R.A.

Dr. Doberck writes to me as follows:—"Markree Observatory, 1881, May 29; observed the *new* star; brick-red, the nicest I ever saw, 8·7 mag."; and "1881, May 31: 1h. a.m., colour same as before; 8·4 m."

Mr. Ward of Belfast, who observed in the early morning of May 31 in strong twilight, describes the star as "deep crimson; beautiful object; two or three comites."

Mr. Gledhill, in Mr. Crossley's observatory, Halifax, found it, on May 30, "strikingly red."

Dr. Ball, the Astronomer-Royal for Ireland, observing at Dunsink, saw it "a superb crimson."

Prof. Krueger, director of the Kiel Observatory, described it as "*auffallend roth*" (remarkably red), on May 30.

On June 2 it appeared to me unchanged in colour, and increased from 9 mag. to 8·4.—June 7 and 8, colour still the same, and 8·3 or 8·4 magnitude.

Dr. H. Krentz, writing from the Bonn Observatory, states that he finds an observation of the star recorded on June 19, 1857, but not at any other time during the progress of the observations for the Bonn Catalogue, in which it has not been published. There does not appear to be any note of its colour, and I think it will most likely prove a variable of a very remarkable character. Prof. Krueger makes its position for 1855 = α 20h. 36m. 37s.·9; δ 47° 37' 33". Herr Krentz's position is α 20h. 36m. 37s.·0; δ 47° 37'·9. The white star mentioned above is not recorded in any of the Bonn observations; and, on the other hand, I may add that I do not identify + 47° 3167 in the telescope. I estimated the white star at about 9·5 mag. of Argelander's scale, and therefore within the limits of the *Durchmusterung*.

The small stars seen by Mr. Ward are perhaps too distant to be strictly considered as comites to the red star. They are sufficiently difficult to me, though probably easy to his well-known extraordinary sight. The position of the nearest that I see is about 0°, and I find two others more distant—one at 350°, and one at 110°, with a power of 120° on a 4½ inch O.G.

There seems a peculiar dimness about the star, referable, probably, to the dark shade of its red. An uneducated person with a very excellent eye, and who never heard a description of a red star, compared it, at first view, to "a drop of *black* blood." It may be conveniently and well compared with Nos. 448 and 553 of my "Red Star Catalogue," especially with the former, the colour of which was described by Secchi as "intense"; and in the glowing red of the one object will be remarked a striking contrast with the deep sombre tint of the other.

I make the approximate positions of the red and the white stars for 1855, and corrected from my first observations, as follows:—

	h. m. s. ° '
The red	20 36 27; + 47 37·5
The white...	20 36 18; + 47 46·8
Argelander's position of his + 47° 3167	20 36 28; + 47 46·8

Millbrook, Tuam, June 3 JOHN BIRMINGHAM

The Doctrine of the Conservation of Electricity

I WISH to take the earliest opportunity of responding to the courteous letter of M. Lippmann, which appears in the current issue of NATURE, with the acknowledgment that his quotation from the *Comptes rendus* of 1876 establishes in the most conclusive manner his priority of date in the enunciation of the doctrine of the *Conservation of Electricity*. As to my own independent enunciation of this doctrine, it was arrived at without any knowledge of the comparison drawn by M. Lippmann in 1876 between the cyclical flow of heat (of Carnot's theorem) and the cyclical flow of electricity. I approached the matter upon somewhat different and less clearly defined lines, and finally struck upon the fundamental notion of the Conservation of Electricity when endeavouring to think out the relations between electromotive and ponderomotive force in an electric theory of radiation based upon Clerk-Maxwell's Electromagnetic Theory of Light. My speculations on this point were committed to writing some weeks ago, and will shortly be published. I content myself in the meantime with pointing out how near Clerk-Maxwell came to a similar conclusion. In Article 35 of his well-known treatise, he says emphatically: "While admitting electricity, as we have now done, to the rank of a physical quantity, we must not too hastily assume that it is, or is not, a substance, or that it is, or is not, a form of energy, or that it belongs to any known category of physical quantities. All that we have hitherto proved is that *it cannot be created or annihilated*" (the italics are mine). Nevertheless the immediate and logical conclusion that electricity, like matter and like energy, is subject to a law of conservation, appears to have been rejected by Clerk-Maxwell for reasons explained in Article 574 of his treatise, consequent on his inability to discover whether an electric current possessed momentum or could exert a mechanical reaction upon the matter of the conductor through which it flows. The unfortunate dilemma which suggested this experiment could hardly have been raised if it had then been as clearly understood as it now is that there is the same distinction between electrokinetic and ponderokinetic energy as between electromotive and ponderomotive force. But to discuss this matter further would lead me to take up too much space. SILVANUS P. THOMPSON

University College, Bristol, June 19

Thought-Reading

IT would seem that the "discovery" of reading people's thoughts, lately mentioned in the daily papers, is in no way essentially different from the well-known "game" of "wishing" often played by young ladies. It consists of the following procedure. One person goes out of the room, while others arrange upon what she is to do. She enters blindfolded, and in the particular instance now alluded to, was turned round several times so as to be quite unconscious of the direction in which she was facing. Two persons now place their hands on either side of each shoulder, making their fingers meet at the back of the neck and under the chin; or they may be placed round the waist; but as the forehead appears to be equally sensitive, perhaps it is immaterial where the hands be situated. After standing still a moment or two, the lady moved slowly round in the direction of a sofa under the impression, as she afterwards said, that she was walking in quite another way. Having reached it, she sat down (not even knowing the sofa was close by), and deliberately put out her hand, took up an antimacassar which lay upon the sofa, and raised it, asking, "Is this what I was to do?" This was perfectly correct, the antimacassar having been expressly laid there for the purpose.

It was settled that another lady should walk into the conservatory. To do this she had to pull up a blind, lift an iron bar and open the shutters, then undo the glass door behind them which led into the conservatory. All this she did unhesitatingly, and walked straight into it. I could describe several other instances where ornaments and other things had to be selected out of various groups of objects, &c.: but the above will illustrate the process.

One essential condition of success is that the individual must voluntarily and entirely surrender the will, while those who hold the person blindfolded must *determine* as powerfully as they can that the latter shall do what they wish. Care should be taken not to *push* the individual in the desired direction. This however may be done involuntarily, but it will not account for the person doing all that has been previously determined after

arriving at the spot. Some ladies describe a remarkable sensation accompanying the proce s: a sort of "all-overishness," or even faintness, so much so that the lady first alluded to on one occasion staggered, and could not proceed at all.

It appears to me to be a very peculiar psychological phenomenon well worthy of investigation, if possible, but too well known to be disputed. GEORGE HENSLOW

6, Titchfield Terrace, Regent's Park, N.W.

P.S.—Since writing the above I have heard of a much more remarkable case than the preceding. The operators sat in a circle, silent, but determinately "willing" that a certain lady should do what they had resolved upon. She stood in the centre, and was not blindfolded or touched by any one. In every case she did it correctly. One thing that was agreed upon was for her to take a bottle of wine from one table, carry it to another, and pour out a definite quantity of wine. This she did, not exceeding the amount predetermined. On a second occasion she had to find a key hidden away behind some books. As she approached the place she became very excited and hysterical, but at once extracted the key.

The above cases clearly show that as far as they are concerned "thought reading" is an incorrect expression, as the person operated upon is a passive automaton, while others, as it were, force their wills upon her. "Will-imparting" would seem to be a better term.—G. H.

Notes of the Cuckoo

IN a letter appearing in NATURE, vol. xxii. p. 76, I stated that—"All the cuckoos here intone in a minor key except one, which alone does not flatten the 3rd of the tonic. The key is in all cases precisely D of concert pitch, as proved by a tuning-fork, and the first note is F on the fifth line." This year I find that, while the cuckoos here generally intone in D minor, as above, there is one again that intones in D major, and two others in C major and C minor respectively. Some that I casually heard in other places in the neighbourhood intoned in D minor.

Millbrook, Tuam, June 1 JOHN FIRMINGHAM

Notes on the Indian Glow-Fly

HAVING failed to find any critical description of these interesting insects, it is possible that the notes I am now able to send you may cause others to enter the field of inquiry. Situated some 2900 feet above the sea, and in Central Southern India amidst hills, valleys, and streams, I have had peculiar opportunities for observing them.

They are not to be seen during the daytime, but so soon as darkness steals upon twilight, so surely do these small natural lanterns become visible, and their numbers rapidly increase, much indeed as the visibility of the stars increases as the evening passes into midnight.

The fire-fly, when examined individually, is by no means a pretty-looking insect, and comparing it to other insects and flies, it is certainly both large and ungainly. An ordinary house fly is ive-sixteenths of an inch in total length and weighs ·25 grains, but the subject of my notice has a total length of nine-sixteenths of an inch and weighs ·66 of a grain; we thus at once learn something as to his size and weight. The glow-fly—or beetle as should term it—has a black head and antennæ; the thorax and abdomen are of a yellow-red colour. This latter part of he insect's body is divided into six rings, and, counting from the horax, it is the fourth ring that emits the light. There is a ectangular opening in this ring which is merely covered by a ery thin skin; it is in fact a *window* from which the light nerges. The insect has only one pair of wings: these are nall, most delicate and thin, and are sheathed. It is worthy careful notice that these insects fly both rapidly and slowly, it make no noise or buzz in the air. To test this further I have equently liberated several of these glow-beetles in my bedom, and in the dark they have only appeared as *fairy stars*, no humming could be detected.

As regards the character and quantity of the light, I have to serve that one insect enables me to see the time by a white-ed watch when four inches distant; twelve of them placed in glass jar enable me to read a book with ease, and are equal to mall Geissler's tube. The light is of an exceedingly beautiful lour—a sombre yellow tinged with green, but at intervals it is lliant. A preliminary examination of the light in the spectroscope (a large one made for me by Browning) shows a distinct clear continuous spectrum, no lines or bands of any kind being visible.

The insect made to crawl on a card placed over the poles of a powerful compound permanent magnet showed no signs of uneasiness or change of light. Similarly placed over an electromagnet (ten Grove cells) and rapidly alternating the current caused no change. Placed within a coil of covered wire, no change. Blowing very gently, my breath on the insect caused no change; this was also tried with a blowpipe. Cold air at 50° caused a distinct diminution of the light; on the other hand, air at 100° caused an increase of light.

I now placed several of the insects in a bell-jar, and gave them a good supply of clean oxygen gas; the luminosity at once increased fully 25 per cent. On a dead insect (which still sheds light) oxygen gave similar results, and on extracting the luminous part and blowing oxygen upon it the light was much increased.

It will prove interesting to mention that, so soon as darkness has fairly set in, millions of these insects invade the trees, and as my bungalow is near to a stream and level with the tops of the trees, I am able to notice them with much care. The curious pulsation or flashing of their light is remarkable: the insects resting on the tree all act in perfect concert, *i.e.* five seconds of no light, then seven rapid flashes; five seconds, no light, seven flashes; and so the game continues throughout the dark hours.

At first I had reason to believe that the insect when flying only emitted light; this however is not the case; for when observing the Pole Star for variation with my theodolite, it occurred to me during a passing cloud to turn a telescope on to the glowing tree. At once I had the field of view filled with tiny stars, but both fixed and wandering.

It is also worthy of special notice that all the glow-insects on a dozen or more trees will continue to keep up the most perfect time as to the flashing of their light and the interval of pause, and this for many consecutive hours; but this singular agreement as to the time relates to close clusters of trees only. Thus distinct groups of trees separated by one or more hundred yards may not agree, and do not do so as a rule.

I have been informed on safe authority that the Indian bottle-bird protects his nest at night by sticking several of these glow-beetles around the entrance by means of clay; and only a few days back an intimate friend of my own was watching three rats on a roof rafter of his bungalow when a glow-fly lodged very close to them; the rats immediately scampered off.

In conclusion, these insects see by day as well as by night, and I incline to the idea that the beautiful light they carry serves as a means of intimidation or protection, and certainly as a means whereby to recognise friends.

As I gaze from my verandah down the Nadgani Valley into the dark night I see the pulsations of light here, there, and everywhere! and as my optical powers increase so do these gaseous, nebulous patches become resolved into real living stars! H. A. SEVERN

Wynaad, India, May 5

Birds Suffering from Cold

THE unusually severe weather (5° – 12° Réaumur) of these last twelve days struck heavily on the swallows of our country. They have been found dead by hundreds. The distress of the poor animals must have been extreme. Suffering from hunger and cold, they pressed against the windows, and being brought in suffered to be petted and fed, but died from exhaustion. In Kopidlno about 300 took shelter under a balcony, and the cold growing more and more intense towards night, they clustered on each other like bees until morning, when thirty were found dead. I have been walking this afternoon in the suburbs of Prague, where a fortnight ago I have seen swallows skipping on the river and hunting in gardens, but although the weather was now clear and warm, I could not see a single one. Tidings of suffering swallows come from the country, where people have been kind to them, feeding them on ants' eggs and flies, but they would not eat, and died. In some nests the young ones were found starved alone, in others their mothers were with them. J. V. SLÁDEK

Prague, June 16

An Optical Illusion

THE illusion of the inverted pin was shown me about the year 1846-47, and I well remember, when I was at Cambridge,

working out the explanation inductively. In the autumn of 1847 I was spending an evening with Dr. P. M. Roget, at his house in Woburn Square, when among other subjects we conversed upon was that of optical illusions. The inverted pin was one of his illustrations, and I think he mentioned having explained it in some scientific serial.

Some years ago the late Mr. Beeker, formerly scientific foreman to Messrs. Elliot Brothers, constructed for me a binocular apparatus for showing the union of two shadows, one on each retina. To my surprise I found the resulting phantom did not differ in position from the single shadow. C. M. INGLEBY

Athenæum Club

How to Prevent Drowning

I ONLY write in the interests of humanity. Let those who will go in for swimming, and I wish sincerely that every one could swim. Treading water however conducts at once to swimming. Every one can tread water who likes. It is just as easy, if we only knew it, to tread water as to tread the earth, and proximately just as safe. Men and women might walk into the deep sea and out again when they pleased. Nature has not been so niggard with us as some persons imagine. Why are we not as safe in water as is the dog? It is simply because he treads water, and we do not. As often as I chose to chuck my stick into the Causey surge my dog brought it out. I could have done the same; any one could do the same who chose. But assuredly I should have paddled water as the dog did. In treading water the body is erect, or nearly so; in swimming we sprawl, and are comparatively helpless. The admirals, both of them, have given valuable testimony as regards the efficacy of treading water. Before the present pier at the Cape was built, vessels in bad weather could not communicate with the shore, even by boats. Men, then, treading water amid the mountain seas, carried communications to and fro in oilskin caps. I have heard it was the same at Madras. Young Gordon, apprentice to the sea, fell into mid ocean while fisting a sail. The poor fellow's heart sank when he saw the ship sailing away. But, as he afterwards told me, he trod water, and kept up till the boat reached him. I have trodden water again and again with a big boy on my back. Any one might do the same. Not one woman in ten thousand, not one man in a thousand, I suppose, can swim. They do not know they can tread water when they fall in, and of course drown, as two fine young women who had got a little out of their depth in this place did last year. But ignorance and prejudice cannot always rule, and the day will surely come when human beings, better instructed, shall enjoy the same immunity in the water that other animals not human beings, now enjoy. HENRY MACCORMAC

Bournemouth, June

Buoyancy of Bodies in Water

A propos of the question of drowning, as the same is now raised in NATURE, and especially so as to the alleged "fact that men are very different in buoyancy," allow me to say that when stationed many years ago at Pembroke Dock, South Wales, two soldiers were drowned there within a few days of each other. One of these casualties occurred off an island named the Stack Rock, in Milford Haven, that was garrisoned by invalided artillery, while the other took place in the creek that separates the town and dockyard from the huts. In the former instance the body of the (drowned) man remained floating upright in the water, "bobbing up and down with every wave"—as an eye-witness assured me—for a considerable time, or until it was lost to sight or recovered (I forget which just now). In the latter the body—that of a healthy, muscular man—was picked up a day or so afterwards by a passing boat as it was floating out with the tide to sea; and I have since seen several fresh bodies floating in the Ganges. Indeed the survivors always attach weights to the remains of even the poorest of their kindred ere they deposit them in that sacred stream; but this may be for the purpose of counteracting the current; and it is, I think, generally assumed in books and courts of law that all bodies, human and bestial, sink as a rule in water as soon as life is extinct; in other words, it is stated that they remain submerged till decomposition sets in, or sets up such an amount of gas within them as enables them to overcome all resistance from above, and float. If such be the case we must either suppose that the corpses referred to within possessed some special attributes of their own, or that "men are very different in buoyancy" after death than they were during life. Assuredly these men could not have been lost in this way had their bodies been able to float in the one state as well as they were in the other; and I heartily agree with Mr. Hill when he says that "no amount of coolness or presence of mind will either supersede the art of swimming or alter the laws of gravity."

Ashton-under-Lyne W. CURRAN

Resonance of the Mouth-Cavity

THE observation of Mr. John Naylor, forwarded to you by Mr. Sedley Taylor (p. 100), admits of being made with more striking (because louder) results than by the method described, and so far from being a "discovery," is well known to most schoolboys. Tap with the thumb-nail upon the front teeth, and at each tap alter the shape of the mouth-cavity so as to produce the note desired; any tune may then be played loud enough to be heard at the other end of a large room. It is remarkable that without previous practice one instinctively shapes the mouth-cavity so as to produce, in almost every case, the exact note required. GEORGE J. ROMANES

Thunder Storm at the Cape

A YOUNG man of my acquaintance, who some time ago joined the Cape Mounted Rifles, has just forwarded to me an account of a severe storm which occurred on the evening of Thursday last, April 14. C. TOMLINSON

Highgate, N., June 13

"The storm set in about 6 p.m., whilst the men were at stables, and was accompanied by loud thunder and vivid flashes of lightning. At 6.15 there was a fearful roll of thunder, accompanied by a most vivid flash, which lit up the square for at least thirty seconds. It struck the barracks at the upper end, ran past a room to the stables, which have iron roofs; it ran along the course of the roofs into the stables, striking down two men in the doorway. It then ran along the iron of the manger, flooring all the horses, nineteen in all, and so went to ground. One man was struck in the left shoulder bone, the fluid passing from there under the left arm to his watch in the left-hand trousers pocket, and burnt a hole clean through the silver case. From the watch it struck again six inches below, and travelled round the leg under the knee, and from thence probably to the spurs, as no burn was found below the knee. The extremities of both tracks were marked by large burns, and each track by a burn two inches over. The surgeon says it was the most miraculous escape he ever saw, the watch having saved the man's life. The second man was merely stunned, and lost the use of his legs for some hours; he was standing in the stable behind the first, and although only slightly burnt, is still unable to walk. The other is doing well, but is rather dazed. Ten other men were floored, but soon regained their legs. As to the horses, one was struck dead in the forehead: two others, blind in both eyes, were shot yesterday; and four more blind in one eye are condemned. A horse in town was struck, and his fore-leg broken in four places.

"Within a hundred yards of the barracks is a powder magazine full of powder, fitted with conductors which were struck several times. This occasioned great alarm to the inhabitants, as it contains many tons of powder.

"JOHN P. CUNNINGHAM

"King William's Town, South Africa, April 18"

A Six-Fingered Family

IT may interest some of your readers to hear that there is at present living in Brown's Town, Jamaica, a family in whom the possession of six fingers has been hereditary for at least four generations. Unfortunately they consider the sixth finger a deformity, and always amputate it, so that there is very little opportunity of observing it. There is a little girl there however upon whom this operation has not been performed, and I much regret that, as her parents had taken her up into the hills to work in their provision grounds, I could not see her. As I am informed, the sixth finger springs from the little finger knuckle at right angles to the little finger, and when it is free of it, it turns up parallel to the rest, being a little shorter than the little finger, but quite perfect, with nail and two joints. It is bent and extended with the rest on opening or closing the fist.

Another fact, which I daresay however is usual in such cases,

came under my notice at Brown's Town, viz. two perfectly black parents having a family all pure albinos.

Kingston, Jamaica, May 26 THOMAS CAPPER

Singular Behaviour of a Squirrel

A NEIGHBOUR of mine, whose cottage is thickly surrounded with trees, observed a squirrel, during the severe weather of winter, occasionally stealing food from the troughs set out for the poultry. At first it caused great commotion among the birds, but latterly they were less uneasy in its presence. Taking an interest in the wild creature he began to lay out refuse food for it, including bits of ham, which it greedily appropriated. Getting more courageous, it ventured within doors. After a time it got caught in a trap set for rats underneath the bed. Being freed from its irksome position it was thought that the squirrel would venture no more within doors. Neither the incident of the trap nor confinement for some time within a cage availed to restore to it its original shyness. With the coming of summer its visits have been less regular, but occasionally it looks in still. May not a habit like this, affecting only one out of many, be looked upon as corresponding to a "sport" in the vegetable world, and shed some light on the subject of the domestication of animals? The squirrel seems to have been quite a wild one to start with, for there is no one in the district who had been in the habit of keeping one as a pet. J. SHAW

Dumfriesshire

Hot Ice

IN reply to a very interesting letter on this subject recently published in NATURE (vol. xxiii. p. 504) by Dr. Oliver J. Lodge, I wish to express my views of the theoretical and practical possibility of the experiment of Dr. Carnelley. I wish to start from some well-known principles accepted by everybody acquainted with the mechanical theory of heat and its applications. According to these principles the volume "v" (and also the total amount of internal energy) of water can be expressed as a function of its pressure "p" and temperature "t"; $v = f(p, t)$. The form of this function, which we need not discuss here, will change with the state of aggregation, so that we shall have three different equations expressing the volumes of water in the solid, liquid, and gaseous form.

$$\left.\begin{array}{ll} v = f_{\mathrm{I}}(p, t) \; \ldots \; \ldots & \text{ice} \\ v = f_{\mathrm{II}}(p, t) \; \ldots \; \ldots & \text{water} \\ v = f_{\mathrm{III}}(p, t) \; \ldots \; \ldots & \text{vapour} \end{array}\right\} \text{p and t being considered independent variables.}$$

Geometrically the volumes of ice, water, and vapour will belong to three different surfaces extending between certain limits. Thus the surface $v = f_{\mathrm{I}}(p, t)$, which represents the volumes of ordinary ice, is situated between the limits $q\,p$, $l\,m$, $m\,d$; the surface representing liquid water lies between $m\,n$ and $m\,d$, though it may be extended a little on either side of these limits, if it applies to water heated or cooled over its regular boiling or freezing temperatures, which are situated along the lines $m\,d$ and $m\,n$.[1] The values of p and t, which belong to $m\,d$ and $m\,n$, will satisfy two equations—$\phi(p, t) = 0$ and $\psi(p, t) = 0$. At these points the water will change its form of aggregation and pass over in the state of saturated vapour along the line $m\,n$ [equation $\psi(p, t) = 0$], or into ice along $m\,d$ [equation $\phi(p, t) = 0$] in a continuous and *reversible* way. At any other point, which is not situated on $m\,n$ or $m\,d$, water may also be liable to change of aggregation, but this process will not be *reversible*. The line $m\,n$, where the surface $v = f_{\mathrm{II}}(p, t)$ breaks up and liquid water changes into vapour, is the *curve of tension of saturated vapour* contained in the renowned table of Regnault. The boiling-points of water under varying pressure are situated on $m\,n$, and may be found by solving the equation $\psi(p, t) = 0$. At the point m ($p = 4{\cdot}6$ mm., $t = -0^{\circ}{\cdot}0078$ C.) the line $m\,n$ terminates, but is continued by $l\,m$ [equation $\chi(p, t) = 0$], along which the vaporisation of ice takes place in a reversible way. According to the table of Regnault there is no sudden rupture at the point m, the pressure of saturated vapour at 0° C. being identically the same, if the vapour is in contact with water or with ice. The differential coefficients $\dfrac{dp}{dt}$ of the functions $\phi(p, t)$, $\psi(p, t)$, and $\chi(p, t)$, or the tangents to the lines $m\,d$, $m\,n$, and $m\,l$ are found by application of Carnot's Theorem to be of the general form $\dfrac{dp}{dt} = \dfrac{A\,r}{[s - s_1][273 + t]}$ [r = latent heat; s and s_1 = the specific volumes of water in two different forms of aggregation].

The point m, where $m\,n$, $m\,d$, and $m\,l$ unite, is of particular interest. J. Thomson called it "the triple point," and Guldberg the "Fällespunkt" of water. Lately (in *Berichte*, 1880) I ventured to call it the "absolute point of sublimation," not because I wished to introduce a new term for a well-known scientific object, but only to point out some important consequences of the phenomenon just then announced by Carnelley, of which Prof. Lothar Meyer of Tübingen had published an interpretation different from mine. This point m, situated $-0^{\circ}{\cdot}0078$ C. below the ordinary freezing-point of water, is really the upper limit of sublimation, because at any higher temperature ice first changes into water before it evaporates. At $-0^{\circ}{\cdot}0078$ C., where the boiling- and melting-point of water coincide, a real sublimation of ice begins, provided that the barometric pressure does not exceed 4·6 mm. (= "the critical pressure" of Carnelley).

Now according to the discovery of Dr. Carnelley, ice at pressures lower than 4·6 mm. would exist by temperatures up to +178° C. Thus the surface $v = f_{\mathrm{I}}(p, t)$, which we have hitherto supposed to be inclosed between the limits $q\,p$, $q\,l$, $l\,m$, $m\,d$ would extend far beyond $l\,m$ nearly up to k, but always at pressures smaller than 4·6 mm. Geometrically this new and unforeseen

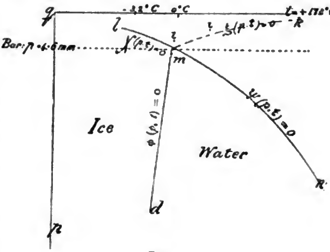


FIG. 1.

extension of the surface of ice is represented by the area $l\,m\,k$. Here the process of Carnelley, whereby ice of low pressure is heated to astoundingly high temperatures, would go on. The area $l\,m\,k$ would of course be entirely a *terra incognita* to the science of the present day, but there is nevertheless no theoretical objection why the surface of ice $v = f_{\mathrm{I}}(p, t)$ should not extend farther than to the limiting line $l\,m$ pointed out by Regnault. Confiding in the experimental proofs already furnished by Dr. Carnelley, I concluded (*Berichte*, 1880): if the surface of ice really extends upwards to about +178° C. there must be a limiting line $m\,k$ to the area $l\,m\,k$, since this area cannot extend so far as to the dotted line in the figure indicating the critical pressure = 4·6 mm. At this new limit, $m\,k$, corresponding to an equation $\xi(p, t) = 0$, the vaporisation of the "hot ice" may go on in a reversible way, just as liquid water gives up saturated vapour at those pressures and temperatures which belong to the line $m\,n$ (equation $\psi(t, p) = 0$). The line $m\,k$ would in many respects be the continuation of $m\,d$ (just as $m\,l$ forms the continuation of $m\,n$), but naturally the symbols entering the equation of its differential coefficient $\dfrac{dp}{dt} = \dfrac{A\,r}{(s - s_1)(273 + t)}$ must change their signification on the other side of the point m, so that r here would represent the latent heat of *vaporisation* of the hot ice, s its specific volume, &c. I did not expressly mention this in my paper in the *Berichte*, because I thought it unnecessary. This omission on my side may probably have misled Dr. O. Lodge as to the real meaning of my words, since he declares my opinion that an equation $\xi(p, t) = 0$ having a differential

[1] The surface corresponding to the volumes of aqueous vapour $v = f_{\mathrm{III}}(p, t)$ is not sketched in the figure, which gives only *the projection* of the surfaces on the plane of co-ordinates p and t, not the real situation of these surfaces in space. The reader will also observe that the limiting lines $m\,n$, $n\,d$, $l\,m$, $m\,k$ are the *intersections* of vertical cylindrical surfaces ("Uebergangsflächen") with the plane p, t.

coefficient $\frac{dp}{dt}$ of the general form above mentioned will still exist for pressures below 4·6 mm. and temperatures higher than −0·0078° C., where it is now supposed to have a *point d'arrêt*, to be "naturally erroneous," an assertion which I hope the learned English inquirer may feel inclined to withdraw after the preceding explanation. Of course the necessity of my conjecture may be disproved by facts—if there really should exist no "hot ice"—but nevertheless it deserves to be discussed as well as the opinion of Dr. Lodge, who considers the existence of hot ice to depend entirely on an irreversible process of vaporisation from the ice resembling the evaporation of water in an atmosphere which is not saturated with damp. This observation only regards the experiment, not the theory. I fully admit that Dr. Carnelley's experiment is carried on in an irreversible way, but that is the case with every distillation or sublimation which is *practically* performed. Nevertheless there exists a line *m n* where the liquid water changes gradually and reversibly into saturated vapour, and that may be the case also with the hot ice at the limit *m k*. Any irregularity in the operation will not exclude the possibility of the existence of an equilibrium established by nature. The difference of temperature between the hot ice in the experiment of Carnelley and the cooled vacuum bottle is no objection to this, because we *might* carry on the operation in quite another way, dispense with the vacuum bottle and the cooling mixture, and keep up the necessary minimum of pressure, which is the only *sine quâ non*, by means of a powerful air-pump. In a similar experiment (with $HgCl_2$) Dr. Carnelley operates in that way. Dr. Lodge, on the contrary, is convinced that ice, which has once passed the triple point *m* can sustain whatever augmentation of pressure and temperature may be applied to it. We may destroy the vacuum, so scrupulously kept up by all experimenters, and allow the air to enter through hot pipes, nevertheless the ice will not melt. I interpret this in the following way:—Dr. Lodge admits that the surface $v = f_1(p, t)$ extends over the limit *l m*, and even surpasses the critical pressure 4·6 mm., the vaporisation of the hot ice going on irreversibly the whole time.[1] This is indeed an interesting hypothesis, which well deserves to be tested by experiments, but yet lacks any foundation from facts. I therefore think that the proper method of resolving the entire problem would be:—

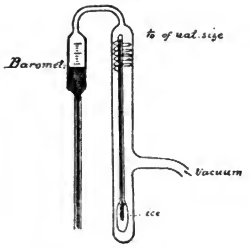


Fig. 2.

1. To try (by experiment) if ordinary ice under low pressure by sufficient supply of heat can be made to pass over the limit *l m* and assume higher temperatures than those corresponding to the equation $\chi(p, t) = 0$ (or Regnault's table).

2. If this should be the case it remains to ascertain if the vaporisation of the "hot ice" tends towards any limit (*m k*), where this process becomes reversible, saturated vapour being formed [my conjecture], or if there is no such limit [theory of Dr. Lodge].

The apparatus employed (see Fig. 2) differs from those recently used by Messrs. Boutlerow, McLeod, L. Meyer, &c., only by its combination with a barometer, by means of which the variation of the pressure of the vapour given up by the ice during the whole process could be exactly measured. The only drawback to this was that the barometer of the apparatus did not *instantly* indicate the variation of the pressure, because the *upper* part of the barometer was made of a very wide glass tube to avoid the influence of capillarity. The effect of the vacuum, which consisted of a 4-litre glass bottle, was very powerful, since the full heat of two strong gas-lamps, each furnished with three pipes, must be employed on the outside of the glass tube in order to raise the temperature of the ice covering the bulb of the thermometer from − 15° C. or − 11° C., up to 0° C. The result of the experiment was (the ice being heated *only by radiation* from the glass tube):—By intense heating the temperature of the ice slowly (in about six minutes) rose from − 11° C. to 0° C., when it became *constant* for half a minute. Then the ice melted, and the first drop of water falling upon the bottom of the heated glass tube was sufficient to crush the apparatus. During the process of heating the niveau of the mercury in the barometer-tube constantly fell, the internal pressure augmenting as the temperature of the ice rose. It was quite impossible to raise the temperature of the ice without simultaneously augmenting the pressure.

	Experiment I.	*Experiment II.*
The ordinary barometer showed	= 756·8 mm.	= 771·1 mm.
The barometer of the apparatus showed	= 755·0 mm.	= 769·5 mm.
The initial pressure in the apparatus	= 1·8 mm.	= 1·6 mm.
The initial temperature of the ice	= − 11°·0 C.	= − 15° C.
By heating the temp. rose to = − 8°; the press. = 1·5		*t* = − 9° p. = 1·8 mm.
" " " = − 6°; " = 2·90		*t* = − 6° p. = 2·6 mm.
" " " = − 4°; " = 3·5		The mercury in the stem of the thermometer separated by the heat.
" " " = − 0°; " = 4·0		
The ice melted.		

Table of Regnault.

Tension of saturated vapour at—

$t = -20°$ C.; $p = 0·927$
$t = -15°$ C.; $p = 1·700$
$t = -10°$ C.; $p = 2·093$
$t = -5°$ C.; $p = 3·113$
$t = -0°$ C.; $p = 4·600$

I also repeated the experiment of Mr. Hannay by substituting a little sealed tube containing frozen water under atmospheric pressure, instead of the bulb of the thermometer. I found, in accordance with Mr. Hannay, that the enveloping ice melted before the ice in the tube.

After the experiments published by Messrs. McLeod, Boutlerow, L. Meyer, v. Hasselt, de la Rivière, and Hannay, I think it may be considered as a matter of fact that ordinary ice under low pressure cannot be heated over 0° C. In the experiments I. and II. I vainly tried to raise the temperature of the ice without simultaneously augmenting the tension of the vapour in the apparatus. It seems probable therefore that the area corresponding to $v = f_1(p, t)$ does not extend farther than to the limit *l m* [equation $\chi(p, t) = 0$], since the temperature of the ice and the tension of its vapour vary almost exactly in the ratio given by Regnault's table, which in Fig. 1 is represented by the line *l m*. We may conduct the heating of the ice so as to follow almost continuously the line *m l* [Experiment I.] without ever being able to pass over it or to reach temperatures situated beyond *l m* (*i.e.* in the area *l m k*). Still I think these experiments to be strictly convincing only in the case of *ordinary ice*. Nobody has yet repeated Dr. Carnelley's experiment exactly in the same way as Dr. Carnelley himself. In his experiment the ice on the bulb of the thermometer is formed not by the freezing of a quantity of water, but by the sublimation and condensation of icy vapour to thin layers. It *may* be possible that ice, by sublimation under low pressure, changes into another allotropic modification, just as the red modification of HgI_2 is changed into yellow iodide by sublimation. In this case we may foresee the existence of a new surface, $v = f_{iv}(p, t)$ on the other side of *l m*. For, according to the principles of the theory of mechanical heat, there ought to be a new function, $v = f_{iv}(p, t)$ for every new allotropic modification of a body which, geometrically, is represented by a surface (?? in the figure). We are scarcely authorised to deny the possibility of the existence of hot ice, since Dr. Carnelley has obtained several pieces of ice, which

[1] *Or:* Dr. Lodge supposes that the volume of ice which has once passed the limits, beyond which liquid water cannot exist, is totally *independent* of the temperature and pressure. In this case no theory can be applied to account for the existence of hot ice, because every theory must start from the assumption that there exists a certain relation between the variables *v*, *p*, and *t*, and that the volume of ice, as long as it *is* ice, is not *arbitrary*, but regulated by an equation $v = f(p, t)$. Therefore I do not think that this explanation can be in accord with the views of Dr. Lodge.

did *raise* the temperature of the calorimeter. I have tried to repeat this experiment, but I never could obtain the whole bulb of the thermometer entirely covered with sheets of sublimated ice, and without this the experiment will be illusory. Many times I obtained lozenges of sublimated ice, which did adhere very strongly to the bulb of the thermometer, until it showed + 35° or + 40° C. Then the lozenges generally fell off. I do not consider this to be any deciding proof, as it may depend on a phenomenon similar to that of Leidenfrost, nor do I think it very probable that ice really can exist at those temperatures, but if that should be the case the simplest manner to account theoretically for the existence of hot ice would be to assume a new allotropic modification, since it may be regarded as a matter of fact that *ordinary ice* cannot be heated over the limits pointed out by Regnault. If this should be the case I think that the importance of the discovery of Dr. Carnelley could hardly be overrated.

Upsala, May 28 OTTO PETTERSSON

TEMPERATURE OF RAIN-WATER.—"A Subscriber" asks where he can find records of the temperatures of rain-water when falling, and of the earth a few inches below the surface, during any or all the months of the year.—As regards the British Islands, the most extensive and long-continued observations on the temperature of the soil are those published by the Scottish Meteorological Society since 1857. A *résumé* of the first five years' observations was published in the Society's *Quarterly Report* for October-December, 1862. In the Society's *Journal* (vol. i. p. 320) is a discussion of valuable series of observations made on the temperature of drained and undrained land at various depths; also in *Journal* (vol. ii. p. 273, and vol. iii. p. 211) discussions of hourly observations on the temperature of the soil and of the air at different stations in Scotland. With respect to the temperature of falling rain, little, if anything at all, quite satisfactory, has been accomplished, the practical difficulties in the way being either not apprehended by the observer or not satisfactorily disposed of. Our correspondent may also consult with advantage the publications of the various Continental organisations for the prosecution of forest meteorology.—ED.

NOTES ON ALGÆ[1]

THE publication of the second part of Bornet and Thuret's volume on Algæ seems a fitting opportunity to notice it in some little detail. While the First Part, published in 1876, treated chiefly of the red Algæ, by far the larger portion of this Part treats of the Nostocs; while the First Part contained a good deal of the notes of Thuret, the present is practically the work of Bornet, and the drawings are in almost every instance from this author's pencil.

Under the modest title of Notes, we find in this handsome quarto volume, of over a hundred pages and twenty-four plates, in addition to notes on the higher Algæ, a most exhaustive treatise of a very interesting group of simple Phycochromaceous plants.

The illustrious Thuret had laid the foundation of a knowledge of the Nostocs; his friend Bornet has built thereon a very solid structure. He has not attempted to write a complete monograph of the group, including therein all the "book" species, but having had access to most of the published collections of dried Algæ, to the collections of the Paris and Dublin Herbaria, and to the original types of de Brebisson, Lenormand, Montague, Harvey, Grunow, and Le Jolis, he has performed wonders in the way of clearing up a most tangled synonymy.

It might shock the nerves of some botanists to recommend that all defective descriptions of Algæ—of which no original type specimens exist—ought to be overlooked. We believe, however, that it would be for the advantage of science that such a step should be taken. We may here mention that the collection of Dr. Hassall, from which most of the drawings of that author's "History of British Freshwater Algæ" were made, has been long since dispersed; so far as the Nostocs are concerned, this does not much matter, as all the species were described from authentic specimens still attainable.

The Nostocs (this name is traceable to Paracelsus—"Sic etiam quicquid aër gignit et ex aëre est vivitque vel oritur ut Tereniapin, Nostoch," &c.—and yet no one seems to know its meaning) are well-known plants. One common species makes its appearance on lawn or garden walks in summer or autumn in the form of olive green (rarely bluish), irregular, and more or less shining masses. It is strange to hear the guesses that are made as to the nature of these. We have had them sent to us as the "peculiar spawn of earthworms," and again as the eggs of some foolish frog that had mistaken dry land for water. Some species delight in moist banks over which water continually trickles; some live a wholly aquatic life on stones in streams. The species have an enormously extended, but not yet accurately defined geographical area. As to size, they vary much, some being scarcely visible to the unassisted vision, some forming masses as large as the upper joint of one's thumb.

The details of such a volume as the one before us are too special to be of general interest, so we rest satisfied with indicating the chief contents. The genus Nostoc is treated of very fully; the reproduction of the species by hormogones and by spores is well illustrated. Instead of the term trichome, we would have preferred that of filament, for the former has now obtained such a common usage in another sense among botanists. Despite a wonderful uniformity in their structure, the spores seem to furnish good diagnostic differences. It is unfortunate that they are not as yet known in all the species, while in some they are difficult to hit off. Twenty-nine species are formulated. Carefully-conducted culture experiments, carried on over four years, have proved that Nostoc cells found within the cells of aquatic plants (Potamogeton, &c.) will develop into regular Nostoc colonies, which latter have been traced to the spore-producing stage.

Four species of the genus Nodularia are described and figured. This genus is better known under its more familiar title of Spermosira. *Nodularia litorea* is a somewhat remarkable species. In July, 1874, M. Crié was commissioned to make an inquiry into the cause of a noisome smell proceeding from the little lake of Deauville (Calvados). It would appear that for some years this district had been a regular focus of maladies, and those living near it had remarked that the fœtid odour perceived at times came from a reddish matter which periodically covered the surface of the water. M. Crié soon found that this consisted of ruddy masses of this Nodularian, spreading over the surface of Ruppia, and that its periodic decomposition—at the moment of greatest heat and lowest water—was the cause of the stench. The perfect remedy was found in guiding a stream through the little lake or pond, and thus preventing the too rapid growth of the Alga.

Of the other genera treated of we must mention Lyngbya, Plectonema (for *Conferva mirabilis* of Dillwyn), Scytonema (twenty-one species, of which a provisional analysis is given. Some twenty-one species (?) are included under *Scytonema thermale* (Kutz.), and a very important Appendix gives a list of plants determined NOT to belong to the genus, though referred to it); Calothrix (several of our native species are figured and described); and lastly, Glœotrichia (of which six species are admitted).

Enough has been written to prove how valuable an addition to our works on the lower algal forms this volume is. To the worker on this group—ever increasing in interest—this contribution to our knowledge of it will be very welcome. Such will call to mind, too, that there are still lower and more confusing forms of these Algæ, and will be glad to hear that it is probable that the same patient and clever hand hopes shortly to have reduced even them to something like order. E. P. W.

[1] Notes Algologiques: Recueil d'Observations sur les Algues, par M. Ed. Bornet et G. Thuret. Deuxième fascicule. Paris, 1880. G. Masson.

PENNSYLVANIA OIL REGIONS

THREE years ago the Second Geological Survey of Pennsylvania, under the able leadership of Mr. J. P. Lesley, published a report on the oil-well records of the State—a laborious compilation by Mr. J. F. Carll. During the interval the value of this report has been duly tested and acknowledged. It is a treasury of facts classified and indexed for the guidance of the compiler of statistics, the well-sinker, the mining engineer and the geologist, while the general reader may learn much of interest from its pages. Another report by the same author is just about to appear. It forms a volume of about 500 pages with two indexes, twenty-three plates, and an atlas of twenty-two sheets of maps, well-sections, and working drawings of machinery and tools. We have been favoured with advance-sheets of the Letter of Transmission prefixed to the report by Mr. Lesley, from which we make the following extracts:—

"The main feature of the report is the settlement of the true character of the Venango oil-sand group as a distinct and separate deposit, with characteristic marks distinguishing it from the Palæozoic formations of a preceding and a succeeding age; the differentiation of the group into three principal and other subordinate layers of gravelly sand, holding more or less oil or gas; the local variability of these sands, their singular persistency beneath long and narrow belts of country, their change into barren shales elsewhere, and their independence of other oil-bearing sands and shales of an earlier and of a later date."

Some characteristically caustic remarks are made as to the consequences of the contempt entertained by "practical" men for what they consider the "theoretical" opinions of geologists, and a flagrant example is given of the results of trusting to mere empirical guidance. These passages ought to be well studied by oil-men in Pennsylvania and Canada. Mr. Lesley goes on to relate an incident in his own experience. "In 1841," he says, "I was ordered by the chief of the First Geological Survey to report on the counties lying along the New York State line, and down the eastern bank of the Allegheny River, as far as the Kiskiminitas. Other assistants on that survey had already discovered and reported the geological structure of the Allegheny River and Beaver River water basins, and the rate of descent of the rocks southward and south-westward in relation to tide level had been calculated. My business was to follow and locate upon the map the anticlinal and synclinal rolls which locally change and modify this general dip, and to identify the principal coal beds over a large area.

"After the discovery of petroleum (which of course did not in the least set aside or essentially change the structure of Western Pennsylvania as established by the First Survey), I happened to be employed by the Brady's Bend Company to examine their property, and to give them, among other items, an opinion upon the probable existence and depth of oil beneath it. To do this, I merely did what any geologist who had thoroughly studied that country would have done; I calculated the vertical distance from the oil sand on Oil Creek up to coal A, then I calculated the dip of the measures between Oil Creek and Brady's Bend, and then I identified coal A at Brady's Bend. I reported that the Venango oil sand, *if it extended under ground as far as Brady's Bend*, ought to lie at 1100 feet beneath water-level. Any geologist who knew the country could have done this. It required no genius, no uncommon knowledge, nothing but a plain, simple, systematic, or scientific, in other words, true theoretical method of applying known facts for discovering the unknown. Any oil-man could have done the same if he had noticed the rocky layers as he went up and down the river, and put this and that carefully together.

"Yet, when after a few months, oil was actually struck at Brady's Bend within a few feet of the depth which I had assigned to it, the astonishment of all classes of oil-men was ludicrously extravagant; a score or two of copies were made from the manuscript report, and these copies passed from hand to hand as precious things, and their author was looked upon as a prodigy of mental penetration, and was offered large sums of money to locate wells in different districts, none of which offers, of course, were accepted, because he was as ignorant of the *actual existence of an oil-bearing sand in any given locality* as everybody else.

"The story has its moral. Let 'practical' men believe in and respect the slowly, carefully reached conclusions of 'theoretical' men enough to take them into consideration, so far as to comprehend them, and to govern themselves by them in their own collection and collation of facts relating to their own pecuniary interests."

Notwithstanding the amount of detailed information now collected regarding the occurrence of the liquid hydrocarbons in these ancient American formations, it must be frankly confessed that we seem to be as far as ever from a clue to their source and history. "The origin of petroleum," says Mr. Lesley, "is still an unsolved problem, and Chapter 26 of this Report merely suggests queries respecting it. That it is in some way connected with Palæozoic sea-weeds, the marks of which are so infinitely abundant in the rocks, and with the infinitude of coralloid sea-animals, the skeletons of which make up a large part of the limestone formations which lie several thousand feet beneath the Venango oil-sand group, scarcely admits of dispute; but the exact process of its manufacture, of its transfer, and of its storage in the gravel beds, is utterly unknown. That it ascended rather than descended into them seems indicated by the fact that the lowest sand holds oil when those above do not, and that upper sands hold oil where they extend beyond or overhang the lower. The chemical theory, so-called, which looks upon petroleum as condensed from gas, the gas having been previously distilled from the great black shale formations (Marcellus and Genessee), must face the objection that such a process, if chemically possible, which is doubtful, ought to have distributed the oil everywhere, and permanently blackened and turned into bituminous shales the entire thickness of this part of the earth crust, several thousand feet thick. It fails to explain the petroleum obtainable from the Cannel coals, and from the roof shales of bituminous coal beds. And it fails also to explain the entire absence of petroleum from immense areas of not only shales, but sand and gravel rocks equally underlaid by the Marcellus and Genessee formations."

One of the most generally interesting questions in the report is one discussed in great detail by Mr. Carll—an episode in the history of the glacial period in North America. Certain oil-bearing river-gravels are connected with a very thick "deposit of Canadian rock fragments not only upon the surface, but to the depth of several hundred feet beneath it in Northern Pennsylvania, a deposit which forms a great belt, more than a thousand miles long, across the continent from Cape Cod in Massachusetts to Iowa and Minnesota beyond the Mississippi River. It was brought from the north by a vast sheet of moving ice which filled the great lakes and rode over the highest mountains to the south of them—burying all New England and New York, Northern New Jersey, Northern Pennsylvania, the Western Reserve in Ohio, and large portions of the States lying further west—projecting long tongues or slowly moving torrents of solid ice southward as far as and even beyond the Ohio River in Kentucky. It drove slowly before it the reindeer, musk ox, caribou, moose, and other Arctic animals whose bones are found in the diluvial clays of the Kentucky caves; while the walrus inhabited the shores

of the Atlantic as far south as the Ashley River in South Carolina. The Esquimaux race no doubt accompanied these animals into the Gulf States, just as it did in France as far south as the Pyrenees. By the deposit of this vast pile of moraine matter, sand, clay, scratched rocks and huge boulders, the valleys by which our rivers had previously flowed into Lake Erie were filled up so that the waters were turned southward into the Ohio."

"*THOUGHT-READING*"

THE public mind has of late been somewhat agitated by the doings of a Mr. Bishop, who has come before the world of London society in a capacity no less startling than that of a professed reader of thought. Armed with a favourable letter of introduction from Dr. W. B. Carpenter, he has not only taken by storm the general public and daily press, but also succeeded in convening an assembly of scientific men to witness his performance, which in point of numbers and importance resembled in miniature a *soirée* of the Royal Society, while still more recently he has had the honour of exhibiting his powers before the Heir Apparent to the Crown. There is no doubt that Mr. Bishop owes this wide and sudden celebrity to the patronage which was extended to him by the great opponent of all humbug; and although Dr. Carpenter doubtless intended his letter to exert a salutary influence by recommending Mr. Bishop to the attention of the credulous, it is to be regretted that it served to recommend him also to the attention of the scientific. This is to be regretted, because the result was to endow the powers which were afterwards exhibited with a fictitious degree of importance in the eyes of the public, and also to bring a large number of distinguished men into the somewhat undignified position of acting the stalking-horse to Mr. Bishop's notoriety. But however this may be, it seemed to Prof. Croom Robertson worth while to make a more careful trial of Mr. Bishop's powers than was possible in the first crowded assembly, and he therefore invited Mr. Francis Galton, Prof. E. R. Lankester, and myself, who were all present on the first occasion, to join him in an investigation. When we had assented to the proposal, Mr. Bishop was invited to meet us at Prof. Croom Robertson's house. He immediately accepted the invitation, and it is but just to state that throughout the investigation which followed he placed himself entirely in our hands, and with the utmost good nature submitted to all our requirements. He professes that he is himself ignorant of his *modus operandi*, and merely desires that this should be adequately investigated and satisfactorily explained.

Two meetings were arranged. At the first, which was held on May 28, Prof. Lankester was not able to attend, and his place was taken by Mr. Leslie Stephen. Mr. Alfred Sidgwick was also present. At the second meeting, held on June 11, there were present as before, Prof. Croom Robertson, Mr. F. Galton, and myself, but Mr. Leslie Stephen and Mr. Alfred Sidgwick were absent, while Prof. Lankester was present. The room in which both meetings were held was a double drawing-room of the ordinary shape of those which usually have folding-doors; here however the folding-doors were absent. The extreme length of the room was 36 feet, the width of its front part was 19 feet, and of its back part 12 feet.

First, Mr. Bishop was taken out of the room by me to the hall down stairs, where I blindfolded him with a handkerchief; and, in order to do so securely, I thrust pieces of cotton-wool beneath the handkerchief below the eyes. In all the subsequent experiments Mr. Bishop was blindfolded, and in the same manner. While I was doing this, Mr. Sidgwick was hiding a small object beneath one of the several rugs in the drawing-room; it having been previously arranged that he was to choose any object he liked for this purpose, and to conceal it in any part of the drawing-room which his fancy might select. When he had done this the drawing-room door was opened and the word "Ready" called. I then led Mr. Bishop up stairs, and handed him over to Mr. Sidgwick, who at that moment was standing in the middle line between the two drawing-rooms, with his back to the rug in question, and at a distance from it of about 15 feet. Mr. Bishop then took the left hand of Mr. Sidgwick, placed it on his (Mr. Bishop's) forehead, and requested him to think continuously of the place where the object was concealed. After standing motionless for about ten seconds Mr. Bishop suddenly faced round, walked briskly with Mr. Sidgwick in a direct line to the rug, stooped down, raised the corner of the rug, and picked up the object. In doing all this there was not the slightest hesitation, so that to all appearance it seemed as if Mr. Bishop knew as well as Mr. Sidgwick the precise spot where the object was lying.

This is Mr. Bishop's favourite experiment; so I may give some of our other observations relating to it before passing on to the variations which we introduced. It was soon found that he succeeded much better with some of us than with others; so at the second meeting, in order to make a numerical comparison, he was requested to try two experiments with each of the four persons who were present. With Mr. Galton, Prof. Robertson, and Prof. Lankester he failed utterly, while with myself he succeeded once perfectly and the second time approximately. For on the first occasion I concealed a pocket-matchbox upon the top of a book behind the leather lap of a book-shelf. After feeling along the rows of books for some time he drew out the one on which the matchbox was lying. In the second experiment I placed a visiting-card on the key-board of a grand piano and closed the cover. After going about the room in various directions for a considerable time he eventually localised the piano, and brought his finger to rest upon its upper surface about six inches from the place where the card was lying. It will thus be seen that his success with me, although so much better than with any of the other three persons present that evening, was not so immediate and precise as it had been with Mr. Sidgwick the evening before. It has also to be mentioned that in one of the experiments which he tried with Prof. Robertson the evening before, he was, after a good deal of feeling about, successful in localising a particular spot on an ordinary chair which Prof. Robertson had selected as the spot to be found. From this it will be seen that it made no difference whether a particular article or a particular spot was thought of; for if the subject thought of was a certain square inch of surface upon any table, chair, or other object in the room, Mr. Bishop, in his successful experiments, would place his finger upon that spot. Neither did it make any difference whether the article or place thought of was at a high or a low elevation. Thus, for instance, in one of the experiments I placed a small pencil-case high up in the chandelier of one of the drawing-rooms. There was first a great deal of walking about in various directions, examining tables, bookshelves, &c., so that it was thought that the experiment was about to prove a failure. (It may here be mentioned parenthetically that in all the experiments tracings were taken of the routes which Mr. Bishop traversed, but it seems needless to occupy space with recording the analysis o these results.) Then, while feeling over the surface of a table in the other drawing-room, and not far from the corresponding chandelier, Mr. Bishop suddenly pointed at arm's length vertically to the ceiling. He remained motionless in this position for a few seconds, and then set off at a brisk pace in a straight line to the other drawing-room, until he came beneath the other chandelier. As his finger was all this time pointing to the ceiling, it

touched this chandelier on his coming beneath it. He then stopped and pointed as high as he could, but not being a tall man, was not able to touch the pencil-case, which had been purposely placed above his reach. After satisfying ourselves that his determination to reach up at that particular spot could not be attributed to accident, but rather that his finger appeared to be smelling the object of his search, the experiment was concluded. As a rule, unless success is achieved within the first two or three minutes, it is never achieved at all; but in some cases, as in the one just quoted, after several minutes of feeling about in various places and directions, a new point of departure seems suddenly to be taken, and Mr. Bishop starts off straight to the right spot. As an instance of this I may quote another experiment, in which I placed a shilling beneath a sheet of paper lying on a table which was crowded with other articles. After going about the room in various directions for a considerable time, this table was reached, apparently by accident, and just at the time when I was thinking that the experiment would certainly prove a failure, Mr. Bishop suddenly became more animated in his movements, and exclaiming "Now I am within two feet of it," began to hover the point of his finger over the table, and eventually brought it down upon the sheet of paper just where the shilling was lying beneath.

Mr. Bishop can also very frequently localise any spot on his subject's person of which the subject may choose to think. As in all other cases he presses the hand of the subject upon his forehead with one hand, and uses the other as a feeler. Here again he succeeds much better with some persons than with others, and the persons with whom he succeeds best are the same as those with whom he does so in his other experiments. Thus he altogether failed with Mr. Galton, although the latter, in order to fasten his attention the more exclusively on one particular spot, pricked this spot with a needle. With Prof. Lankester success was partial; for while he thought of the point of his nose, Mr. Bishop was only able to say that the point thought of seemed to occupy the median line of the body on the front aspect. But on a previous occasion at Bedford Square Mr. Bishop localised correctly a pain (slight toothache) from which Prof. Lankester was suffering. With Prof. Croom Robertson success was better, though not quite perfect, for while the place thought of was the ball of the right thumb, Mr. Bishop localised it in the right wrist. In the only two experiments tried in this connection with myself the results were somewhat peculiar. In the first experiment I thought of a spot situated under the left scapula, and Mr. Bishop localised it as situated under the right: in the second experiment I thought of my right great toe-nail, and for a long time Mr. Bishop prodded round and on the left great toe-nail, though he eventually changed to the right one, and so localised the spot correctly. In both these experiments, therefore, it seemed that with me Mr. Bishop experienced a strong tendency to confuse symmetrically homologous parts.

From this brief summary of the results gained by following Mr. Bishop's own methods, it will be seen that on the whole his power of localising objects or places thought of by a person whose hand he clasps is unquestionably very striking. Of course the hypothesis which immediately suggests itself to explain the *modus operandi* is that Mr. Bishop is guided by the indications unconsciously given through the muscles of his subject—differential pressure playing the part of the words "hot" and "cold" in the childish game which these words signify. Mr. Bishop is not himself averse to this hypothesis, but insists that if it is the true one he does not act upon it consciously. He describes his own feelings as those of a dreamy abstraction or "reverie," and his finding a concealed object, &c., as due to an "impression borne in" upon him. But however this may be (and of course we had no means of testing the statement) all our experiments have gone to show that the hypothesis in question is the true one, and that Mr. Bishop owes his success entirely to a process of interpreting, whether consciously or unconsciously, the indications involuntarily and unwittingly supplied to him by the muscles of his subjects. Thus when his subject is blindfold and loses his bearings, failure results. Failure also results if the connection between Mr. Bishop and his subject is not of a rigid nature—a loose strap, for instance, being apparently of no such use to him for the establishment of connection as a walking-stick. Similarly, although he was very successful when he grasped my left hand when I did not know where the object was concealed, but when my left wrist was held by Mr. Sidgwick, who had concealed the object; he failed when, under otherwise similar circumstances, Mr. Sidgwick held my right hand—so establishing a limp instead of a firm connection through my person.

Lastly, a number of other experiments were tried, in deference to some statements which Mr. Bishop made concerning his occasional success in reading thoughts of a kind which could not be indicated by muscular contraction. From these experiments, it is needless to say, we did not anticipate any results; but (with the exception of Prof. Lankester) we thought it was worth while to make them, not only because Mr. Bishop seemed to desire it, but also to satisfy the general public that we had given the hypothesis of "thought-reading," as well as that of "muscle reading," a fair trial. The experiments consisted in the subject looking at some letter of the alphabet which Mr. Bishop could not see, and the latter endeavouring to read in the thoughts of the former what the letter was. Although this experiment succeeded the first time it was tried, it afterwards failed so frequently that we entertain no doubt as to the one success having been due to accident, and therefore conclude that if Mr. Bishop has any powers of "thought-reading" properly so-called, he has failed to show us evidence of the fact.

Deeming it a remarkable thing that such precise information as to a mental picture of locality should be communicated so instantaneously by unconscious muscular movement, we thought it desirable to ascertain whether Mr. Bishop, who is able so well to interpret these indications, is endowed with any unusual degree of tactile sensibility or power of distinguishing between small variations of resistance and pressure. We therefore tried the sensitiveness of his finger-tips with the ordinary test of compass-points, but found that he did not display more than a usual delicacy of tactile perception, while his power of distinguishing between slight differences in weights placed successively on a letter-balance concealed from his eyes was conspicuously less than that displayed by Prof. Croom Robertson. As Mr. Bishop is not opposed to the hypothesis by which we conclude that his results are obtained, there is no reason to suppose that he tried to depreciate his powers of tactile sensibility and of distinguishing between small differences of weight. In their main features Mr. Bishop's experiments are frequently performed as an ordinary drawing-room amusement, and we are therefore inclined to think that he does not enjoy any peculiar advantages over other persons in regard to sensitiveness of touch or power of appreciating pressure, but that his superior success in performing the experiments is to be ascribed merely to his having paid greater attention to the subject.

In conclusion, we desire to express our thanks to Mr. Bishop for the trouble which he has taken in submitting to the numerous experiments, the general results of which have now been stated.

This report has been read in proof by Prof. Croom Robertson, Mr. Francis Galton, and Prof. E. R. Lankester, and meets with their full approval.

George J. Romanes

THE WEATHER AND HEALTH OF LONDON[1]

II.

AN examination of the curve for the whole mortality (Fig. 4, NATURE, vol. xxii. p. 144) shows that the great preponderance of deaths in London takes place during the coldest months of the year. Of the diseases to which this excessive mortality is due, the first place must be assigned to diseases of the respiratory organs, the more marked of which are given in Figs. 12 to 15. About one in eight of all deaths that occur is caused by bronchitis, and one in sixteen by pneumonia; so that nearly one fifth of the deaths is occasioned by these two diseases of the respiratory organs. Our researches appear to warrant the conclusion that the greatest fatality from these diseases occurs when the temperature is between 32° and 40°. In New York, when the winter temperature is 10°·0 lower than in London, the mortality from bronchitis and pneumonia is greatly less; on the other hand, in Melbourne, where the winter temperature is about 10°·0 higher than that of London, the mortality from diseases of the respiratory organs forms but a small fraction of the whole deaths.

These four curves of the mortality from diseases of the respiratory organs are substantially the same, each having its maximum in the cold months and its minimum in the warm months. Asthma shows, in the amplitude of its annual range, the greatest sensitiveness to weather, and pneumonia the least. They all show, though in different degrees, a double-ridged maximum: the one ridge being in the middle of January, when the temperature falls to the annual minimum, and the other in March, when the combined qualities of cold and dryness are at the annual maximum. Asthma and bronchitis are decidedly at the maximum when the weather is coldest, whereas laryngitis has its maximum in March, when the weather is coldest and driest, the last disease thus forming the link connecting the more strictly throat diseases with diseases of the nervous system.

But an element of weather other than mere temperature plays an important part in bringing about the high death-rate from these diseases. That deleterious atmospheric influence is fog; and in cases where the fog is dense and persistent the mortality from diseases of the respiratory organs becomes truly appalling, as happened in London early in 1880, when the mortality was nearly doubled. An examination of the fogs of London shows that they do not commence till the autumnal equinox; and it is at this epoch that asthma (Fig. 12), by far the most sensitive of all diseases to fog, starts from its annual minimum; and in the end of November and begin-

[1] Substance of a Lecture delivered at the Royal Institution, March 25. Continued from p. 146.

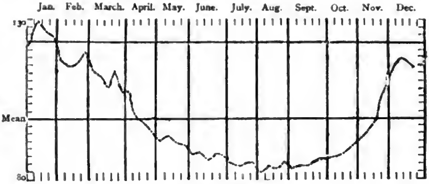


FIG. 12.—Asthma.

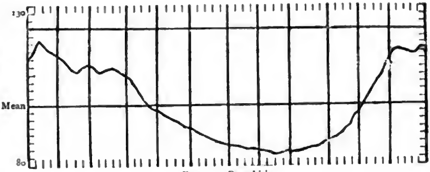


FIG. 13.—Bronchitis.

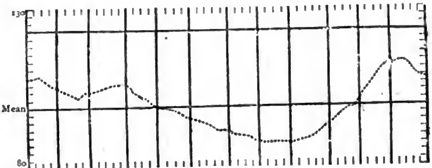


FIG. 14.—Pneumonia.

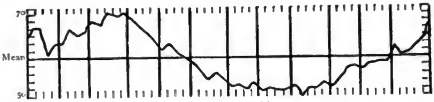


FIG. 15.—Laryngitis.

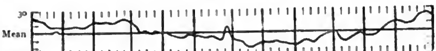


FIG. 16.—Apoplexy.

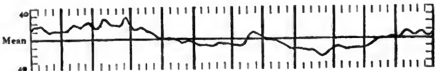


FIG. 17.—Convulsions.

FIG. 18.—Cephalitis.

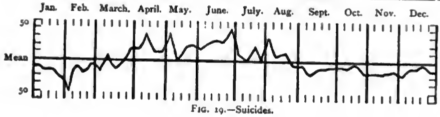


Fig. 19.—Suicides.

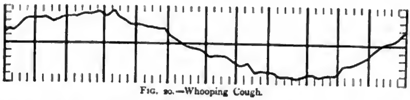


Fig. 20.—Whooping Cough.

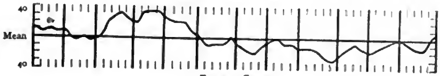


Fig. 21.—Gout.

Fig. 22.—Phthisis.

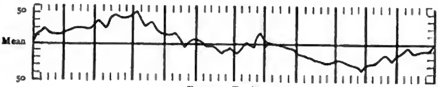


Fig. 23.—Teething.

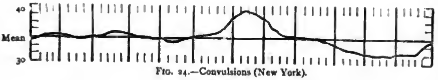


Fig. 24.—Convulsions (New York).

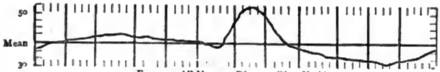


Fig. 25.—All Nervous Diseases (New York)

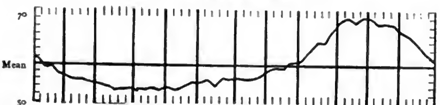


Fig. 26.—Scarlet Fever.

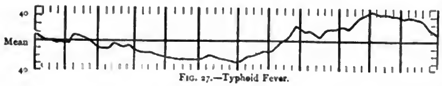


Fig. 27.—Typhoid Fever.

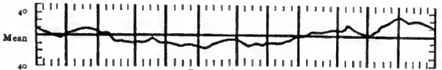


Fig. 28.—Diphtheria.

ning of December, when fogs become most frequent, the curves for asthma and bronchitis shoot up with startling suddenness.

Figs. 16, 17, and 18 represent the curves for three of the nervous diseases, viz. apoplexy, convulsions, and cephalitis. Apoplexy will be observed to show a double-ridged maximum quite analogous to that of the diseases of the respiratory organs; whereas, in the case of convulsions, the maximum may be regarded as quite single, and occurring in spring, this being the season when nervous diseases generally are most fatal. On the other hand, the curve for cephalitis stands alone among nervous diseases as having its annual maximum somewhat later, and keeping above the mean till at least the end of July, thus covering that portion of the year when the climate is driest and hottest, as well as driest and coldest. The intimate relations observed between the curve for suicides (Fig. 19) and that for cephalitis is very striking.

The maximum mortality for whooping-cough, Fig. 20, gout, Fig. 21, and phthisis, Fig. 22, occur in the same season as that for the nervous diseases. The maximum mortality from whooping-cough occurs in the spring months, and the curve suggests that this is more a disease of the nervous system than of the respiratory organs, a view which, singularly enough, was maintained by the elder Dr. Begbie, one of the most distinguished of our Edinburgh physicians, upwards of thirty years ago. The relations of gout to diseases of the nervous system are too obvious to call for remark. Phthisis is one of the two most fatal scourges of our British climate, one out of every eight deaths which occur being caused by consumption. Its mortality-curve, Fig. 22, shows unmistakably its intimate relations to nervous diseases, thus affixing greater significance to its known complications with hereditary insanity, scrofula, and some other mental diseases.

Reference has been made to the influence of the heat of summer on certain of the nervous diseases. That influence acts fatally, both indirectly through the bowels in the case of the young, and directly on the nervous centres. The curve for convulsions, Fig. 17, is identical with that for teething, Fig. 23, and it may be added that the curve for hydrocephalus is simply a reproduction of the same curves. Now these curves show a small, but distinct, and, as revealed by each year's figures, a constantly recurring secondary maximum in summer, which in the case of London is almost wholly due to the bowel complications of these diseases. The curve (Fig. 24) for convulsions for New York, where the summer temperature is 10°·0 hotter than in London, shows this feature of the curve enormously magnified, so much so, indeed, that instead of being, as in London, an insignificant secondary maximum, it stands out as the prominent feature of

the curve. Whilst this result is doubtless largely due to complications with bowel complaints, it is, as an examination of the statistics shows, in no small degree caused by the direct influence of the great summer heat of New York on the nervous centres. This is impressively shown by the mortality curve for the whole of the nervous diseases (Fig. 25), which is even more pronounced in this particular than the curve for convulsions alone (Fig. 24). Keeping this fact in view, the peaks showing an increased fatality in London from cephalitis (Fig. 18) and suicides (Fig. 19) during July and August acquire, in the eyes of the physician, a more impressive significance.

The curve for the whole mortality (Fig. 4, NATURE, vol. xxiv. p. 144) shows September and October to be two of the healthiest months of the year. The three curves, scarlet fever (Fig. 26), typhoid (Fig. 27), and diphtheria (Fig. 28), are the most striking exceptions to this, these curves all indicating either a large increase in the death-rate or a high mortality during these months. While closely related to each other, each of these three diseases has a distinct individuality of its own as regards the times of occurrence of the annual maxima and minima, and the varying amplitudes of their range from the mean line. It is a singular circumstance that diphtheria shows closer relations in its death-rate with typhoid than with scarlet fever.

Several other diseases suggest close alliances with each other through their seasonal death rates. Thus the curve for mortification is substantially that of nervous diseases, and the curves for erysipelas and puerperal fever are in all essential respects the same, a fact of singular suggestiveness to the family practitioner. The curve for old age is exactly parallel to that of paralysis, the old man's disease. The curves for skin diseases, rheumatism, dropsy, pericarditis, Bright's disease, and kidney disease exhibit most striking, and in many cases the closest alliances with each other. Lastly, while bowel complaints attain their greatest mortality when the temperature is highest, diseases of the respiratory organs when it is lowest, nervous diseases during the dry weather of spring and early summer, and skin diseases and certain fevers during the raw weather of autumn and early winter, such diseases as ileus, that are quite removed from weather influences, exhibit curves which show no obedience whatever to season, but only a succession of sharp, irregular serratures resembling the teeth of a saw.

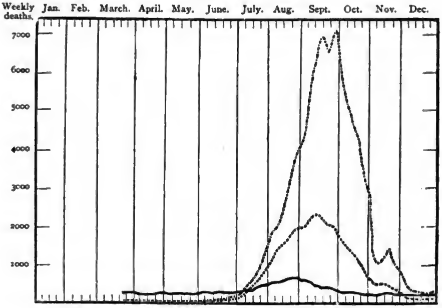


FIG. 29.—The Great Plague of London.

Atrophy and debility are most fatal to the very young in summer, but to the aged in winter; in the former case the complication being with bowel complaints, and in the latter with diseases of the respiratory organs. The annals of influenza show that a special character is given to this epidemic according to the season of the year in which it occurs. Thus when it occurs in spring the head and nervous system are most affected, but the bowels when the epidemic prevails in summer and autumn.

Fig. 29 shows by the doubly-dotted line, or highest curve, the weekly mortality of London during the Great Plague of 1665, the lower dotted curve the mean weekly mortality of the last six plagues, and the solid curve the mean weekly mortality from all other diseases during the continuance of the last six plagues. The manner in which the plague, as a death-producer, obeyed the weather is striking, and full of interest. It did so exactly in the way in which we have seen bowel complaints to be influenced by weather. The curve of mortality for the plague bears no resemblance whatever to that for typhus, or indeed any other disease except bowel complaints. The fact that the progress of deaths from plague in relation to weather resembles so closely the corresponding progress of deaths from bowel complaints raises the question whether there may not be a closer alliance between them than has been suspected. If we are correct in regarding such a question as a fair outcome of this investigation of the relations of weather and health, it results that such investigations may occasionally point to a seat of morbid processes which have been cloaked by prominent phenomena, apparently of a primary, but in reality of a secondary character.

ALEXANDER BUCHAN

NOTES

THE death of Sir Josiah Mason on the 16th inst., at the advanced age of eighty-six, closes a remarkable career. Born at Kidderminster in humble circumstances, he began life as a street hawker of cakes, and after trial of shoemaking, baking, and a variety of other things in his native place, he went to Birmingham and found employment in the gilt toy trade. In 1824 he set up on his own account as a manufacturer of split-rings by machinery, and he afterwards added the manufacture of stee pens, of which he became really the largest producer, though less known than Gillott and Mitchell, owing to his pens being supplied by Messrs. Perry of London. He shares the credit of perfecting the modern steel pen, the history of which practically

dates from the discovery of the art of splitting by machinery. Sir Josiah Mason also carried on for many years the business of electro-plating, copper-smelting, and india-rubber making, along with the late George Richard Elkington. While he was very much a self-taught man, his very liberal benefactions indicated his sense of the value of good education. Conspicuous among these is the Erdington Orphanage, established at a total outlay of 300,000*l.*; and the munificent gift to Birmingham of a Science College (the building of which cost 60,000*l.*, while the total value of the endowment is probably little short of a quarter of a million) is fresh in public memory.

OUR readers will learn with profound sorrow the loss which biological science has sustained in the death, at the comparatively early age of fifty-one, of one of its most brilliant and gifted cultivators, Prof. Rolleston of Oxford. He had spent the greater part of the winter in Southern Europe, his medical advisers having hoped that a warmer climate and rest from his incessant labours might have averted the malady with which he was threatened. All precautions however proved unavailing. He returned to England about a fortnight ago in a sinking state, and died at his home on Thursday, the 16th inst. We propose to give a sketch of Dr. Rolleston's scientific career in our next number. Immediately after the funeral a meeting of Prof. Rolleston's old pupils was held in the Museum, with the object of perpetuating his name by some suitable memorial. A committee was formed, with power to add to its number; the following gentlemen being elected honorary secretaries:—Dr. C. Mansell-Moullin, 17, George Street, Hanover Square, W., and Dr. Theodore Acland, St. Thomas's Hospital, S.E., London; Mr. E. B. Poulton, M.A., Wykeham House; and Mr. A. P. Thomas, M.A., Anatomical Department Museum, Oxford.

THE Council of Owens College, at their meeting on Friday, June 17, elected Dr. Arthur Schuster, F.R.S., to the Professorship of Applied Mathematics in Owens College. Dr. Schuster was a distinguished student of Owens College in 1870-71; he then proceeded to Germany, studying mathematics and physics under Kirchhoff, Weber, and Helmholtz. On his return he first occupied the position of Demonstrator in Physics at Owens College, lecturing on the Mathematical Theory of Electricity. Afterwards he continued his studies at Cambridge under Maxwell and Rayleigh, publishing several papers on the higher branches of physics. In 1874-75 he was intrusted with the conduct of the Government expedition to observe the total eclipse in Siam, the results of his observations being printed in the *Philosophical Transactions* for 1878. In 1878 he undertook a similar expedition to Colorado, and in the following year he was elected a Fellow of the Royal Society.

THE Davis series of lectures upon zoological subjects will be given in the lecture-room in the Society's Gardens, in the Regent's Park, on Thursdays at 5 p.m., commencing June 16, as follows:—June 16—Whales, by Prof. Flower, LL.D., F.R.S.; June 23—Dolphins, by Prof. Flower, LL.D., F.R.S.; June 30—Extinct British Quadrupeds, by J. E. Harting; July 7—The Limbs of Birds, Prof. W. K. Parker, F.R.S.; July 14—Birds, Ancient and Modern, by W. A. Forbes; July 21—Zoological Gardens, by P. L. Sclater, F.R.S.; July 28—Chameleons, by Prof. Mivart, F.R.S. These lectures will be free to Fellows of the Society and their friends, and to other visitors to the Gardens.

AMONG other features of the forthcoming meeting at York, the noble Guildhall is placed at the Association's use as reception room. The theatre of the Museum of the Yorkshire Philosophical Society has been granted for the Geological Section. The beautiful grounds, containing the ruins of St. Mary's Abbey, &c., will be open to members and associates. The Yorkshire Fine Arts Institution will also be open, and the great hall will be used for some of the evening meetings. The Minster will be thrown open for inspection. Excursions are being organised to several places of interest, including Scarborough (where the Spa Company give free admissions), Whitby, Castle Howard, and the works of Messrs. Bolckow, Vaughan, and Co., at Middlesborough.

THERE has been recently some talk of establishing at Athens or Smyrna an American Institute for the training of Archæologists, and as a permanent committee for archæological research and correspondence. Two institutes with like aims are at present in existence, viz. the German Institut für archäologische Correspondenz, having its seat in Rome, with a branch in Athens, and the École Française d'Athènes, which has a branch in Rome. Mr. Thomas Davidson describes the work of these in a recent issue of the *Nation*, and advocates Smyrna as the place for the American Institute, as offering a more promising field of research than Athens, while there would be a better prospect of getting any antiquities discovered for museums. The cost of such an institution is estimated at 5000 dollars to begin with, for a library and necessary apparatus, and about 6000 dollars a year afterwards.

THE fourth and fifth numbers of *Naturen* for 1881 contain interesting summaries, by Dr. Hercules Tornö, of the results obtained by him, during the Norwegian Arctic Expedition, of the depths of the Arctic seas; the amount of salt contained in the water at various depths and at different distances from land; and the variations observable in the relative quantities of the different constituents of the air contained in sea-water. In regard to the latter point, it may be observed that the mean amount of oxygen present in the air was found to diminish with increasing depth below the surface of the water from 35·3 at the surface, to 32·8 between 1000 and 1400 fathoms; while the relative quantity of the nitrogen present rose with the increased depth from 13·1 at the surface to 14 between 600 and 1000 fathoms. Carbonic acid was found both in a gaseous and basic form.

WE notice a very interesting lecture which has been given at St. Petersburg on the use of the heliograph during the Trans-Caspian war. The heliograph used in the Russian army is that of Mans, and the alphabet is the usual one, that of Morse. The smaller system, which is employed in cavalry, transmits signals to a distance of seventeen miles, and the larger, employed in forts, has a double power. All independent parts of the army, on their march to Akhal-Tepe, had their "heliograph-drafts," and owing to the bright sky of the steppe, and to the level country, the heliograph was continuously used for establishing communication between different parts and small detachments of the army. The heliograph was at work during all the battles, and experiments were made as to the use of it during night, by means of lunar light, as well as with special lamps. The latter, however, being fed with turpentine, which evaporates very soon during the hot days of the summer, did not render great service. It was observed also that the sight of those who receive the heliogram gets very soon fatigued, which occasions error. But altogether the heliograph has rendered very great services during this campaign.

THE French Minister of Postal Telegraphy recently sent to the several telegraphic offices forms for recording all the observations connected with thunderstorms. The forms have been drawn up by M. Mascart, the head of the Meteorological Office, and printed at its expense.

A REPORT has been presented to the Paris Municipal Council on the state of telephonic exchanges in Paris, and the propriety of putting a tax on them for the use of sewers in which the wires are located. The number of telephonic halls will be increased, and six of them will be established shortly, which will bring

their number to ten. At present the number of persons renting wires is a little more than 1000, and the average number of messages a little more than five a day for each.

M. MAREY has asked the Paris Municipal Council for the grant of a space of 1200 metres in the Park of the Champ de Mars for establishing a zoological station; but such a large space could not be afforded without inconvenience to foot-passengers, so he has accepted the grant of a space which was tendered to him at Passy, in the Park of Princes.

THE Congrès archéologique of France holds its forty-eighth session at Vannes (Morbihan) on June 28. Among other subjects to be discussed are the megalithic monuments of the Gulf of Morbihan, the chronology of sepultures, influence of soil on the distribution of megalithic monuments, bronze objects and other remains found in tombs of Brittany, Gallic and Roman coins, ante-Roman remains in Brittany.

TWENTY shocks of earthquake were reported from Szt. Ivan Zelina and Blazedotoc (Hungary) between May 20 and June 7. Some rather severe shocks also occurred on June 11 and 12, direction north-east to south-west.

THE Caucasian Museum at Tiflis is fast approaching completion under the active and energetic direction of Dr. Radde. The visitors from Western Europe, who are expected at the Archæological Congress, will already find a tolerably numerous collection of natural history objects and archæological specimens.

THE discoveries of remains of palæolithic man in Russia continue to be most interesting. Recently M. Shaposhnikoff discovered a great quantity of stone implements in the district of Valdai, where a forest has been cut down and the wind has denudated the sand of the subsoil. The implements belong to four categories: (1) knives and saws similar to those of Moustier, St. Acheul, and Solutre, more perfect than any found previously; (2) the same in miniature, most accurate, and made of the finest kind of flint; they might have been used as ornaments, or for tattooing; (3) figures of animals and men made in flint, and relief pictures of the same, also in flint; (4) ornamental designs on stone. The collection is very rich, especially in miniature implements.

A LITTLE book just published by the Kössling'sche Buchhandlung (Gustav Wolf) of Leipzig is named "Naturwissenschaftlich-mathematisches Vademecum," being an alphabetical and systematic compilation of all modern publications in the domains of natural sciences and mathematics.

AT the meeting of the South-Eastern Railway Company the other day Sir Edward Watkin announced the complete success of the preliminary borings of the Channel Tunnel, and the resolution of his own Company on this side and the French Company on the other to make a further important step. A gallery seven feet in diameter has already been driven from the shaft near Abbots' Cliff for half a mile towards France, and an agreement has been made to push forward a similar headway under the sea for a mile on each side of the Channel. At the present rate of progress this will probably be done within the next six months, and then it is expected that the further nine miles on each side will be undertaken at once. All the conditions seem favourable to the project. The soil is found to be exactly similar at both ends. It is, as was expected, grey chalk impervious to water; and there is every reason to anticipate that it will be found to stretch in an unbroken bed across the Channel. Last week the machinery excavated sixty-seven yards of lineal distance, equal to about two miles a year. At this rate the two headways might meet under the middle of the Channel in about five years; and probably a nearly equal period might be occupied in enlarging this mere seven-feet burrow to a capacious railway tunnel.

IT is proposed in Edinburgh to make a three months' trial of lighting Princes Street and the North Bridge as far as the Tron Church with the electric light, on the Brush system.

BIRMINGHAM has resolved to invite the British Association to hold their annual meeting for 1883 in that town.

THE last number of the *Journal de Physique* describes a set of registering electrometers and magnetometers which are being tried at the Collège de France. The magnetic bars and the apparatus generally are very small. The instruments have been invented by M. Mascart, who believes they will give trustworthy results, and will compare favourably with the large magnetometers used in Kew and other places.

THE past winter cold in Norway, between October, 1880, and March, 1881, has exceeded the normal mean by 7° C. The greatest cold yet registered at any of the Norwegian meteorological stations occurred between January 13 and 15. At Karasjok, the lowest temperature was observed on February 4, when the thermometer fell to $-50°\cdot6$ C., the lowest ever noted in Norway with trustworthy instruments.

A WORK on the Butterflies of Europe, illustrated and described by Dr. Henry Charles Lang, F.L.S., will shortly be published in about twenty monthly parts. It will give accurate coloured figures of all the species of Rhopalocera found in Europe, showing both the upper and under side where necessary, as well as the differences of sex, if requiring a separate figure; and the most important of the named varieties will, when possible, be also represented. Typical illustrations of larvæ and pupæ will from time to time appear. The figures will be drawn from specimens in the author's collection. The description of a species will, whenever possible, include a notice of its transformation, habitat, and times of appearance, along with the principal synonyms and necessary references. The arrangement and nomenclature will be mainly those of Dr. Standinger's well-known catalogue. Each part will contain four coloured plates and sixteen pages of letterpress. The cost is very moderate. The publishers are Messrs. Reeve and Co., of Henrietta Street, Covent Garden.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*) from South Africa, presented by Mr. Thornburgh-Cropper; four Harvest Mice (*Mus minutus*), British, presented by Mr. Henry Laver; a Banded Grass Finch (*Poëphila cincta*) from Queensland; two Yellow-bellied Liothrix (*Liothrix luteus*) from India, presented by Mrs. Hylton Joliffe; a Red-legged Partridge (*Caccabis rufa*), European, presented by Mr. Arthur Morrell, School Ship *Cornwall*; a Horrid Rattlesnake (*Crotalus horridus*) from Bahia, presented by Dr. A. Stradling, C.M.Z.S.; a Patas Monkey (*Cercopithecus ruber*) from West Africa, a Blue Jay (*Cyanocitta cristata*) from North America, purchased; a Rhesus Monkey (*Macacus erythræus*), a Cape Buffalo (*Bubalus caffer*), born in the Gardens; seven Australian Wild Ducks (*Anas superciliosa*), five Chiloe Widgeons (*Mareca chiloensis*), a Mandarin Duck (*Aix galericulata*), two Geoffroy's Doves (*Peristera geoffroii*), two Turquoisine Parrakeets (*Euphema pulchella*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE SOLAR PARALLAX.—At the sitting of the Paris Academy of Sciences on the 6th inst. M. Tisserand communicated a note received from Mr. Todd of the office of the *American Ephemeris* at Washington, giving the value of the solar parallax deduced from the photographic operations of the American expeditions, as detailed in the "General Discussion of Results," a volume which has just been issued. The number of photographs is 213, distributed over various stations thus:—

Northern Hemisphere.		Southern Hemisphere.	
Wladiwostok	13	Kerguelen	8
Nagasaki	45	Hobart Town	37
Pekin	26	Campbelltown	32
		Queenstown	45
		Chatham Island	7

Putting dA for the correction of the difference of R.A. between the Sun and Venus, dD for the correction of the difference of declination, and $d\pi$ for that of the assumed value of the parallax (8″·848) the solution of the equations of condition corresponding to the distances gives—

$$dA = +1''\cdot181 \ \ldots \ dD = +2''\cdot225 \ \ldots \ d\pi = +0''\cdot0397 \pm 0''\cdot0418.$$

Similarly from the equations corresponding to angles of position there result—

$$dA = +1''\cdot109 \ \ldots \ dD = +0''\cdot637 \ \ldots \ d\pi = +0''\cdot0252 \pm 0''\cdot0595.$$

Combining these values, the final results become—

$$dA = +0^s\cdot076 \ \ldots \ dD = +2''\cdot083 \ \ldots \ d\pi = +0''\cdot035 \pm 0''\cdot034,$$

and the corrected value of the solar parallax is thus 8″·883. It will be seen that the magnitudes of the probable errors are very large in proportion to the corrections obtained.

BIELA'S COMET IN 1805.—Those who have acquainted themselves with the history of this remarkable comet will remember that at its appearance towards the end of 1805 it was last observed in Europe by Thulis at Marseilles on the evening of December 9, at which time it was at its least distance from the earth, and was moving rapidly southwards. The comet was then visible to the naked eye, and it was known that it would probably continue so for some days, and might attract attention in the other hemisphere. But there was no southern observatory in existence at that time. Prof. Hubbard, in his masterly investigation on the motion of Biela's comet, remarked how greatly observations taken in the southern hemisphere at this appearance might have contributed to the progress of the theory of the comet. Gauss was then applying his methods for the determination of elliptic orbits, or rather of orbits without the assumption of a particular conic section, and it could hardly have happened that with good southern observations the nature of this comet's orbit would not have been detected from the observations of 1805, and the comet might thus have been re-observed in 1812, if not in 1819.

Up to a quite recent date it was not known that the comet had been observed in the other hemisphere, but Prof. Winnecke has discovered (where he does not say) some observations made at the Mauritius by MM. Malavois and Dupeloux on December 14 and 15. They were brought to light while he was inquiring into the periodicity of the comet detected at Strassburg by Dr. Hartwig on September 29, 1880, and he gives an account of them in the last number of the *Vierteljahrsschrift der Astronomischen Gesellschaft*. They are entitled "Observations sur la Comète qui a paru à l'Ile de France dans le cours de frimaire au 14ᵉ par M. Malavois et M. Dupeloux." M. Malavois states that he was apprised of the appearance of the comet on the morning of December 14. Four days previously (and therefore the day after it was last seen in Europe) it had been detected by MM. Laprie and Dabadie, the one a censor, the other a professor of the colonial Lycée: they had remarked that it had passed in a very short interval "over the space between the constellations Grus and Pavo, moving almost from north to south." On the evening of December 14 Malavois "avec un excellent sextant à lunettes d'un pied de rayon," measured the distance of the comet from Achernar and Canopus, and the distance between the stars, and Prof. Winnecke has been at the trouble to reduce these observations accurately, and to compare them with Hubbard's elements with the aid of an ephemeris calculated therefrom by Herr Kaufmann. Referring to the above-mentioned periodical for particulars, we may say that Hubbard's orbit is found to place the comet too much to the west by 13′ in right ascension, and in declination one minute to the north. But while giving these differences between calculation and observation, Prof. Winnecke only aims at proving that Biela's comet was recognised in the southern hemisphere, and well observed, considering the means available.

The description of the comet's appearance given by Malavois is worthy of particular remark:—"Cette comète est photosphérique, c. à d. entourée d'une sphère lumineuse dont le diamètre m'a semblé être en totalité d'environ 45 minutes: mais la partie la plus lumineuse, ou l'auréole vue au commencement du crépuscule du soir n'étoit guère, que de 20 à 25 minutes; on la distinguoit alors très bien, tandis qu'à peine on pouvoit apercevoir les étoiles de 7ᵐᵉ grandeur. La comète vue avec une lunette qui grossit seize fois le diamètre des objets m'a paru divisée en deux par une petite band obscure; j'ai jugé son diamètre apparent d'environ une minute, mais les bords m'ont semblé confusément terminés et se foudre avec la lumière nébuleuse; une étoile de 4ᵉ à 5ᵉ grandeur que je distinguais très bien dans l'auréole à 4 ou 5 minutes de la comète, s'est trouvée dans une position et une distance différente par rapport à cet astre dont le mouvement propre étoit en effet considérable, comme on la voir." Malavois' observations were limited to the evening of December 14, absence preventing his observing it on the following night, and clouds interfering on December 16 and 17; and he adds: "les jours suivans j'ai cessé de l'apercevoir."

Prof. Winnecke draws attention to the remarkable appearance of the comet under a power of 16, in the evidently small telescope, and asks: "Can we recognise in the 'petite bande obscure,' which divided the head into two parts, the commencement of the action which led to the highly important result, that in 1846 and 1852, instead of a single comet, two were observed, and later the comet has been no longer visible as such." In connection with the Mauritius observation it must be remembered that the aspect of the comet was particularly noted on December 8, when it was nearer to the earth than on December 14, by Olbers, Bessel, and Gauss, who agree in their description: on the same evening it was examined and measured by Schroeter with powerful reflecting telescopes at Lilienthal. Neither observer has any reference to the appearance of a division in the head of the comet on that date. Olbers says "it had a small but very distinctly defined nucleus, surrounded by an extensive nebulosity, without any appearance of a tail." The comet was visible to the naked eye with the brightness of a star of the third or fourth magnitude, and could be well seen after the moon had risen. Schroeter noted that without the telescope it appeared nearly as large as the moon: in his 13- and 15-feet reflectors it was apparently much diminished, his measures giving a diameter of only 5½ minutes: the diameter of the brightest part of the nucleus he found to be 4″·05, and that of the whole nucleus 6″·42; if a division had existed at the date of these measures it is hardly probable that it would have been overlooked by Schroeter. At the reappearance of the comet in 1826, and again in 1832, nothing of the kind was remarked. Hubbard, we know, considered that the division of the comet, from whatever cause it might be produced, took place at the end of the year 1844.

GEOGRAPHICAL NOTES

THE hydrographical expedition for the exploration of the mouth of the Obi River has started from St. Petersburg. It consists of four officers of the navy, one astronomer, M. Fuss, and two students of the St. Petersburg University, one of whom is a surgeon and the other a zoologist. Two small steamboats were sent to Perm, on the Kama River, and they will be transported to the Obi.

HERR SIEGFRIED LANGER of Vienna is about to undertake an exploring tour in Arabia under the auspices of the Vienna Geographical Society. His researches will be mainly of a linguistic nature, but scientific research is not excluded from his programme; he has prepared himself for the tour during the last few years at the Vienna University.

THE geographical department of the British Museum during the past year was deprived of the services of its able curator, Mr. R. H. Major, through ill-health, and on his retirement the opportunity was taken to reduce it to a sub-department. If we may judge by the newly published report of the British Museum, this change has not tended to increase activity in geographical matters, and among the most interesting acquisitions of the past year all that can be mentioned seem to be some old plans of towns and the like.

IN the last volume of Consular Reports S. de Zuccato at Venice furnishes an interesting map showing the lines proposed and in course of construction for the completion of the network of railways in Venetia. M. Consul Pernal also contributes a plan of the Havre docks.

IT is announced that the Portuguese travellers, Capello and Joens, are about to publish an account of their expedition under the title of "De Benguella as terras d'Iacca." The work will be

issued in parts, and when complete, will form two volumes, illustrated with engravings.

HERR SCHÖLER, who was sent out by the German African Society, has returned to Zanzibar after founding a station at Kikoma.

THE Italian traveller, Piaggia, returned to Khartum on April 30. It is believed there that he will be appointed governor of the Fashoda district, and that the Austrian, Maruo, will become governor of the province of the Blue Nile.

THE Scientific Commission, recently despatched from Paris, has arrived at Zanzibar on its way to examine M. Paiva's vast concession in the Zambesi region, which it is proposed to develop by means of a company. The Commission is to investigate the resources of the territory, chiefly with regard to the mineral wealth supposed to exist there.

PHYSICAL NOTES

A FEW months ago the phenomenon of the "passive state" of iron was examined by M. L. Varenne, who attributed it to the presence of a film of nitrous acid gas upon the surface of the metal. The question has been recently reinvestigated by M. E. Bibart, who finds reason to doubt M. Varenne's conclusions. M. Bibart states that any oxidising agent aids, and any deoxidising agent hinders, the production of passivity.

IN a long memoir presented to the Académie des Sciences by Edmond and Henri Becquerel some very valuable data are given respecting the fluctuations of underground temperatures during 1880 beneath different surfaces. Their observations extended to a depth of 36 metres. The fluctuations were of less extent beneath herbage than below a bare soil, the maxima and minima being more retarded and of less amplitude in the former case. Another interesting point is the protecting effect of a bed of snow. Though the temperature of the air fell to $-15°$, and continued below 0° for long periods, that of the surface of the soil was rarely below $-1°$, never below $-1°.5$.

ACCORDING to Nies and Winkelmann, who have lately studied the expansion exhibited by bismuth, cast-iron, and other metals during their solidification, the specific gravity of bismuth is between 1·031 and 1·0497 times as great in the liquid as in the solid state; a sample whose (solid) density was 10·2 assumed a density of 10·77 when melted. The ratio of the density in liquid state to that in solid state was greater than unity also for the metals tin and zinc, the ratio for tin being 1·0070, and for zinc 1·002. Our readers will doubtless recall the recent experiments of Mr. Wrightson and Prof. Chandler Roberts in the same direction.

HERR STUCKER concludes from experimental inquiry (*Wied. Ann.* No. 5), that the gases chlorine, bromine, and iodine, in regard to thermal behaviour, form a group by themselves among biatomic gases. The ratio of the kinetic energy of the progressive motion of the molecules to the total energy is different for them from that for the others. In their molecules the atoms seem to have a different reciprocal action. From the behaviour of biatomic gases it is inferred that neither Boltzmann's nor Maxwell's supposition as to the nature of the mobility of atoms in the gaseous molecule has a general validity.

WITH regard to the subject of hot ice, Herr Wüllner describes fresh experiments (*Wied. Ann.* No. 5), and he finds that so long as the thermometer-bulb is wholly surrounded with dry ice its temperature does not reach 0°. If the thermometer rises higher, either the bulb is no longer quite covered with ice, or it is surrounded with water, along with a thicker ice-layer. The author's method was to have the thermometer-bulb first coated with ice in a separate vessel; then introduced into the heating-tube and fixed in a caoutchouc stopper; this tube is connected through a tube and spherical vessel with the air-pump, and with the sphere is surrounded with a cold mixture while the vacuum is produced.

THE subject of double refraction of light in moving frictional liquids has been taken up anew by Herr Kundt (*Wied. Ann.* No. 5), using a method which Maxwell did not succeed with, viz., rotation of a cylinder within another cylinder, and sending a beam of polarised light in axial direction through liquid in the annular space. Herr Kundt got positive results in this way with various liquids. 1. The amount of internal friction of liquids is not a certain measurement of the occurrence of double refraction in motion; liquids with small friction giving considerable refraction, and *vice versâ*. 2. The liquids which, with small internal friction, prove doubly refractive, belong to the so-called colloids (gelatine, gum, collodion) or the oils. Solutions of crystalloids did not give the phenomenon by the method described. 3. The double refraction did not markedly affect the rotation of the plane of polarisation in the circularly polarising liquids (but the strongest refraction, it is to be noted, produced a difference of only about half a wave in penetrating a pretty long column of liquid). 4. In collodion-solutions the axes of the double refraction do not lie in the azimuths required by theory. The anomaly was not accounted for. Herr Kundt further offers some general remarks on the relations between the elastic properties of liquids, their coefficients of friction, and the double refraction developed in them.

IT has been hitherto supposed that light directly reflected from a diffraction-grating has the same state of polarisation as light passing through the same plate unruled, or reflected from its smooth surface. Herr Fröhlich now finds, with a very finely-ruled grating, that it is not so. The proof and numerical amount of the difference are indicated in *Wied. Ann.* No. 5.

IN the cold of last winter M. Damien (*Journ. de Phys.*, May investigated the indices of refraction of water under 0° (*i.e.* in surfusion) down to $-8°$. He measured the indices corresponding to the three hydrogen lines by the prism method. Starting with a temperature of $+20°$, he first confirmed M. Jamin's observation that the passage through the maximum of density does not at all disturb the course of the indices, and he further found that the indices continue to increase below zero, though the density diminishes. The variations of the indices are very small. M. Damien hopes, next winter, to apply the interferential method. (The use of freezing mixtures does not present such favourable conditions as the very slow cooling of the atmosphere.)

RECENT researches by Herren Sohncke and Wangerin on Newton's rings (*Wied. Ann.* Nos. 3 and 4) appear to require a considerable change of ideas as to this phenomenon, and more especially as to the place where interference occurs. The starting-point was an experiment in which the rings produced by a beam of parallel sodium light falling at an angle on a horizontal plate above a plane convex lens were examined with a microscope inclined at the same angle, and capable of being moved horizontally as well as in the direction of its axis. The microscope was first so placed that one part of a dark ring was as sharply defined as possible; the instrument being then moved along to another ring, or another part of the same ring, it was found necessary to move it axially, higher or lower, to get the maximum definition for that part; indicating that the rings do not lie in a horizontal plane, but in some other position. The amounts of axial displacement for different parts of the ring-system were carefully noted. For details of the results we must refer to the original, merely noting, *inter alia*, that the places of interference in the plane of incidence going through the centre of the rings seem to lie in a straight line rising towards the side whence the light comes. In a central plane at right angles to that of incidence, all the places are at the same depth. Herr Sohncke undertook the experimental part in this investigation, while Herr Wangerin has worked out the theory of the phenomena.

ACCORDING to experiments by Herr Kundt (*Wied. Ann.* No. 4), the common surface-tension between liquid and gas decreases considerably with increasing pressure of gas in the case of alcohol, ether, alcoholic solution of calcium-chloride, sulphide of carbon, chloroform, and water. The decrease is greater at low pressures than at high. For a given liquid it varies with the nature of the gas compressed. With alcohol, ether, and alcoholic chloride of calcium solution, air causes a greater decrease of the capillary constant than hydrogen. The decrease is so great with some liquids (*e.g.* ether in air) that probably, with pressures reached without much difficulty, the surface-tension is *nil*, the liquid passing at ordinary temperature into the Cagniard de la Tour state. (The author's experiments were concluded before he knew of Cailletet's experiment, in which a mixture of five vols. of CO_2 and one vol. of air is compressed at a low temperature till the meniscus of CO_2 disappears, and the Cagniard de la Tour state is reached.)

THE ratio of intensity of the two sodium lines has been estimated by Herr Dietrich (*Wied. Ann.* No. 4) using apparatus of great dispersion with a Vierordt double slit giving one spectrum above another, and allowing of displacement, so that one

of the two lines in one spectrum is brought directly over the other in the other spectrum. The photometric parts included a Nicol capable of rotation, a right and left rotating double quartz, and a fixed Nicol. The mean value of the ratio sought, from measurements on three days, was 1·60 ± 0·01; the probable error of an observation being ± 0·032.

THE amount of electric expansion in caoutchouc has been investigated by Herren Korteweg and Julius (*Wied. Ann.* No. 4). They used tubes of white vulcanised caoutchouc made insulating in water by being kept twenty-four hours in oil; the tube was filled with water and placed in a water-bath (to form a condenser), while hydrostatic pressure was varied, and the changes of volume were measured by means of a connected tube-system. The change of volume is shown to be proportional to the square of the striking distance, and inversely proportional to the square of the thickness of tube-wall. It is the same whether the inner liquid be charged negatively or positively. The maximum was reached when the Holtz machine, left to itself, gradually slackened speed till the last spark passed. A table, giving also Quincke's data for glass, shows that both for this and for caoutchouc the volume-changes are, roughly speaking, in inverse ratio of the coefficients of elasticity, so that they must be ascribed to the same cause.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—During the Long Vacation there will be a course of lectures in the University Laboratory by Mr. Fenton, one of the Demonstrators, on Organic Chemistry. The Laboratory will be open for practical work.

During the Michaelmas Term there will be twelve courses of lectures on Chemistry and various branches of Physics, including one by Mr. Shaw on the Conservation of Energy and Theory of Unitation, and a course by Prof. Lewis on the more Common Mineral Rock-Constituents, and another on Descriptive Crystallography by the same.

Prof. Dewar's subject will be Physical Chemistry; and among other advanced lectures will be a course on the General Principles of Chemistry by Mr. Pattison-Muir.

Prof. Hughes and Mr. Tawney will divide between them the work in Geology, Mr. Tawney taking Palæontology in the Michaelmas Term.

Lectures on Botany will be given by Messrs. J. W. Hicks, Vines, Saunders, and Hillhouse. Dr. Vines' lectures in the Michaelmas Term will be on the Physiology of Plants, with practical work, at Christ's College.

Prof. Newton and the Demonstrator of Comparative Anatomy, Dr. Michael Foster, and his corps of lecturers, Prof. Humphry, Mr. Creighton, and Mr. Balfour, will give their usual series of lectures and demonstrations during the Michaelmas Term. Mr. Lea will give advanced lectures on Digestion and Chemical Physiology, and Mr. Langley will lecture on the Histology and Physiology of Muscle, Nerve and the Nervous System.

Dr. Bradbury will lecture on Pathological Anatomy, Prof. Latham on General Therapeutics, Prof. Paget on Clinical Medicine, and Mr. Carver on Clinical Surgery.

The Natural Sciences Tripos, Part I., under the Old Regulations, has just been completed, and the pass-list contains the names of thirty-three men, and three are excused the General Examination. The second part of this examination takes place in December.

The first part of the examination under the New Regulations, by which men can enter for the examination in their second year if they prefer, has just resulted in the publication of a list with five names, in alphabetical order, in the first division, viz. Messrs. Daniels (Trinity), Duncan (Caius), Earl (Christ's), Sherrington (Caius), and Wilberforce (Trinity).

The special examinations in Natural Science for the ordinary B.A. degree have yielded seven men in the first class in Chemistry, and eighteen in the second class. In Geology there was but one man in the first and one in the second class; in Botany, one in the first class; in Zoology, one in the second class.

At Trinity College the prizemen in the June examination in Natural Science were: Third year, Hillier and Ritchie; second year, Daniels and Wilberforce; first year, Davis, Head, Ransom, Thompson. The prizemen at Christ's College are Shipley (first year), Earl (second year), and Parkyn (third year).

The Chancellor of the University (the Duke of Devonshire) has (with the concurrence of Earl Cairns, Chancellor of the University of Dublin) declared that the statutes of Cambridge do not preclude the University from using the Previous and the Tripos Examinations for the purpose of testing the proficiency of women, as sanctioned by the Senate on February 24, 1881.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 19.—"Molecular Magnetism," by Prof. D. E. Hughes, F.R.S.

1. *Influence of an Elastic Torsion upon a Magnetic or an Electric Conducting Wire.*—In my paper of March 7th on "Molecular Electro-Magnetic Induction," I showed that induced currents of electricity would be induced in an iron wire placed on the axis of a coil through which intermittent currents were passing, and that these currents were produced only when the wire was under the influence of a torsion not passing its limit of elasticity. It became evident that if the intermittent magnetism induced by the coil produced under torsion intermittent currents of electricity, an intermittent torsion under the influence of a constant current of electricity or a constant magnetic field would produce similar currents. This was found to be the case, and as some new phenomena presented themselves indicating clearly the molecular nature of the actions, I will describe a few of them directly relating to the subject of this paper.

The apparatus used was similar to that described in my paper of March 7th. An iron wire of 20 centims. was placed in the centre or axis of a coil of silk-covered copper wire, the exterior diameter of the coil being 5½ centims., having an interior vacant circular space of 3½ centims. The iron wire is fastened to a support at one end, the other passing through a guide, to keep it parallel but free, so that any required torsion may be given to the wire by means of a connecting arm or index. A sensitive telephone is in direct communication with the coil, or a galvanometer may be used as the currents obtained by a slow elastic torsion are slow and strong enough to be seen on a very ordinary galvanometer. I prefer, however, the telephone, because it has the inestimable advantage in these experiments of giving the exact time of the commencement or finish of an electric current. It has, however, the disadvantage of not indicating the force or direction of the current; but by means of the sonometer the true value and direction of any current is at once given. The current from a battery of two bichromate cells is sent constantly through the wire if we wish to observe the influence of the torsion of the wire upon the electric current, or a constant field of magnetic energy is given to the wire by either a separate coil or a permanent magnet. The currents obtained in the coil are induced from the change in the molecular magnetism of the wire, but we may equally obtain these currents on the wire itself without any coil by joining the telephone and rheotome direct to the wire; in the latter case it is preferable to join the wire to the primary of a small induction coil, and the telephone and rheotome upon the secondary, as then the rheotome does not interrupt the constant electric current passing through the wire. As the results are identical, I prefer to place the telephone on the coil first named, as the tones are louder and entirely free from errors of experimentation.

If we place a copper wire in the axis of the coil we produce no effect by torsion, either when under the influence of a constant magnetic field or a current passing through it, nor do we perceive any effects if we place an iron wire (2 millims. in diameter), entirely free from magnetism, and through which an electric current has never passed. I mention this negative experiment in order to prove that all the effects I shall mention are obtained only through the magnetism of the wire. If now I pass an electric current for an instant through this same wire, its molecules are instantly polarised, and I have never yet been able to restore the wire to its original condition, and the magnetisation induced by the passage of a current is far more powerful and more persistent in soft iron than tempered steel. This may be due, however, to the fact that in tempered or softened steel we find traces only of a current during the rotation by torsion of its molecules some two to three degrees of sonometer, whilst iron gives constantly a current of 70 sonometric degrees.[1]

In order to obtain these currents, we must give a slight torsion of 5° or 10° to and fro between its zero point. We then have a current during the motion of the index to the right, and a contrary current in moving the index to the left. If we use a

[1] 0·8 of a Daniell battery.

galvanometer, we must time these movements with the oscillations of the needle; but with the telephone it gives out continuous sounds for either movement, the interruptions being only those caused by the rheotome. The direction of the current has no influence on the result; either positive to the free arm or index or negative gives equal sounds, but at the moment of reversal of the current a peculiar loud click is heard, due to the rapid change or rotation of the polarisation of its molecules, and this peculiarly loud momentary click is heard equally as well in steel as in iron, proving that it is equally polarised by the current, but that its molecular rigidity prevents rotation by torsion.

If a new soft iron wire of two millims. (giving no traces of a current by torsion) has passed through it a momentary current of electricity, and then the wire observed free from the current itself, it will be found to be almost as strongly polarised as when the current was constantly on, giving by torsion a constant of fifty sonometric degrees. If, instead of passing a current through this new wire, I magnetise it strongly by a permanent magnet or coil, the longitudinal magnetism gives also 70° of current for the first torsion, but weakens rapidly, so that in a few contrary torsions only traces of a current remain, and we find also its longitudinal magnetism almost entirely dissipated. Thus there is this remarkable difference, and it is that whilst it is almost impossible to free the wire from the influence produced by a current, the longitudinal magnetism yields at once to a few torsions. We may, however, transform the ring or transversal magnetism into longitudinal magnetism by strongly magnetising the wire after a current has passed through it; this has had the effect of rotating the whole of the molecules, and they are all now symmetrical with longitudinal magnetism, then by a few torsions the wire is almost as free as a new wire; and I have found this method more efficacious than heating the wire red hot, or any other method yet tried. If I desire a constant current from longitudinal magnetism, I place at one of the extremities of the wire a large permanent magnet, whose sustaining power is five kilometres, and this keeps the wire constantly charged, resembling in some respects the effects of a constant current. The molecular magnetism or the current obtained by torsion is not so powerful from this, my strongest magnet, as that produced by the simple passage of a current, being only 50 sonometric degrees in place of 70° for that due to the passage of a current. The mere twisting of a longitudinal magnet, without regard to the rotation of its molecule having no effect, is proved by giving torsion to a steel wire strongly magnetised, when only traces of a current will be seen, perhaps one or two degrees, and a constant source of magnetism or electricity then giving no measurable effect.

If we magnetise the wire whilst the current is passing, and keep the wire constantly charged with both magnetism and electricity, the currents are at once diminished from 70° to 30°. We have here two distinct magnetic polarisations at right angles to each other, and no matter what pole of the magnet or of the current the effect is greatly diminished; the rotation of the two polarities would now require a far greater arc than previously. The importance of this experiment cannot as yet be appreciated until we learn of the great molecular change which has really occurred, and which we observe here by simply diminished effects.

If we heat the wire with a spirit flame, we find the sounds increase rapidly from 70 to 90, being the maximum slightly below red heat. I have already remarked in my previous paper this increased molecular activity due to heat, and its effects will be more clearly demonstrated when we deal with the sounds produced by intermittent currents.

2. *Influence upon the Molecular Structure of an Iron or Steel Wire by Electricity or Magnetism.*—Being desirous to modify the apparatus already described, so that it would only give indications of a current if they were of a spiral nature, the wire was kept rigidly at its zero of strain or torsion, and the coil was made so that it could revolve on an axis perpendicular to the wire; by this means, if the wire was free from strain, the centre or axis of the coil would coincide with that of the wire. Thus, with a straight copper wire, we should have a complete zero, but if this wire formed a right or left-handed helix, the coil would require moving through a given degree (on an arbitrary scale) corresponding to the diameter and closeness of the spirals in the helix; the degrees through which the coil moved were calibrated in reference to known copper helices. 50° equalled a copper wire 1 millim. diameter, formed into a helix of 1 centim. diameter, whose spiral turns were separated 1 centim. apart.

In order to obtain a perfect zero, and wide readings, with small angular movement of the coil, it is necessary that the return wire should be of copper, 2 millims. diameter, offering comparatively little resistance, and that it should be perfectly parallel with the steel or iron wire.

The rheotome is joined to a battery of two bichromate cells, and by means of a reversing switch, an intermittent current of either direction can be sent through the wire. The telephone is joined direct and alone to the coil, thus no currents react upon the coil when perpendicular to the iron, and its return wire, if not of a spiral nature.

Placing an iron wire 0·5 diameter, and passing a current through it, I found a change had taken place similar to those indicated in my paper of March 17th; but it was so difficult to keep the wire free from magnetism and slight molecular strains, that I preferred and used only in the following experiments tempered steel wire (knitting-needles I found most useful). All the effects are greatly augmented by the use of iron wire, but its molecular elasticity is so great that we cannot preserve the same zero of reading for a few seconds together, whilst with steel, 0·5 millim. diameter, the effects remained a constant until we removed the cause.

I have not as yet been able to obtain a steel wire entirely free from magnetism, and as magnetism in steel has a remarkable power over the direction of the spiral currents, I will first consider those in which I found only traces. On passing the intermittent current through these, the sounds were excessively feeble for either polarity of current, but at each reversal a single loud click could be heard, showing the instant reversal of the molecular polarity. The degree of coil indicating the twist or spirality of the current was 5° on each side of its true zero. The wire was now carefully magnetised, giving 10° on each side for different currents. The positive entering at north pole indicating 10° right-handed spiral, negative entering the same pole, a left-handed spiral, we here see in another form, a fact well known and demonstrated by De la Rive by a different method, that an electric current travels in spirals around a longitudinal magnet, and that the direction of this spiral is entirely due to which pole of an electric current enters the north or south pole. I propose soon, however, to show that under certain conditions these effects are entirely reversed.

If through this magnetised wire I pass a constant current of two bichromate cells, and at the same time an intermittent one, the spiral is increased to 15°, but the direction of the intermittent current entirely depends on that of the constant current; thus, if the positive of the constant current enters the north pole, the intermittent positive slightly increases the spiral to 17°, and the negative to 13°, both being right-handed; the two zeros of the constant battery are, however, as we might expect from the preceding experiment, on equal opposite sides of the true zero; but if we magnetise the wire whilst a constant current is passing through it, a very great molecular disturbance takes place; loud sounds are heard in the telephone, and it requires for each current a movement of the coil of 40°, or a total for the two currents of 80°. This, however, is not the only change that has taken place, as we now find that both constant currents have a right-handed spiral; the positive, under which it was magnetised, a right-handed spiral of 95°; the negative, a right-handed spiral of 15°, and the true central or zero point of the true currents indicates a permanent spiral of 55°.

This wire was magnetised in the usual way, by drawing the north pole of my magnet from the centre to one extremity, the south from the centre to the other, and this repeated until its maximum effects were obtained; in this state I found, sliding the coil at different portions, that the spiral currents were equal, and in the same direction throughout.

It now occurred to me to try the effect of using a single pole of the magnet; this was done whilst a constant current was passing through the wire, commencing at the extremity, where the positive joined, drawing the north pole through the length of the wire, from positive towards the negative; the effect was most remarkable, as the steel wire now gave out as loud tones as a piece of iron, and the degree on the coil showed 200°. The constant and intermittent currents now showed for either polarity a remarkably strong right-handed twist; the positive 200 right, and the negative 150 right-handed spirals; the molecular strain on its wire from the reaction of the electric current upon the molecular magnetism was so great, that no perfect zero would be obtained at any point, a fact already observed when a wire was under an intense strain, producing

tertiary currents that superposed themselves upon the secondary. In order to compare these spiral currents with those obtained from a known helix, I found that taking a copper wire of similar diameter (0·5 millim.), and winding it closely upon the steel wire ten turns to each centimetre, having a total of 200 turns, with an exterior diameter of 1·5 millims., withdrawing the steel wire, leaving this closely wound helix free, that it gave some 190°, instead of the 200 of the steel wire alone; thus the spiral currents fully equalled a closely wound copper wire helix of 200 turns in a similar length.

If it were possible to twist a magnetised wire several turns to the right, and that its line of magnetism would coincide with that of the twist, then on passing a positive or negative current, there would be an apparent augmented or diminished spirality of the current, but both would have a right-handed twist. The result would be identical with the phenomenon described, although the cause is different.

The explanation of this phenomenon can be probably found in the fact that the constant spirality now observed is that of the electric current under which it was magnetism, for whilst magnetising it we had a powerful source of magnetism constantly reacting upon the electric current, and the constant spirality now observed is the result or remains of a violent molecular reaction at the instant of magnetisation, and the remaining evident path or spiral is that of the electric current. On testing this wire as to its longitudinal magnetic force, I found that it was less than a wire simply magnetised in the usual way; thus the effects are internal, affecting the passage of the electric current, giving, however, no external indications (except apparent weakness) of the enormous disturbance which has taken place.

3. *Molecular Sounds.*—The passage of an intermittent current through iron or other wire gives rise to sounds of a very peculiar and characteristic nature. Page in 1837 first noticed these sounds on the magnetisation of wires in a coil. De la Rive published a chapter in his "Treatise on Electricity" (1853) on this subject, and he proved that not only were sounds produced by the magnetisation of an iron wire in an inducing coil, but that sounds were equally obtained by the passage direct of the current through the wire. Gassiot, 1844, and Du Moncel, 1878–81, all have maintained the molecular character of these sounds. Reis made use of them in his, the first electric telephone invented, and these sounds have been, since the apparition of Bell's telephone, often brought forward as embodying a new form of telephone. These sounds, however, for a feeble source of electricity, are far too weak for any applied purposes, but they are most useful and interesting where we wish to observe the molecular action which takes place in a conducting wire. I have thus made use of these sounds as an independent method of research.

The apparatus was the same as in the last chapter, except no telephone was used. The intermittent electric current was connected by means of switch key, either with the coil inducing longitudinal magnetism in the wire, or could be thrown instantly through the wire itself, thus rapid observations could be made of any difference of tone or force by these two methods; a reversing key also allowed when desired a constant current of either polarity to pass through the wire under observation.

Iron of all metals that I have yet tried gave by far the loudest tones, though by means of the microphone I have been able to hear them in all metals; but iron requires no microphone to make its sounds audible, for I demonstrated at the reading of my paper, March 31st, that these sounds with two bichromate cells were clearly audible at a distance. A fine soft iron wire (No. 28) is best for loud sounds to be obtained by the direct passage of the current, but large wires (1 millim.) are required for equally loud tones from the inducing coil. By choosing any suitable wire between these sizes, we can obtain equal sounds from the longitudinal magnetism or direct current. The wire requires to be well annealed; in fact, as in all preceding experiments, the sounds are fully doubled by heating the wire to nearly red heat. There are many interesting questions that these molecular sounds can aid in resolving, but as I wish to confine the experiments to the subject of the two preceding chapters, I will relate only a few which I believe bear on the subject.

On sending an intermittent electric current through a fine soft iron wire we hear a peculiar musical ring, the cadence of which is due to that of the rheotome, but whose musical note or pitch is independent both of the diameter of the wire and the note which would be given by a mechanical vibration of the wire itself. I have not yet found what relation the note bears to the diameter of the wire; in fact, I believe it has none, as the greatest variation in different sizes and different conditions has never exceeded one octave, all these tones being in our ordinary treble clef, or near 870 single vibrations per second, whilst the mechanical vibrations due to its length, diameter, and strain vary many octaves.

I believe the pitch of the tone depends entirely upon molecular strain, and I found a remarkable difference between the molecular strain caused by longitudinal magnetism and the transversal or ring magnetism produced by the passage of a current, for if we pass the current through the coil, inducing magnetism in the wire, and then gradually increase the longitudinal mechanical strain by tightening the wire, the pitch of the note is raised some three or four tones (the note of the mechanical transversal vibrations being raised perhaps several octaves); but if we tighten the wire during the passage of an electric current through it, its pitch falls some two or three notes, and its highest notes are those obtained when the wire is quite loose. A similar but reverse action takes place as regards torsion; for if the wire is magnetised by the coil we obtain an almost complete zero of sound by simply moving the torsion index 45° on either side, and as this was the degree which gave silence in the previous experiments for the same wire, it was no doubt due to the same rotation of its polarised molecules. If we now pass a constant current through the wire whilst the intermittent one is upon the coil, we hear augmented sounds, not in pitch but loudness, and if we give torsion of 45° to one side we have silence, or nearly so, whilst the other side it gives increased tones which become silence by reversing the battery. If whilst the wire by torsion has been brought to zero, we decrease or increase the mechanical longitudinal strain, then at once the polarised molecules are rotated, giving loud sounds; and we further remark that when the wire is loosened, and we again tighten it, we gradually approach a zero, and on increasing the strain the sounds return; thus we can rotate the molecules by a compound strain of torsion and longitudinal strain.

If we wish to notice the influence of a constant current passing through the wire under the influence of the intermittent current in the coil, we find that if the wire is free from torsion that on passing the current the tones are diminished or increased according to the direction of the current; the tones then have an entirely distinctive character, for whilst preserving the same musical pitch as before, the tones are peculiar, metallic, and clear, similar to when a glass is struck, whilst the tones due to longitudinal magnetism are dull and wanting in metallic timbre. If we now turn the index of torsion upon one side, we have a zero of sound with or without the current; but the opposite direction gives increased tones whilst current is passing through the wire, but zero when not. Here again a peculiarity of timbre can be noticed, as although we have loud tones due only to the action of the current through the wire, the timbre is no longer metallic, but similar to that previously given out by the influence of the coil; evidently then the metallic ring could only be due to the angular polarisation of the molecules, and when these were rotated by torsion the tones were equally changed in its action upon the wire.

I have already shown that a permanent magnet brought near the wire could rotate its polarisation, and it equally can produce sound or silence in these molecular sounds (during that the wire is at its zero of torsion, and a constant current sent through the wire as in the last experiment). We find that either pole of the natural magnet has equal effect in slightly diminishing the sound by an equal but opposite rotation from the line of its maximum effects; but if the wire is brought nearly to zero by torsion, then on approaching one pole of the natural magnet we produce a complete silence, but the opposite pole at once rotates the molecule to its maximum loudness, and on taking away the magnet we have comparative silence as before.

Heating the wire to nearly red heat by a spirit lamp increases the tones of longitudinal magnetism induced by the coil some 25 per cent., but it has a much more marked increase on the tones produced by the direct passage of the current where they have more than 100 per cent. increase; and if we pass the intermittent current through the coil and constant through the wire, we find no direct rotation of the molecules by heat, although an apparent rotation takes place if we by the required torsion first place the wire at its zero. Then on the application of heat faint sounds are heard, which become again almost silent on cooling; this is simply due to the diminution by heat of the effect of the elastic torsion.

Tempered steel gave exceedingly faint tones, requiring the use of the microphone; but on magnetising with a constant current,

inducing spiral magnetism, the sounds became audible, some 15° sonometer against 175° for iron; thus the molecular rigidity of steel as observed by previous methods was fully verified.

I have, I believe, demonstrated by actual experiments which are easy to repeat, that—

1. An electric current polarises its conductor, and that its molecular magnetism can be reconverted into an electric current by simple torsion of its wire.

2. That it is by the rotation of its molecular polarity alone that an electric current is generated by torsion.

3. That the path of an electric current through an iron or steel wire is that of a spiral.

4. That the direction of this spiral depends on the polarity of the current, or that of its magnetism.

5. That a natural magnet can be produced, having its molecular arrangement of a spiral form, and consequently reversed electric currents would both have a similar spiral in passing through it.

6. That we can rotate the polarised molecules by torsion or a compound strain of longitudinal and transversal.

7. That the rotation or movements of the molecules give out clear audible sounds.

8. That these sounds can be increased or decreased to zero by means that alone have produced rotation.

9. That by three independent methods the same effects are produced, and that they are not due to a simple change or weakening of polarity, as when rotation has been incomplete a mere mechanical vibration has at once restored the maximum effect.

10 That heat, magnetism, constant electric currents, mechanical strains and vibrations have all some effect on the result.

Linnean Society, June 2.—Sir John Lubbock, Bart., F.R.S., in the chair.—Mr. R. Romanis of Rangoon was elected a Fellow of the Society.—Dr. G. Hoggan exhibited and made remarks on preparations of the lymphatics of vascular walls.—Mr. Elwes exhibited samples of quinine made by a new process by Mr. Gammie of Sikkim.—Mr. Thos. Christy drew attention to living rubber plants from West Africa, viz., *Urostigma Vogelii* and *Tabernæmontanum crassa*, and he showed products of *Pistacia terebinthus*, viz., the nuts, the resin, and the so-called butter, separated from the resin, and used for sweetmeats in the East, also Chian turpentine from the same tree.—Sir J. Lubbock afterwards read a paper on the habits of ants, for abstract of which see p. 142.—Mr. S. O. Ridley read a paper on the genus *Plocamia* of Schmidt and some other Echinonematous sponges, with reference to the genus mentioned, for which he accepted Prof. Duncan's name of *Dirrhopalum*. He enumerated three species already described, but assigned to other genera, which must be added to it; the distribution is thus extended from the tropical Atlantic to the British, Portuguese and Ceylon Seas. He described a New Zealand species which proves to be new to science and appears to decide a point which has been disputed, viz., the existence of ceratinous material in the skeleton. Geological facts were brought forward showing the existence of the genus in the Eocene, Upper Chalk, and Greensand formations. A new genus of the same order was described, based on a species of Lamarck and two other species; it is closely allied to *Dictyocylindrus*; its distribution extends from Arabia to Australia.—Prof. Duncan also read a communication on two new species of sponges of the genus *Dirrhopalum* from the Atlantic sea-bed.—The ninth part of the Rev. Boog Watson's mollusca of the *Challenger* Expedition, family Pleurotomidæ, was read by the Secretary.

June 16.—Sir John Lubbock, Bart., president, in the chair.—Mr. Alex. Somerville, Capt. J. T. Wright, and Mr. John Forrest, the Australian explorer, were elected Fellows of the Society.—Mr. W. Hood Fitch exhibited a set of folio drawings of new orchids, species of *Odontoglossum*.—The Rev. W. Higgins showed a Holothurian (*Psolus squamatus*) got between the Falklands and Patagonia, originally figured by Otho Fred. Müller in his "Zoologia Danica," and now recorded of wide distribution.—A letter was read from Mr. W. Ferguson of Colombo, mentioning his having found *Wolffia arrhiza*, Wimm., in abundance, and discovery of *Adiantum Æthiopicum*, L., in the Kandyan country, both plants being new to Ceylon.—Mr. J. G. Baker exhibited a specimen of the inflorescence of *Aloe Paryii*, which had flowered for the first time in this country, although the drug obtained from the plant had been known for 2000 years.—Surgeon-Major Aitchison then read a communication on the flora of the Kuram Valley, Afghanistan (Part II.); he showed by a map the peculiarities of the vegetation of the district, and in illustration of his paper referred to a series of dried specimens of the plants and the products in use by the natives, and otherwise characteristically interesting.—The next paper read was on Central African plants collected by Major Serpa Pinto, by Prof. Count Ficalho and W. P. Hiern. The specimens herein discussed were collected by Major Serpa Pinto in the month of August, 1878, along the upper course of the River Ninda, an affluent of the Zambesi, on the west side of the high plateau. As regards the climate of this locality the temperature is described as variable, the weather as very dry during seven or eight months of the year, and very wet during two or three months. The nature of the soil is metamorphic argillaceous schist; the latitude is 14° 46′ S., the longitude 20° 56′ E., and the elevation 1143 metres above the ocean. The rest of the botanical collections made by Major Serpa Pinto at different points of the journey, which were much more considerable, were lost. The present little collection consists of seventy-two numbers, comprising sixty-five species in thirty-nine genera; twelve of these species are new or not previously described and published, and at least one new genus appears amongst them. Some of the specimens are too imperfect for final determination, and several of the grasses and sedges can only be generally referred to their approximate position, and not specifically ascertained. In the case of the previously-known species, the affinities of many of the species are not only with the flora of Huilla in South Angola, but also in several instances with that of extra-tropical South Africa; only a few of the species are widely distributed in the tropics of this and other continents.—There followed a paper by Mr. Edward J. Miers, viz., revision of the Idoteidæ, a family of Sessile-eyed Crustacea; and another by Prof. Ewart on the nostrils of the Cormorant. Certain structural peculiarities in the latter were described, these apparently accounting for or being related to a certain extent with the habit of the bird of flying with its mouth open.

Physical Society, June 11.—Lord Rayleigh in the chair.—New Members: Mr. J. E. H. Gordon and Mr. J. E. Stead, E.I.C.—Prof. Fleming exhibited a new form of B.A. unit-resistance coil devised by him for experiments in the Cavendish Laboratory, with a view to obviate the leakage in the older form due to condensed moisture on the paraffin insulating the electrodes, and also to facilitate the equalisation of the temperature of the coil with the medium it is placed in. For this purpose the wire is wound bare, each layer being insulated from the rest by ebonite fenders nitched to receive the turns, and the coil is inclosed in a brass box screwed together. This box is water-tight, and may be soldered or provided with a leaden washer between the two flanged halves, which screw together. Dr. Stone said he usually insulated the B.A. coil, when plunging it in water, by putting it in a beaker of paraffin oil, which was immersed in turn in the water. Prof. Ayrton pointed out the advisability of makers aiming rather at turning out exact resistances of any definite value, rather than wasting time and increasing the cost of the coils by adjusting them to a given figure, such as 1 or 10 ohms.—Prof. W. Chandler Roberts read a paper on the hardening and tempering of steel. He pointed out that few questions connected with the metallurgy of iron or steel are attracting more attention now than the relation between a metal and the gases it comes into contact with during manufacture. The carburisation of iron has long been of great interest, as shown by the work of Clouet at the end of last century, and that of Margueritte in 1865, who showed that though the conversion of iron into steel could be effected by contact with carbon even in the diamond form, it is nevertheless true that in the ordinary process carbonic acid plays a considerable part which had been overlooked. Graham's 1867 paper on the occlusion of gases by metals gave point to Margueritte's work by showing that carbonic oxide can penetrate to the centre of a mass of iron. It is introduced, in fact, at a comparatively low temperature, while a high temperature is necessary to enable the metal to appropriate the carbon and become steel. Metallurgists are now carefully investigating the effect of occluded gases in iron and steel. Prof. Roberts considered the point recently raised as to whether the hardening and tempering of steel might not be influenced by the occlusion or expulsion of gas. He described experiments by which he proves that, as steel hardens when rapidly cooled *in vacuo*, gases could not play any part in the tempering. He also dwelt on the precautions necessary to keep the metal in the experiment free from occluded gas. He then showed that

Bergman (to whom we owe our knowledge that the difference between wrought iron and steel depends on the carbon in the latter) showed in 1781 that fixed air could give up its carbon to iron; and he concluded by showing that Réaumur, so long ago as 1722, actually employed the Torricellian vacuum in experiments on the tempering of metal, the metal being placed red-hot in a highly rarefied atmosphere. Réaumur also had a clear view of the effect a gas might have on the physical properties of a metal—a point of great interest to physicists in general. Prof. Hughes expressed the opinion that temper was not due to absorption of hydrogen, but to the combination of carbon with the iron. Mr. Stroh had found that an electrically-fused steel contact was glass hard. Prof. Guthrie exhibited a steel chain which he had beautifully blackened by dipping in fused nitre. The skin might be useful in the arts, and was perhaps analogous to that produced by Barff's process. Lord Rayleigh, M. Walenn, Mr. Lecky, Dr. Coffin, Prof. Ayrton, and others continued the discussion.—Mr. Grant then read a paper on curves of electro-magnetic induction, which he had traced out by means of primary and secondary coils, sliding on frames so as to take different positions with respect to one another. The paper was illustrated by experiments and diagrams.—Prof. Reinhold then read portions of a paper by Prof. S. P. Thompson on the opacity of tourmaline crystals. The optical and electric properties of these crystals are related; and Prof. Thomson propounds an explanation of this connection based on the late Clerk-Maxwell's electro-magnetic theory of light. The full paper will be published in the *Journal* of the Society.

Meteorological Society, June 15.—Mr. G. J. Symons, F.R.S., president, in the chair.—Eleven gentlemen were elected Fellows of the Society, viz. F. Crowley, A. M. Davis, Rev. R. Drake, F. H. D. Eyre, W. M. Gibson, E. W. Mathew, J.P., D.L., J. Parnell, M.A., F.R.A.S., J. Rigby, T. G. Rylands, F.L.S., F.G.S., H. Smith, and A. H. Wood.—The following papers were read:—The use of synchronous meteorological charts for determining mean values over the ocean, by Charles Harding, F.M.S.—The climate of Fiji, by R. L. Holmes, F.M.S. This paper gives the results of meteorological observations taken at Delanasau, Bua, Vanua Levu, during the ten years 1871–80.—Note on the formation of hail, by J. A. B. Oliver.—Note on a comparison of maximum and minimum temperature and rainfall observed on Table Mountain and at the Royal Observatory, Cape Town, during January and February, 1881, by John G. Gamble, M.A., M.Inst.C.E., F.M.S.—Mr. E. J. Spitta exhibited and described a new mercurial maximum and minimum registering thermometer.

PARIS

Academy of Sciences, June 13.—M. Wurtz in the chair.—The first volume of Annals of the Rio Janeiro Observatory was presented.—The following papers were read:—On a simple law of natural circular or magnetic double refraction, by M. Cornu. The decomposition of a wave polarised rectilinearly into two waves polarised circularly in opposite directions is such that the mean of the velocities of propagation of the resultant waves is equal to the velocity of the single wave which exists where the causes of decomposition do not act.—On dialdanic alcohol, by M. Wurtz.—On reproduction, by aqueous method, of orthose felspar, by MM. Friedel and Sarasin. The felspar was formed at a high temperature in the heart of a mother-water rich in alkaline silicate. The crystals were very small.—Summary account of experiments made at Pouilly-le-Fort, near Melun, on *charbon* vaccination, by M. Pasteur, with MM. Chamberland and Roux. We now possess vaccine matter of *charbon*, capable of preserving from the fatal disease, but not itself fatal, capable of cultivation at will, transportable anywhere without alteration, and prepared by a method which may probably be generalised. M. Milne-Edwards noted the analogy of some of M. Pasteur's facts to the phenomenon of alternating generations, asking whether, by changing the biological conditions, one or other term of such alternation might not be had at will.—Observations on M. Pasteur's paper, by M. Bouley. He calls attention to the successful vaccination, by MM. Arloing, Cornevin, and Thomas, against *symptomatic charbon* (which is distinct from *bacteridian charbon*). They do not use attenuated virus (like M. Pasteur), but natural virus, attenuating the effects by bringing it directly into the blood.—Reply to observations by M. de Lesseps at the last *séance*, by M. Cosson. This relates to the Tunisian Chotts.—On a system of differential equations, by M. Briordier.—On the means of saving water in double locks and acceleration of the service, by M. de Caligny.—M. Milne-Edwards presented a *brochure* on some macruran Crustaceans from great depths in the Caribbean Sea, calling attention, *inter alia*, to a large Crustacean, *Phoberus cæcus*, quite blind, which is a transitional form between groups hitherto thought very different.—M. Fouqué was elected Member in Mineralogy in room of the late M. Delesse.—On linear differential equations with periodical coefficients, by M. Floquet.—On the treatment of vines with sulphide of carbon, by M. Boiteau.—On the functions of two variables arising from the inversion of integrals of two given functions, by M. Fuchs.—On certain systems of differential equations, by M. Halphen.—On the influence of temperature on radiophonic selenium receivers, by M. Mercadier. These tend more or less quickly to a stable state relatively to effects of temperature. At ordinary temperatures and even up to 100° the resistance varies inversely as the temperature. Between 5° or 6° and 35° these variations may be approximately considered proportional one to the other.—On some means and formulæ of measurement of electric elements and coefficients of utilisation with the arrangement having two galvanometers, by M. Cabanellos.—Hemeralopia and retinian torpor, two opposite forms of Daltonism, by MM. Macé and Nicati. Hemeralopia is in general Daltonism for blue. Persons having retinian torpor are as if in a poor light; other rays than the blue are badly perceived, especially the red.—Water-raising machines, by M. de Romilly. He describes a machine, consisting, in its general form, of a shallow cylinder with vertical axis, wide circular opening above, and near the circumference the orifice of a pipe which curves upwards. The cylinder (turbine) is rotated, and the water accumulates by centrifugal force at the circumference, where it leaves tangentially through the pipe. Water can thus be raised much higher than hitherto by centrifugal force (*e.g.* 150 m. with a turbine driven by hand). Two modifications are described. The same principle is applied in oiling the machine.—Cyanides of strontium, calcium, and zinc, by M. Joannis.—Industrial preparation of crystallisable formic acid, by M. Lorin.—Researches on tertiary monamines: I. action of triethylamine on monobromised propylenes, by M. Reboul.—Nervous system of Ophiurans, by M. Apostolidès.—On the squamous temporal bone in the vertebrate series, by M. Lavocat.—On *Phytolacca dioica*, by M. Balland.—On the Carboniferous fauna of Rémy (Loire), and its relations with that of Ardoisière (Allier), by M. Julien.—On the dissolution of false membranes of *angina pellicularis* by local applications of papaine, by M. Bouchut.—On an apparatus for suppressing the dangers of movable stoves, by M. Godefroy. The air for combustion is drawn from the chimney itself by a second tube; the chimney and stove may thus be hermetically closed.

CONTENTS

THURSDAY, JUNE 30, 1881

ILLUSIONS

Illusions. By James Sully. International Science Series, Vol. xxxiv. (London: C. Kegan Paul and Co., 1881.)

OF the many interesting subjects to which the publishers of the International Science Series have hitherto devoted their volumes, few have presented so formidable a test of the strength of their respective authors as the one which has been assigned to Mr. James Sully. Occupying some of the most obscure regions of physiology, and passing over into the remotest cloudlands of psychology, illusions furnish material alike for the most laborious exploration and the most keen-sighted analysis; but for this very reason they constitute a class of phenomena which invite failure of treatment at the hands of any but the most accomplished psychologist. Mr. Sully is already well known to stand in the first rank as a writer of this class, and the able manner in which he has handled the difficult subject consigned to him shows that it could not have been consigned to a better man. The wide range of his reading, the clearness and force of his style, as well as the soundness of his judgment, give him what we may call exceptional advantages for undertaking a treatise of this kind, while the methodical, not to say laborious, manner in which he has executed the task, shows that he has thrown all his strength into its performance.

First we have presented a "Definition of Illusion," which, standing in a scientific as distinguished from a philosophical treatise, is justly framed so as to exclude any question with the idealist or the sceptic. "For our present purpose the real is that which is true for all. . . . Human experience is consistent; men's perceptions and beliefs fall into a consensus. From this point of view illusion is seen to arise through some exceptional feature in the situation or condition of the individual, which, for the time, breaks the chain of intellectual solidarity which under ordinary circumstances binds the single member to the collective body. Whether the common experience which men thus obtain is rightly interpreted is a question which does not concern us here. For our present purpose, which is the determination and explanation of illusion as popularly understood, it is sufficient that there is a general consensus of belief, and this may be provisionally regarded as at least practically true."

Next we have a very methodical and judicious "Classification of Illusions." As distinguished from hallucinations, illusions "must always have a starting-point in some actual impression, whereas a hallucination has no such basis." Still the one shades off so gradually into the other that no determinate line can be drawn between them. Therefore this distinction, although recognised as a distinction, is not constituted, as it has been constituted by some technical writers, "the leading principle of classification"; but a more popular or common-sense principle is adopted. All immediate knowledge, or knowledge not attained by any conscious process of inference, may be divided into four principal varieties—Internal Perception (Introspection), External Perception, Memory, and Belief, including Unreasoned Expectation. The difficult question as to the relation of Belief to Knowledge is expressly set aside—it being allowed by every one that many of our beliefs are for all purposes of action as good as knowledge. Each of these four sources of immediate or uninferred knowledge is open to the contamination of illusion. Such is notoriously the case with sense-perception, which, as the best-marked variety, is treated first.

In the course of a clear analysis of Perception stress is laid upon what the author calls a "stage of preperception," during which the mind receives the impression of sense, but has not yet interpreted the impression into a coherent percept. "In many of our instantaneous perceptions these two stages are indistinguishable to consciousness . . . But in the classification of an object or the identification of an individual thing there is often an appreciable interval between the first impression and the final stage of complete recognition." [The time, that is, during which, in Mr. Spencer's language, the mind is forming its "integrations"—a process which takes place more rapidly in adults than in children, and in "quick-witted" than in less "ready" individuals.] "And here it is easy to distinguish the two stages of preperception and perception. The interpretive image is slowly built up by the operation of suggestion, at the close of which the impression is suddenly illumined as by a flash of light, and takes a definite precise shape." Now illusions of perception may arise in either of these two stages. Even *in limine* sufficient attention may not be paid to the original impression, and thus a timid man will readily fall into the illusion of ghost-seeing, because too little attentive to the actual impression of the moment. But next, even if the sensation is properly attended to, "misapprehension may arise of what is actually in the mind at the moment." Although this "may sound paradoxical," it means nothing more than that "the incoming nervous process may to some extent be counteracted by a powerful reaction of the centres." Thus, for instance, a sensation of colour may be appreciably modified when there is a tendency to regard it in one particular way.

After giving parenthetically a number of illustrations of errors of perception which have their root in the initial processes of sensation and "preperception," the essay passes on to a further consideration of the more important class of illusions which are connected with the later stages of perception, or the process of interpreting the sense-impression. These misinterpretations of sense-impressions fall into two classes, according as they are connected with a process of *suggestion* or with the process of *preperception*. The illusion of a second shouter in an echo is given as an example of the former, while that of seeing spectres in familiar objects after the imagination has been excited by ghost-stories, supplies an illustration of the latter. The first of these classes of illusions arises from without, the second from within: the one is therefore called Passive, the other Active. Besides these there are other "sub-divisions" which need not here be detailed. Indeed we think that a desirable simplicity of classification might have been attained by ignoring these lesser ramifications, and restricting attention to the main divisions—*i.e.* illusions arising in the initial processes of sensation, in those of preperception, and in those of

perception, passing on to illusions of introspection, memory, and belief.

Taking first the illusions of sensation, preperception, and perception, the following is a brief sketch of Mr. Sully's analysis of their nature and causes.

1. *The Limitation of Sensibility;* the amount of sensation is not always a fair measure of the amount of stimulation, which may be either inadequate or over-adequate in relation to the excitability of the sense-organs. 2. *The Variation of Sensibility;* changes of organic state—whether temporary, as those arising from fluctuating nutrition or fatigue, &c., or permanent, as those arising from age or disease—by supplying us with a variable index of objective phenomena, lead to illusory misrepresentations of these phenomena, unless such variations are duly recognised and allowed for. 4. *Exceptional Relation of Stimulus to Organ;* a man crunching a biscuit can scarcely believe that others do not hear the sound more loudly than he does, or, on rubbing his nose with the points of his third and fourth fingers crossed, that this organ has not become split into two; in such cases the sense perceptions are interpreted by the help of more familiar relations, and so illusion arises. 5. *Exceptional External Arrangements;* unless the fact that we are ourselves moving is clearly presented to consciousness, we instinctively conclude that surrounding objects are moving in the opposite direction, and under similar circumstances are apt to suppose that a train which is just shooting ahead of our own train is moving but slowly; on this principle depends the illusion of the stereoscope, misjudging distances in the clear atmosphere of Switzerland, &c. 6. *Devices of Art;* perspective, effects of light and shade, &c., are all so many devices to ensnare visual perception into a misinterpretation of marks on a flat surface for objects situated in space of three dimensions. 7. *Misconception of Local Arrangement;* the examples given under this head appear to involve exactly the same principle as 5, and nothing more. 8. *Misinterpretation of Form;* the same remark applies to this head—7 and 8 are really species of the genus 5, and ought to have been considered under it. 9. *Illusions of Recognition;* as in general we attend only to what is essential and constant in objects, to the disregard of what is variable or accidental, opportunity is thus furnished for a large class of illusions; imagination and expectant attention likewise play an important part in producing illusions of this kind. 10. *Voluntary Selection of Interpretation.* So far the enumeration has been concerned with what the author calls "Passive Illusions," *i.e.* illusions in which the imagination is inactive, or comparatively so; now we pass to the "Active Illusions," and as an example of voluntary selection of interpretation we may notice that in looking at a geometrical drawing of a truncated pyramid the figure may by a voluntary act be seen to represent alternately a solid upstanding form, or a hollow receding box. 11. *Involuntary Mental Preadjustment;* this resembles the last case, save that the illusion is not due to an act of volition. "The whole past mental life . . . serves to give a particular colour to new impressions. . . . There is a personal equation in perception as in belief." 12. *Sub-expectation;* this has already been alluded to, and is obviously a potent cause of illusion. 13. *Vivid Expectation;* still more obviously so. Indeed vivid expectation may "produce something like a counterfeit of a real sensation." An anxious mother may fancy that she actually hears her child cry in an adjoining room, &c. 14. *Transition to Hallucination;* clearly but a step farther, and illusion passes into hallucination, where imagination has become altogether detached from present surroundings, and has entered on the stage of highest activity. But hallucinations are not invariably of central origin; they may also be of peripheral, and do not always betoken pathological conditions, though they usually reach their highest perfection in the insane Thus there is an apparently unbroken continuity from the scarcely noticeable illusions of normal life leading up to the most startling hallucinations of abnormal. This consideration leads to the following pretty piece of speculation:—

"We may, perhaps, express this point of connection between the illusions of normal life and insanity by help of a physiological hypothesis. If the nervous system has been slowly built up, during the course of human history, into its present complex form, it follows that those nervous structures and connections which have to do with the higher intellectual processes, or which represent the larger and more general relations of our experience, have been most recently evolved. Consequently, they would be the least deeply organised, and so the least stable; that is to say, the most liable to be thrown *hors de combat.* This is what happens temporarily in the case of the sane, when the mind is held fast by an illusion. And, in states of insanity, we see the process of nervous dissolution beginning with these same structures, and so taking the reverse order of the process of evolution.[1] And thus, we may say that throughout the mental life of the most sane of us these higher and more delicately balanced structures are constantly in danger of being reduced to that state of inefficiency which in its full manifestation is mental disease."

Next there follows an interesting chapter on Dreams, in which the mechanism of thought in sleep is ably and suggestively laid bare, so far as the complex and difficult nature of the subject permits. Want of space however prevents our entering upon this chapter, and therefore we shall pass on at once to the Illusions of Introspection. This and the next division of the work is perhaps the part that displays most originality. At first sight it seems almost impossible that the mind could be subject to illusion in its consciousness of its present state or contents; but yet it is clearly shown that such is very frequently the case. "No such clearly-defined mosaic of feelings presents itself in the internal region" as that which is presented in the external when interpreted by sensuous perception; "our consciousness is a closely-woven texture in which the mental eye often fails to detect the several threads or strands." Moreover, "many of these ingredients are exceedingly shadowy, belonging to that obscure region of sub-consciousness which it is so hard to penetrate with the light of discriminative attention." Thus numberless illusions of introspection become possible. All cases of "self-deception" fall into this category, whether they arise from a wrong intellectual focussing of the attention, so as to give undue prominence to some feelings over others, or from a mere emotional bias. As examples we may take the self-deception of a man who is really "bored" by a social entertainment, yet making himself

[1] Reference is there made to Dr. J. Hughlings-Jackson's papers in *Brain.*

believe that he is enjoying it; or that of a conceited man who thinks more highly of himself than a just introspection, untainted by emotion, would show that he deserves. It is pointed out that illusions of introspection have been more generally recognised by theologians than by philosophers; for while the former preach that the heart mistakes the fictitious for the real, and the evanescent for the abiding, the latter frequently regard a "deliverance of consciousness" as bearing the seal of supreme authority. This consideration leads to an interesting section on "Philosophic Illusions," wherein the question is discussed as to how far the introspective method is a trustworthy one for Philosophy to follow. The result of the discussion is that, as the internal experience of individuals, no less than their common environment, has a common nature, individual introspection should always be guided as much as possible in matters of internal experience by the general consensus; and that "the progress of psychology and the correction of illusion proceed by means of an ever-improving exercise of the introspective faculty."

Coming next to the Illusions of Memory, or Representative as distinguished from Presentative Illusions, it is shown that these are distinct from mere forgetfulness or imperfection of memory. To forget a past event is one thing; to seem to ourselves to remember it when we afterwards find that the event was other than we represented it, is another thing. Illusions of memory are classified under three heads:—(1) *Falsification of Dates;* (2) *Misrepresentation of Events; and* (3) *Creation of Events by the Imagination which never happened in Reality.* Each of these classes of illusory representations has its counterpart in the illusions of Presentation. Thus, Class 1 has its visual counterpart in erroneous perceptions of distance; Class 2 in those optical illusions which depend on the effect of haziness or the action of refracting media; and Class 3 in subjective sensations of light or other hallucinations. In the detailed discussion of Class 1 there is a long and careful analysis of time-consciousness, in which numerous causes of erroneous estimate of duration are clearly stated, after which follows a statement of the conditions leading to *Indefinite Localisation;* these sections are exceedingly good. Under Class 2, or Distortions of Memory, it is shown that although we may in some cases account for the confusion of fact with imagination, "in other cases it is difficult to see any close relation between the fact remembered and the foreign element imported into it. An idea of memory seems sometimes to lose its proper moorings, so to speak; to drift about helplessly among other ideas, and finally, by some chance, to hook itself on to one of these, as though it naturally belonged to it." The analogy between this class of mnemonic illusions and that of illusions of perception is obvious. "When the imagination supplies the interpretation at the very time, and the mind reads this in to the perceived object, the error is one of perception. When the addition is made afterwards, on reflecting upon the perception, the error is one of memory." To the several sources of such mnemonic illusions mentioned by Mr. Sully, I may add another, which I have recently had occasion to observe. This consists in what may be called a transposition of associations. In a club I saw a man walk through the smoking room. He was an eminent psychologist, and although I knew him very well I mistook him for another man equally eminent in the same line, and whom I knew equally well. Clearly the similarity of their pursuits caused a most extraordinary transposition of two sets of associations, for the two men bore no personal resemblance to one another. As soon as the man had left the room, I remembered that I had something to ask the man for whom I had mistaken him. I therefore sent a page to find this other member of the club, but without success. I then went to the hall-porter, who said he was sure that this member had not come in. Yet so strong was my conviction of having seen him that I began to think I must have seen an optical illusion, and therefore resolved to write him a letter to ascertain still more certainly that he had not been in the club at that particular hour. And it was not until I had seriously meditated on the matter for ten or twelve minutes that I suddenly perceived the illusion to have been one of memory and not of sense. This I think is a remarkable case, because both the men in question are so well known to me that I have never ventured to tell either of them of my illusion, lest, psychologists though they be, they should suppose that I had been somewhat excessive in patronising the good things which the club had to afford.

It is shown that Hallucinations of Memory may arise either from believing events in dreams to have occurred in fact, or from waking imagination being strong enough to read spurious facts into the past. The former source is clearly common to us all, and the latter is so far from being distinctive of pathological condition that in one respect, at least, it is even more universally present than the other. For "the total forgetfulness of any period or stage of our past experience necessarily tends to a vague kind of hallucination. In looking back on the past we see no absolute gaps in the continuity of our conscious life." Yet it is obvious that we must fill up immense lacunæ without conscious knowledge, and in so far as this is the case, memory is subject to hallucination. From this position there follows a section on "*Illusions with respect to Personal Identity,*" the substance of which may be gathered from the following quotation:—"To imagine that we have ourselves seen what we have only heard from another or read, is clearly to confuse the boundaries of our identity. And with respect to longer sections of our history, it is plain that when we wrongly assimilate our remote with our present self, and clothe our childish nature with the feelings and the ideas of our adult life, we identify ourselves over much. In this way, through the corruption of our memory, a kind of sham memory gets mixed up with the real self, so that we cannot, strictly speaking, be sure that when we project a mnemonic image into the remote past we are not really running away from our true personality."

Lastly, we come to "*Illusions of Belief,*" the latter word being taken in its widest sense as embracing all representative knowledge other than memory—including therefore anticipation of the future, acquaintance with the experience of others, and "our general knowledge about things." In so wide a field there is boundless scope for illusions of many kinds. These are classified and considered in the latter part of the work, but we have no space left to follow our diligent author into this division of his subject.

The work concludes with a chapter of "*Results*," which shows that illusion in general consists in a "bad grouping of psychical elements," and as such shade off into fallacies of reasoning; in both there is a want of correspondence between internal and external relations. In the future and for the race natural selection and "direct equilibration" can only be expected to remedy the sources of any such mal-adjustment in so far as it may be of actual injury to life. Thus we can have no absolute criterion of illusion. "Science cannot prove, but must assume the coincidence between permanent common intuitions and objective reality. To raise the question whether this coincidence is perfect or imperfect, whether all common intuitions known to be persistent are true, or whether there are any that are illusory, is to pass beyond the scientific point of view to another, namely, the philosophic." This consideration leads to an exceedingly able statement of the relations between scientific and philosophic thought, but the discussion necessarily runs into an abstruseness that it is not desirable here to enter. In general, however, it may be said that in this, as in some of his other works, Mr. Sully shows that while he has perceived more distinctly than most of our leading psychologists the sharpness of the boundary between science and philosophy, he displays an admirable clearness of thought in never allowing the methods of the one sphere to encroach upon those of the other, while in whichever sphere he chooses to work he enjoys the privilege, almost unique among psychologists, of finding himself equally at home.

GEORGE J. ROMANES

OUR BOOK SHELF

Studies in Biology for New Zealand Students. No. 1.—The Shepherd's Purse (*Capsella Bursa-Pastoris*). By F. W. Hutton, Professor of Biology, Canterbury College, University of New Zealand. (New Zealand: By Authority. 1881.)

THIS is a detailed study of the coarse and minute anatomy of a very familiar and widely-diffused weed. A native originally of the palæarctic region, it has now found its way to all temperate climates. It has certainly received at Prof. Hutton's hands, in the Antipodes, a more systematic investigation than ever fell to its lot in Europe. The treatment is much the same as that given to the bean (*Faba vulgaris*) in Huxley and Martin's "Elementary Biology," though with a more botanical bias. The weakest part is the treatment of the root, where nothing is said about the mode of origin of branches. An earlier stage should have been taken, showing the arrangement of the fibro-vascular tissues before they had coalesced into a central cylinder. This however is simply by way of criticism. The method of treatment is excellent, and the Canterbury students are fortunate in being in the hands of a teacher who has such a thorough appreciation of the biological method as applied to botany.

A Text-Book of Indian Botany, Morphological, Physiological, and Systematic. By W. H. Gregg, Lecturer on Botany at the Hugli College. (Calcutta: Thacker, Spink, and Co., 1881.)

THIS is the first eighty pages of a book which, when completed in 500–600, will apparently be practically Henfrey's Elementary Course, adapted to the local requirements of Indian students. There does not seem anything particularly novel or noteworthy in the treatment of the subject, as far as can be judged from the portion printed. As is usual in books of this type, some space is devoted to the Linnean classification. But, as the author points out, there is the excuse that Roxburgh's *Flora Indica*, which is still unsuperseded, is arranged in accordance with it.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Re W. I. Bishop

I CANNOT but feel greatly surprised that Mr. Romanes, when reporting the result of the investigations made by his colleagues and himself upon the power of "thought-reading" claimed by Mr. W. I. Bishop, should have stated that the letter of introduction which I gave to Mr. Bishop was "doubtless intended to recommend him to the attention of the credulous," since this letter most distinctly expressed my desire to obtain for him "an assemblage of gentlemen specially qualified to appreciate the importance" of what I described in it as (in my judgment) "experiments of great value to the Physiologist and Psychologist." Nor can I see how my having thus recommended "him to the attention of the scientific" is a thing "to be regretted"; since the careful testing of the one set of experiments which Mr. Bishop has shown to Mr. Romanes and his colleagues has resulted in a precise confirmation of my statement that the power of "thought-reading" which he claims is "derived from his careful study of the indications *unconsciously* given by the subjects of his experiments, and from his peculiar aptness in the interpretation of those indications."

What I think "is to be regretted" is that Mr. Bishop did not offer for like careful testing another remarkable set of experiments which he had repeatedly performed in the presence of distinguished medical and scientific men in the United States (from whom he brought introductions to me), and also before a like assemblage in Edinburgh; showing his power of naming words and numbers previously written and sealed up in private, by his acute recognition of indications *unconsciously* given by their writers when the alphabet or digits were "called."

As I have never credited Mr. Bishop with any other power of "thought-reading" than this, I have been surprised to learn that I am accused of "fathering a new humbug."

Another "experiment" which he performed in my own house some time ago struck me as well worthy of careful testing:—

The "subject" of the experiment being asked to draw a card from the pack, to identify it, and then to return it to Mr. Bishop, the latter, after shuffling the pack, dealt out sixteen cards *with their faces downwards*, arranging them in four rows (which I indicate by letters and numbers), as thus:—

	A	B	C	D
E	1	2	3	4
F	5	6	7	8
G	9	10	11	12
H	13	14	15	16

The "subject," having been caused to stand at the table with the cards directly before him, Mr. Bishop, standing at his right side, and taking his right hand into his own left, said to him, "Drop your left hand down on either row (whether *horizontal*, as A, B, C, D, or *vertical*, as E, F, G, H), that you wish taken away." Row B having been selected and taken away, there remained the three rows, A, C, D. "Now," said Mr. Bishop, "drop down on another row." Row D being selected, there remained rows A and C. "Now drop down on a third row." Row A being selected, there remained only row C. "Now drop down upon either the two upper or the two lower cards of the remaining row." The two upper (3 and 7) being selected and taken away, there remained only the two lower (11 and 15). "Now drop down

upon either of the two remaining cards." The lower (15) being selected, and the remaining card (11) being turned up, this proved to be the card originally drawn.

Having seen this experiment twice made successfully with members of my family, I offered myself as the next "subject" of it, with the determination to watch carefully for any manual guidance by which Mr. Bishop might be influencing my choice. The experiment succeeded with me as it had done with my predecessors, and yet I could not, any more than themselves, tell how I was led to make the five successive selections of the cards to be taken away, so as to leave behind the card I had originally drawn.

It may, of course, be assumed that Mr. Bishop knew where he had placed this card, although his "subject" did not; and he informed me that experience has taught him the positions to which the choice of his "subject" can be most easily and certainly guided. The influence of the eyes being excluded by the relative positions of Mr. Bishop and his "subject," the guidance *must* be conveyed through the hand which Mr. Bishop holds in his own; and yet I altogether failed to detect the mode in which it was given.

Of course it may be said that this is only a variation of the conjuror's trick of "forcing" the card which he has determined that the drawer shall choose. (I remember seeing it stated that Louis Napoleon, when Emperor, had defied Houdin then to "force" a card upon him; and that Houdin made him draw the card which in the French pack is designated César.) But though the same principle of "suggestion" is involved, the conditions under which it acts are altogether different. The conjuror stands *opposite* the drawer, looks at his face as well as at his hand, and continually shifts the position of the cards he holds, so as to prevent a *wrong* card from being drawn, while presenting the *right* one in the manner which he knows by experience to be the most suggestive. But Mr. Bishop does no such thing. The cards remain in their places with their faces downwards; and of the guidance given him by Mr. Bishop standing at his side, the "subject"—even when on the watch for it—remains quite unaware.

If I have made myself understood by your readers, I think I shall have satisfied them that this "experiment" (which may by no means invariably succeed) is of great psychological interest, as showing the large measure in which we may be guided in our choice among things "indifferent," by *influences of which we are ourselves unconscious*. WILLIAM B. CARPENTER

American Meteorological Observations

In your valued journal (vol. xxiv. p. 16) I find an expression of your regret that it should have been decided that the printing of the *Bulletin* of Simultaneous Meteorological Observations should hereafter take place one year after date, instead of six months. It may be interesting to those of your readers who made use of the *Bulletin* in studying the general atmospheric phenomena of the northern hemisphere, to know that for several years past the data for several distant land stations in Greenland, Iceland, Siberia, Alaska, &c., have been omitted, merely because the mail facilities did not enable us to receive the reports in time for publication in the *Bulletin*. Thus a large portion of the region covered by our maps has been left unrepresented, for which the necessary data come regularly to hand a few weeks or months later. The case is still worse in reference to the marine reports for vessels off on long voyages; for instance, we regularly pay for and receive a large collection of material from the London Meteorological Office that never appears in our published *Bulletin* or charts. The proposed postponement of publication is in fact merely the outcome of several suggestions and recommendations from co-operating nations, in the propriety of which recommendations myself and assistants fully concur.

W. B. HAZEN,
Chief Signal Officer, U.S.A.

Office of the Chief Signal Officer, Washington, D.C., June 15

A Meteor

LAST night, June 24, at 11h. 29m. G.M.T., I observed a meteor, as bright as Jupiter, cross the tail of the great comet 4° or 5° above the head and disappear some 20° to the left, on the vertical of Beta Ursæ Minoris and at an altitude equal to that of the comet's head. It left a bright streak for some seconds. I did not see the beginning, and perhaps not the end, as it may have continued behind a cloud bank. The duration for the above path was three seconds, determined afterwards by experiment. Place of observation, lat. 51° 32′, long. 0° 11′ W.

G. L. TUPMAN

27, Hamilton Terrace, St. John's Wood, June 25

LOOKING at the comet last night from my garden at 11.25 p.m. I saw a large meteor pass nearly horizontally from a little east of north to within a short distance of the comet, rather above the head. It was as large as Venus when brilliant, but with a red or orange tinge. The motion was rather slow. A.

Adsett Court, Westbury-on-Severn, June 25

Earthquake in Van

IT may perhaps be considered worthy of a note in your columns that an earthquake was experienced in this neighbourhood on Monday, May 30, at a few minutes before 6 a.m.

Here in Van the shock was slight, consisting only of a tremulous motion lasting a few seconds; but I have to-day received information that at Bitlis the shock was so severe as to cause people to rush out of their houses in fright, and that a village named Téout, situated near the western shore of the Lake of Van, was destroyed by it, with the loss of a considerable number of lives. I have as yet heard no details, but if any further circumstances of interest should come to light I will communicate with you again.

It is well known that the environs of the Lake of Van show many signs of ancient volcanic action; at least three volcanoes with distinct craters forming prominent features on or near its shores. Of these the Nimroud Dagh, on the western shore of the lake, is said by tradition to have been active not more than 400 years ago. It contains an immense crater five or six miles across, in which are situated hot springs. The village Téout which has been destroyed lies at the foot of the eastern slopes of this mountain. EMILIUS CLAYTON

Van, Turkey in Asia, July 6

Freshwater Actineæ

I YESTERDAY noticed in a small *freshwater* aquarium four specimens of a small freshwater Actinia of a very pale olivaceous colour. They have each six tentacles more than 1 inch in length when fully extended, but then so extremely fine at the ends as to be almost invisible. The body or stalk is about 0·1 inch long by 0·05 inch in diameter when at rest, and about 0·5 inch long and 0·015 inch in diameter when expanded.

I was not aware before of the existence of freshwater Actinia, but as the specimens to which I now refer are in all respects similar to sea-anemones, there can be no doubt on the subject.

I have succeeded in transferring two specimens, which have duly rooted and expanded themselves in a bottle and a tumbler, and I shall be happy, if they are of sufficient interest, to send one to your office or elsewhere for inspection. W. SEDGWICK

Royal Naval School, New Cross, June 24

The Observation of Hailstorms

IN the most casual survey of the literature relating to the phenomenon of hail one cannot fail to be struck with the remarkable contradictions which everywhere make themselves apparent. Some writers say that hail falls oftenest in the tropics; others assert that it is altogether unknown there! Howard states that the maximum hailfall in this country occurs in the summer season, while Dalton and others say that it is in winter. I think these singular discrepancies are attributable, in many cases at least, to imperfect observations. Most of the meteorologists who have given special attention to the phenomenon of hail have had their pet theories, and naturally their observations have been guided to a considerable extent by the requirements of the particular theories which they advocated. Thus we find Kamtz ascribing the formation of hail to the conflict of opposing currents of wind, Volta to the electrical condition of two separate layers of cloud, Leslie to the presence of strata of air at different temperatures, von Buch to ascending currents of air, and so on, all which theories are based, not upon invariable phenomena, but upon isolated features which happen to have come repeatedly under the notice of those particular observers in the particular hailstorms which they witnessed. I think it would enhance the

value of observations in some degree if all those who have opportunities of making them would draw up their notes on some regular and uniform plan. I do not presume to submit a scheme, but would suggest the following points as being of some importance:—

1. Date, and hour of the day.
2. Area of storm. If it assume the tornado form, give (*a*) length of the course, (*b*) breadth, (*c*) direction of motion, (*d*) rate of progression.
3. Physical features of the locality—(*a*) elevation, (*b*) mountains and plateaux, (*c*) rivers and valleys, (*d*) forests, &c.
4. Temperature (*a*) before the storm, (*b*) after the storm, and if possible to be observed (*c*) changes during the storm.
5. Barometrical readings (frequently taken during time of hailstorm).
6. Wind—(*a*) direction near the earth's surface, (*b*) direction in the higher regions as indicated by the cloud motion, (*c*) force.
7. Preceded or followed by rain.
8. Aspect of the clouds. Note if there be any appearance of two separate strata at different elevations.
9. Electrical phenomena. Should there be lightning, note the relation between the discharges and the fall of the hail—whether the lightning precede the hail, or *vice versâ*.
10. Duration of the storm at one spot.
11. Sound. Note if a peculiar noise precede the descent of hail.
12. Conformation and size of the hailstones.
13. General character of the weather before and after the storm.

Notes.—(1) The precise date of a hailstorm is an important point, as it determines the period of their occurrence. Respecting the *annual* period we have the most conflicting testimony. Shortly after the establishment of hail insurance companies valuable statistics were published by those bodies. From particulars furnished by the Farmers' Insurance Institute the following table was drawn up:—

Hailstorms in	January	0
,,	February	1
,,	March	2
,,	April	3
,,	May	7
,,	June	10
,,	July	17
,,	August	4
,,	September	2
,,	October	0
,,	November	0
,,	December	0
		46

Dalton gives the following as the result of five years' observations:—

In	January it hailed on		11	days.
,,	February	,,	7	,,
,,	March	,,	5	,,
,,	April	,,	8	,,
,,	May	,,	11	,,
,,	June	,,	6	,,
,,	July	,,	2	,,
,,	August	,,	1	,,
,,	September	,,	6	,,
,,	October	,,	7	,,
,,	November	,,	7	,,
,,	December	,,	13	,,

Giddy thus sums up twenty-one years' observations at Penzance:—

January	23	July	1
February	25	August	0
March	25	September	5
April	27	October	17
May	7	November	22
June	5	December	43

Thomson ("Introd. to Met." p. 174) gives the following as the relative proportions:—

Winter to all the other seasons as			45·5 to 54·5
Spring	,,	,,	29·5 to 70·5
Autumn	,,	,,	22·0 to 78·0
Summer	,,	,,	3·0 to 97·0

From a comparison of these tables we see that Dalton, Giddy, and Thomson agree in making winter the season of maximum hailfall, while the insurance statistics point to the opposite conclusion, the hailstorms in June and July being much in excess of those in the other months of the year. I strongly suspect however that Dalton, and other observers who have arrived at similar results, included in their enumeration of hailfalls what we may call, in absence of a better name, *winter hail*. It is very unfortunate that the word *hail* has in our language been used to denote two entirely different phenomena, the French *grêle*, or hail proper, and *grésil*, or that small round powdery snow which often falls towards the end of a snowstorm and in the early part of a very frosty night. *Grésil* has nothing in common with *grêle*. The one falls exclusively in winter, and the other, I venture to say, as exclusively in summer.

(6) Dalton observed that the winds which brought hail-showers in the north of England were always south-west, west, or north-west. The wind often shifts erratically. Howard mentions a hailstorm during which it was first east, then south, afterwards west, again east, and finally west. Beccaria makes the following singular statement:—"While clouds are agitated with the most rapid motions, rain generally falls in the greatest plenty; and if the agitation be very great, it generally hails."

I shall be glad to receive references to memoirs and papers on the subject of hail, also particulars of storms, from any of your readers who have them at hand. J. A. B. OLIVER

Athenæum, Glasgow, June 6

How to Prevent Drowning

IN the discussion that Dr. MacCormac's letter has elicited, the essential principle upon which the whole art of swimming is fundamentally based appears to have been overlooked. As Dr. MacCormac says, the human body naturally floats in water, and freely so in salt water; but *how* does it float, supposing the necessary condition of buoyancy, the inflation of the lungs, is maintained? If the limp, dead body of a man is thrown into water in this condition it floats with the head and face immersed, but with that part of the back just between the shoulders upwards, and just bobbing out of water. This is a drowning position, and the first business of swimming is to counteract the tendency to this position, that is, to balance the body in such wise that the head shall be upwards and the lower part of the face uppermost, in spite of the natural tendency of the head to sink, it having a greater specific gravity than water, or the average of the whole body. Dr. MacCormac, in his letter (p. 166), says that "it is just as easy, if we only knew it, to tread water as to tread earth." Quite so; but it is also about as difficult. No human being can "tread earth" without training, the principal effort in this training being directed to keeping the centre of gravity within the base covered by the soles of the feet; and in like manner we must learn to keep the centre of gravity of the body and the centre of its buoyancy in a perpendicular line with mouth and nostrils in the air. I have been a swimmer since I was eight years of age, and consequently swim as naturally as I walk, and float easily in fresh or salt water, without any treading or paddling of any kind; but though I can thus lie basking luxuriantly and motionless, I am just as unable to sleep floating as to sleep standing upright. I have often tried, and immediately I begin to doze my mouth is under water. The effort of keeping the face upwards is as automatic and unconscious as that of standing still on the ground, but there is an effort of balancing nevertheless.

I have taught many to swim, and my first lesson is on balancing the body; the easiest formula for attaining this power is to *keep the hands down and look at the sky* while the chest is expanded as much as possible by throwing the shoulders well back in military attitude. Any man or woman of ordinary specific gravity who can do this can float *and breathe*, but to do it, simple as it is, requires practice or training, physical training of the muscles, and cerebral training in order to acquire that command of all the faculties without which there can be no treading of water or other device for keeping the mouth and nostrils in the air. If this were taught, not on paper, but in the water, to everybody, Dr. MacCormac's object would be attained. As it is, the human being compared with four-legged animals is relatively as inferior to them in water as it is on land. The calf or the colt walks a few minutes after it is born, the kitten or puppy in a few days; but the human infant only after many months. W. MATTIEU WILLIAMS

Royal Polytechnic Institution, June 27

Buoyancy of Bodies in Water

IN NATURE, vol. xxiv. p. 166, Dr. W. Curran says:—"It is, I think, generally assumed in books and courts of law that all bodies, human and bestial, sink as a rule in water as soon as life is extinct." How far this statement may be true as regards animals generally, I am not prepared to say, but it certainly does not hold good as regards the reindeer. The Eskimos spear many reindeer whilst crossing lakes, and it sometimes occupies them an hour or two in "towing" them all to land, yet it is a rare exception that any are lost by sinking, even of the full-grown males, which in autumn are heavily weighted with large antlers.

4, Addison Gardens, June 25 JOHN RAE

An Optical Illusion

WILL you allow me to add something to the letter from William Wilson in NATURE, vol. xxiv. p. 53.

1. The results described may be produced without bending the card or using a square hole. A flat card, with a pin-hole, is held some distance from the eye, and a pin moved so as to be in a right line between the eye and the hole; the results described by Mr. Wilson follow. 2. Some few trials may be necessary in order to get a clear image (if this is the proper term), but it will be found that considerable variation in the distances from the eye to the pin and from the pin to the hole can occur without destroying the effect. 3. The image seems to me to be close to the card in every case, while the distance from the eye to the card may vary a great deal. CLARENCE M. BOUTELLE

State Normal School, Winona, Minn., U.S., June 10

Resonance of the Mouth-Cavity

IN reply to Mr. George J. Romanes, I beg to say that the object of my communication printed in NATURE, vol. xxiv. p. 100, was to show that the mouth-cavity will give a distinct resonance to different rates of vibration *already in the air* by being shaped suitably for each of them (and providing they come within its limit). The mouth thus gives the means of analysing the composite nature of sound. Any one successfully repeating my experiments given on pp. 100, 126, would be satisfied that they pointed to something different to the boys' amusement mentioned in Mr. Romanes' letter (p. 166).

5, West Park Terrace, Scarborough JOHN NAYLOR

American Cretaceous Flora

IN several of the interesting and valuable papers on the Tertiary flora which Mr. J. Starkie Gardner has contributed to the English journals he has referred to the fossil plants in our Cretaceous rocks as representing a flora really Tertiary in character; and, influenced by the modern aspect of the plants contained in our Dakota group (Lower Cretaceous), he has expressed a doubt whether even that should be regarded as truly of Cretaceous age. In a former number of NATURE I endeavoured to show that our Dakota flora was Cretaceous, inasmuch as it is found in rocks which are overlain by several thousand feet of strata containing many mollusks, fishes, and reptiles which are everywhere recognised as Cretaceous, and none that are Tertiary.

Mr. Gardner was not however convinced by my facts or arguments, and in the April number of the *Popular Science Review* he reiterates and emphasises his formerly expressed opinion, referring all our Cretaceous strata to the Maestricht beds, and intimating that, in common with that formation, they should be separated from the Cretaceous system. His language is as follows:—

"The presence of *Mosasaurus* in the Maestricht beds, and the far newer aspect of its fauna, show that it must have belonged to an altogether different period, probably the one represented in America by a great so-called Cretaceous series containing a mixture of Cretaceous and Tertiary mollusks, dicotyledonous plants, and *Mosasaurus*. . . . .

"No American or European so-called Cretaceous land flora can be proved to be as old as our White Chalk."

Now in no spirit of criticism, for I appreciate and value the excellent work that Mr. Gardner is doing, but simply for the vindication of the truth of geology, I ask him to qualify these statements.

I am impelled to this course by the following facts:—

In our Triassic series we have in some places beds of coal and the remains of a vegetation decidedly Mesozoic in character, consisting of Cycads, Conifers, and Ferns, but, as far as we yet know, without a single Angiosperm.

In the Jurassic age the eastern half of the North American Continent formed a land-surface, for the sediments of the Jurassic sea are confined to a somewhat irregular area in and west of the Rocky Mountain belt.

Of the Jurassic flora of North America we as yet know little or nothing; but the continent that bordered the Jurassic sea ultimately became covered with a new, varied, and highly-organised flora, of which the origin is yet unknown.

In the Cretaceous age all the continent lying east of the Wasatch Mountains was affected by a subsidence which brought the sea in from the Gulf of Mexico with a front 1000 miles wide, and the great inland sea thus formed gradually extended northward till it reached nearly, if not quite, to the present shore of the Arctic Ocean.

The waves of the Cretaceous sea in their advance swept before them a shore that was covered with a luxuriant forest of at least one hundred species of Angiospermous trees; and the remains of trunks and twigs, leaves and fruit, were buried up in the sheet of beach material which accumulated all along the advancing shore line, and which now forms the Sandstones of the Dakota group. Up to the present time very few mollusks have been found in this group, and they are not sufficient to fix with exactness its relation to the Cretaceous series of other countries. The plants, too, are distinct from any found in Europe, though they include, with many extinct forms, genera which are common in the living forests of America, such as *Quercus salix*, *Magnolia*, *Fagus*, *Liquidambar*, *Liriodendron*, &c.

When the subsidence which produced the Dakota group was at its maximum the sea stood several thousand feet deep over the central portion of the trough between the Alleghanies and the Wasatch Mountains, and here we now find at least two thousand feet of marine, calcareous, organic sediment, which have furnished hundreds of species characteristic of the Cretaceous age, and a large number that are identical with those contained in the Upper Greensand and Chalk of Europe.

It is true that up to the present time no Neocomian fossils have been found in the interior of the Continent, but with that exception the entire Cretaceous series of the Old World is represented there. Hence it is not true that our Cretaceous "contains nothing so old as the Chalk."

Nor is it true, as intimated by Mr. Gardner, that our "so-called Cretaceous rocks" contain a Tertiary flora and fauna, as no Tertiary species of either has yet been found there. The flora of the Dakota group is more modern in its aspect than that of the Lower and Middle Cretaceous of Europe, but its plants are specifically different from any found in Europe in our Middle Cretaceous (Colorado group), Upper Cretaceous (Laramie group), or Tertiary beds (of Green River, Fort Union, and Oregon). The facts apparently indicate that the earliest development of Angiospermous plant-life took place here, and this in a *temperate flora* of which the descendants long afterwards—in Tertiary times—occupied Greenland, Spitzbergen, &c., and spread by land connections into Europe and Asia.

The best authorities we have had on questions relating to the Cretaceous fauna—Messrs Gabb and Meek—were fully agreed in regarding our Middle Cretaceous as of the age of the Chalk. Mr. Gabb divided the Cretaceous series of California into four members—

1. The Tejon group.
2. The Martinez group.
3. The Chico group.
4. The Shasta group.

Of these the oldest, or Shasta, group was regarded by him as of Neocomian age, the Chico and Martinez groups—which should perhaps be united—as the representatives of the Upper and Lower Chalk, and the Tejon group as the equivalent of the Maestricht beds.

The coal-beds and the fossil plants of Vancouver's Island lie at the base of the Cretaceous series as it exists there, and the molluscous remains indicate that it is the equivalent of the Chico group. The plants are apparently all distinct from those of the Dakota group of the Interior. They include palms and cinnamons, and evidently grew in a warmer climate than that which produced the temperate flora of the Lower Cretaceous of Kansas, Nebraska, and the Atlantic coast.

Among the Vancouver Island Cretaceous plants is one well-known species, *Sequoia Reichenbachii*, H., which is found in

various localities in the Upper Greensand and Chalk of Europe, and also occurs at Kome.

The fossils collected by Mr. Richardson on Queen Charlotte's Island have been shown by Mr. Whiteaves to represent the very base of the Cretaceous series, and to include some forms that are rather Jurassic than Cretaceous. The plants of this group, though few and imperfect, seem to be chiefly Conifers, as in the oldest Cretaceous flora of Europe.

Our present knowledge of the age of the American Cretaceous flora may then be epitomised as follows:—

1. The oldest Cretaceous rocks known in North America, those of Queen Charlotte's Island, represent the Neocomian of Europe, and have so far furnished no Angiospermous plants.

2. The Shasta group of California, supposed to be the equivalent of the upper part of the Neocomian, has yielded no plants.

3. The coal-strata and plant-beds of Vancouver's Island, probably a little later than the Dakota group of the interior, contain many Angiosperms, and are of the age of the Gault or Upper Greensand. Lesquereux's identification of Vancouver's Island plants in the Laramie of Colorado and Eocene of Mississippi is evidently a mistake. There are no species common to these very distinct formations.

4. The Dakota group, the mechanical base of the Cretaceous series of the interior of the continent, which has yielded at least 100 distinct species of Angiospermous woody plants, is certainly older than the Chalk of the Old World.

5. The Raritan sands and Amboy clays of New Jersey, the lowest members of the Cretaceous on the Atlantic coast, contain a flora not less rich than that of the Dakota, with which it has a few species in common. This group of plants has not yet been described, but a large number of specimens are in my hands, from which drawings and descriptions are being made for the State of New Jersey. The flora is that of a temperate climate, consisting mainly of Angiosperms, but it also includes many beautiful Conifers.

6. The Colorado group, or great series of marine Cretaceous beds of the interior of the Continent, represents the strata known in the Old World as the Gray and White Chalk, and the Maestricht beds. Few plants have been obtained from this group in the United States, but I am informed by Dr. Dawson that an interesting collection of plants has been obtained from it on Peace River, in Canada. These will soon be described by him.

7. The Laramie group, or "Lignite series" of the central part of the continent, underlies unconformably the Coryphodon beds, the base of the Eocene, and is in my opinion the upper member of the Cretaceous system. Many of its plants have been described by Mr. Lesquereux in his "Tertiary Flora," but so far as my observation extends it contains no species identical with any found in unmistakable Tertiary rocks.

School of Mines, New York, May 20 J. S. Newberry

GEORGE ROLLESTON, M.D., F.R.S.

PROF. ROLLESTON'S death, which took place at Oxford on June 16, and which we briefly announced in our last number, may well be called premature, as he was in the prime of life, and but a few months ago seemed to all, except a few closely observant intimate associates, still in the plenitude of his powers, and capable of much good work in time to come.

The son of a Yorkshire clergyman, he was born at Maltby on July 30, 1829, and had therefore not completed his fifty-second year. His early aptitude for classical studies, carried on under the instruction of his father, must have been most remarkable if, as has been stated in one of his biographies, he was able at the age of ten to read any passage of Homer at sight. He was not educated at one of the great public schools, but entered at Pembroke College, Oxford, took a First Class in Classics in 1850, and was elected a Fellow of his College in 1851. He then studied medicine at St. Bartholomew's Hospital, joined the staff of the British Civil Hospital at Smyrna during the latter part of the Crimean war, was appointed assistant-physician to the Children's Hospital in London, 1857, but took up his residence again at Oxford in the same year on receiving the appointment of Lee's Reader in Anatomy at Christ Church. In 1860 he was elected to the newly-founded Linacre Professorship of Anatomy and Physiology, which he held to the time of his death. He was elected a Fellow of the Royal Society in 1862, and a Fellow of Merton College, Oxford, in 1872. He was a member of the Council of the University, and its representative in the General Medical Council, and also an active member of the Oxford Local Board.

In 1861 he married Grace, daughter of Dr. John Davy, F.R.S., and niece of Sir Humphry Davy, and he leaves a family of seven children.

The duties of the Linacre professorship involved the teaching of a wide range of subjects included under the terms of physiology and anatomy, human and comparative, to which he added the hitherto neglected but important subject of anthropology, as well as the care of a great and ever-growing museum. In the present condition of scientific knowledge it requires a man of very versatile intellect and extensive powers of reading to maintain anything like an adequate acquaintance with the current literature of any one of these subjects, much more to undertake original observations on his own account. Even a man of Rolleston's powers felt the impossibility of any one person doing justice to the chair as thus constituted, and strongly urged the necessity of dividing it into three professorships, one of physiology, one of comparative anatomy, and one of human anatomy and anthropology. The work which he did however contrive to find time to publish, and by which he will be chiefly known to posterity, is remarkable for its thoroughness. He never committed himself to writing without having completely mastered everything that had been previously written upon the subject, and his memoirs bristle with quotations from, and references to, authors of all ages and all nations. The abundance with which these were supplied by his wonderful memory, and the readiness with which, both in speaking and writing, his thoughts clothed themselves with appropriate words, sometimes made it difficult for ordinary minds to follow the train of his argument through long and voluminous sentences, often made up of parenthesis within parenthesis.

The work which was most especially the outcome of his professorial duties is the "Forms of Animal Life," published at the Clarendon Press in 1870. Though written chiefly with a view to the needs of the university students, it is capable of application to more general purposes, and is one of the earliest and most complete examples of instruction by the study of a series of types, now becoming so general. As he says in the preface, "The distinctive character of the book consists in its attempting so to combine the concrete facts of zootomy with the outlines of systematic classification, as to enable the student to put them for himself into their natural relations of foundation and superstructure. The foundation may be wider, and the superstructure may have its outlines not only filled up, but even considerably altered by subsequent and more extensive labours; but the mutual relations of the one as foundation and the other as superstructure which this book particularly aims at illustrating, must always remain the same."

Besides this work, Prof. Rolleston's principal contributions to comparative anatomy and zoology are the following:—"On the Affinities of the Brain of the Orang Utang," *Nat. Hist. Review*, 1861; "On the Aquiferous and Oviductal System in the Lamellibranchiate Molluscs" (with Mr. C. Robertson), *Phil. Trans.* 1862; "On the Placental Structures of the Teurec (*Centetes ecaudatus*) and those of certain other Mammals, with Remarks on the Value of the Placental System of Classification," *Trans.* Zool. Soc. 1866; "On the Domestic Cats of Ancient and Modern Times" (*Journal of Anatomy*, 1868); "On the Homologies of Certain Muscles Connected with the Shoulder-Joint" (*Trans.* Linn. Soc., 1870); "On the Development of the Enamel in the

Teeth of Mammals" (*Quart. Journ.* Micros. Soc., 1872); and "On the Domestic Pig in Prehistoric Times" (*Trans.* Linn. Soc., 1877).

Latterly he did much admirable work in anthropology, for which he was excellently qualified, being one of the few men who possess the culture of the antiquary, historian, and philologist on the one hand, and of the anatomist and zoologist on the other, and could make these different branches of knowledge converge upon the complex problems of man's early history. The chief results of his work of this nature are contained in his contribution to Greenwell's "British Barrows" (1877), a book containing a fund of solid information relating to the early inhabitants of this island. In this department of science Prof. Rolleston stood almost alone in this country, and we know of no more fitting tribute which the University of Oxford could pay to his memory than to found a chair of anthropology, a subject in the cultivation of which England is fast being outstripped by every other civilised nation. His last publication, and one which is on the whole the most characteristic as exhibiting his vast range of knowledge on many different subjects, was a lecture delivered in 1879 at the Royal Geographical Society on "The Modifications of the External Aspects of Organic Nature produced by Man's Interference."

That Dr. Rolleston has not left more original scientific work behind him is easily accounted for by the circumstances under which he lived at Oxford. The multifarious nature of the subjects with which the chair was overweighted; the perpetual discussions in which he was engaged consequent upon the transitional condition of education both at Oxford and elsewhere during the whole term of his office; the immense amount of business thrust upon him, or which he voluntarily undertook, of the kind that always accumulates round the few men who are at the same time capable and unselfish, such as questions pertaining to the local and especially the sanitary affairs of the town in which he lived, or connected with the reform of the medical profession, both in and out of the Medical Council, which constantly brought him to meetings in London; his own wide grasp of interest in social subjects, and deep feeling of the responsibilities of citizenship, and his sense of the duties of social hospitality, which made his house always open to scientific visitors to Oxford; all these rendered that intense concentration requisite for carrying out any continuous line of research impossible to him.

He was often blamed for undertaking so much and such diverse kinds of labour, so distracting to his scientific pursuits; but being by constitution a man who could never see a wrong without feeling a burning desire to set it right, who could never "pass by on the other side" when he felt that it was in his power to help, nothing but actual physical impossibility would restrain him. For several years past, when feeling that his health and strength did not respond to the strain he put upon them, he resorted to every hygienic measure suggested but one, and that the one he most required, rest; but this he never could or would take. During the last term he spent at Oxford, before his medical friends positively forced him (though unfortunately too late) to give up his occupations and seek change in a more genial climate, he was working at the highest pressure, rising every morning at six o'clock, to get two uninterrupted hours in which to write the revised edition of the "Forms of Animal Life" before the regular business of the day commenced.

It is impossible for those who had no personal knowledge of Rolleston to realise what sort of a man he was, and how great his loss will be to those who remain behind him. No one can ever have passed an hour in his company, or heard him speak at a public meeting, without feeling that he was a man of most unusual power, of lofty sentiments, generous impulses, marvellous energy, and wonderful command of language. In brilliant repartee, aptness of quotation, and ever-ready illustration from poetry, history, and the literature of many nations and many subjects, besides those with which he was especially occupied, he had few equals. "In God's war slackness is infamy" might well have been his motto, for with Rolleston there was no slackness in any cause which he believed to be God's war. He was impetuous, even vehement, in his advocacy of what appeared to him true and right, and unsparing in denunciation of all that was mean, base, or false. To those points in the faith of his fathers which he believed to be essential he held reverently and courageously, but on many questions, both social and political, he was a reformer of the most advanced type. Often original in his views, always outspoken in giving expression to them, he occasionally met with the fate of those who do not swim with the stream, and was misunderstood; but this was more than compensated for by the affection, admiration, and enthusiasm with which he was regarded by those who were capable of appreciating his nobility of character. The loss of the example afforded by such a nature, and of his elevating influence upon younger and weaker men, is to our mind a still greater loss, both within and without the University in which he taught, than the loss of what scientific work he might yet have performed.

Dr. Rolleston's personal appearance corresponded with his character. Of commanding height, broad-shouldered, with a head of unusual size, indicating a volume of brain commensurate with his intellectual power, and with strongly-marked and expressive features, in which refinement and vigour were singularly blended, in him we saw just such a man as was described by the public orator at the late Oxford Commemoration, in words with which we may conclude this notice—"Virum excultissimi ingenii, integritatis incorruptissimæ, veritatis amicum, et propugnatorem impavidum." W. H. F.

THE ZOOLOGICAL SOCIETY'S INSECTARIUM

IN our notice of this recent addition to the Regent's Park collection (*antea*, vol. xxiv. p. 38) we regret to find we have made an error in the name of the curator of the Insectarium. Mr. *William* Watkins (not *E.* Watkins as there given) has made many good additions to the collection of living insects under his charge since we last wrote, and attracts a host of visitors every day to inspect his living wonders. In a report on the Insectarium read at a recent scientific meeting of the Zoological Society by the secretary, Mr. Watkins gave the subjoined account of the progress of the development of the large moths of the family Bombycidæ during the month of May.

GLOVER'S SILK-MOTH (*Samia Gloveri*).—Specimens of this species emerged almost daily through the month, and fertile eggs were obtained, which hatched on the 12th instant. The larvæ when hatched are a shining black, with numerous spines of the same colour; after the first change, which took place in six days, they assume a yellowish colour; at the second moult they become green with paler coloured spines, each tipped with bright red. A choice of many shrubs were given them, but although they ate plum and sallow they left these for gooseberry.

CECROPIAN SILK-MOTH (*Samia Cecropia*).—This species emerged through the month, and copulation was frequent. A large number of eggs were obtained, but many are not fertile, perhaps owing to the stock already having been interbred. Young larvæ hatched on the 14th instant, and are growing well. Food-plant, plum.

AILANTHUS SILK-MOTH (*Attacus Cynthia*).—This species commenced to emerge towards the end of the month, but only four specimens have yet appeared. It is usually the latest species of all.

PERNY'S SILK-MOTH (*Attacus Pernyi*).—Perfect insects of this species were on view throughout the month. Fertile eggs were obtained, which commenced to hatch on the 30th instant, and are doing well. Food-plant, oak.

TUSSEH SILK-MOTH (*Attacus mylitta*).—This species commenced to emerge on 28th instant, a beautiful male being bred; on the following day a male and female emerged. Eggs were obtained, which are probably fertile.

GREAT ATLAS MOTH (*Attacus Atlas*).—Throughout the latter half of the month specimens of this species have emerged, and many fine ones have been preserved. Eggs will probably be obtained later; there are many more still to come out.

INDIAN MOON-MOTH (*Actias selene*).—This first specimen of this species emerged on the last day of the month.

AMERICAN MOON-MOTH (*Actias luna*).—During the early part of the month specimens of this species emerged. Eggs have been obtained, but it is doubtful if they are fertilised.

PROMETHEAN SILK-MOTH (*Telea Promethea*).—The cocoons of this species have as yet only produced a large ichneumon fly (*Ophion*). Many visitors have evinced great interest on seeing these large parasites produced from externally perfectly-formed Lepidopterous cocoons and internally stout well-made oval cocoons of the Hymenoptera.

JAPANESE OAK SILK-MOTH (*Antheræa Yama-mai*).—The larvæ of this species produced from eggs have done fairly well; many are now nearly full fed and about to spin. Food-plant, oak.

Besides these fine silk-moths, which are in many cases likely to be of economical value, Mr. Watkins has succeeded in breeding during the past month examples of many of the finer European butterflies, such as the swallow-tail, orange-tip, black-veined white, and *Apatura ilia*, not to mention numerous Heterocera, Hymenoptera, and Neuroptera. During the present month also many additions have been made to the series.

A guide-book to the Insectarium is in preparation, not, as we are assured, with any idea of forcing visitors to buy it, as every object exhibited is fully and perfectly labelled, but rather for the purpose of making the Insectarium better known, and getting further contributions to it from foreign parts.

DR. BESSELS' ACCOUNT OF THE "POLARIS" EXPEDITION [1]

D. EMIL BESSELS, as most of our readers will remember, was the chief of the scientific department on board the ill-fated *Polaris*, which was sent on her memorable North Pole Expedition by the United States Government in 1871. He finished the text of the present work in the summer of 1874, shortly after the return of the expedition, but postponed the publication until after the appearance of the official account of the voyage, which was edited by Rear-Admiral Davis. He had the misfortune to lose the greater part of his journal and many other papers in his luggage during a railway journey in Scotland.

The remarkable story of the *Polaris* Expedition is well known. Including Captain Hall, the commander of the expedition, the entire number of persons on board the *Polaris* was thirty-three. Of these eight were Esquimaux, consisting of two married couples and their four children, three little girls of ten, eight, and three years of age, and a boy of six. Another boy, who was named Polaris, was born during the voyage in Polaris Bay, on board the vessel. Two of those on board, besides Dr. Bessels, were scientific men, namely, Messrs. R. D. W. Bryan, astronomer, and Friedrich Meyer, meteorologist.

[1] "Die americanische Nordpol Expedition," von Emil Bessels. Leipzig: W. Engelmann, 1879.

The highest point reached by the *Polaris* was lat. 82° 26′ N. at the northern mouth of Robeson Channel. After being beset by ice and having been nipped sufficiently to render her extremely leaky, the ship was moored for the winter about forty miles south of this point in Thank God Harbour, on the east side of Robeson Channel, to the north of Petermann Fjörd. Several sledge expeditions were made from this point, but without reaching a higher latitude than that attained by the ship. Capt. Hall died on board on November 8. In the following summer attempts were made in vain to push further northward, and it was found the ship leaked so badly that it was necessary to return homewards. The ship became beset in the ice on August 16, and remained thus, drifting southwards with the field, suffering constantly from ice-pressure, until October 15, when it was in such jeopardy from the ice-movements that most of the provisions and stores of all kinds and all the boats were passed out on to the ice. The ice parted suddenly, and drifted away from the ship with nineteen persons upon it, including all the Esquimaux, whilst fourteen, and amongst them Dr. Bessels, remained on board. This took place at night. The castaways remained upon the ice 196 days, suffering terrible hardships, and having drifted to the coast of Labrador, were there picked up by a sealing-ship, even the children having survived. They saw the *Polaris* in shore at the commencement of their long journey, and wondered their comrades did not come to their assistance, not knowing that the ship was practically a wreck, and abandoned. Those left on the ship at

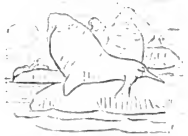


FIG. 1.—Walruses at rest on the ice.

the parting of the ice, keeping the leaking ship with difficulty afloat, and unable to see anything of those on the ice, got ashore near Cairn Point in the middle of Smith's Sound, and having wintered there in company with some Esquimaux families, built some boats from the wreck, and travelling south partly in these, partly on the drift-ice, were picked up in the west of Melville Bay by a whaler, the *Ravenscraig*, on June 23. Thus all engaged in the expedition, excepting Capt. Hall, got back in safety.

All this is related by Dr. Bessels in a most graphic and highly interesting style, and his book is filled besides with interesting accounts of the habits of animals met with, the condition of the vegetation of the region explored, the mode of life of the Esquimaux, meteorological and other scientific observations. We shall touch on a few of these. At Fiskernæs, on the south-west Greenland coast, the author turned over some of the kitchen middens of the Esquimaux, such as are now formed in front of each hut. In a very short time remains of all the eatable vertebrates of the Greenland fauna are to be found in them, and in many cases it would not be difficult to fix the season at which the deposits were made, for in places are found scarcely anything but bird-remains, in other places those of fish, in others those of mussels. Many good dogs' skulls and a number of marrow bones of seals broken for their marrow were found in the middens. At the same place one of the sailors of the *Polaris* nearly lost his life by attempting to perform the feat which most of the Esquimaux accomplish with such ease, of turning their kajak upside down without leaving

their seat, and then righting themselves from under water with a blow of their paddle.

Some interesting remarks are made on the calls used by various Esquimaux tribes whilst dog-driving. All the Esquimaux of that part of Greenland, which is under missionary influence, drive their dogs with the call *i! i! i!* and accompany the sound with a smack of the whip to the right or left when they wish to turn, whilst the dogs are

FIG. 2.—Littleton Island.

stopped by a short whistle. The Esquimaux tribe on the side of Smith Sound use similarly the sound hă! hă! hă! and as a halting signal a lengthened ōh! Those inhabiting the neighbourhood of Ponds Bay, as one of the Esquimaux on board the ship informed the author, use the call wōā-āh-hă-hă-hă! to turn their dogs to the

FIG. 3.—Group of Ita Esquimaux huts with *Polaris* House and the wreck in the distance.

right, and āh-wōā-wā-hă! to send them to the left; ōh! for halt. The calls used in Cumberland, a district of Baffin's Land, are somewhat similar sounds to the last; formerly they were more like them still, but have changed. The natives on the shores of the Hudson's Straits use only the call au! au! au! and those of King

William's Land only kgu! kgu! kgu! Amongst these two latter tribes the whip is almost unknown; the dogs are led or turned from side to side by means of a piece of wood which the driver throws out on the side from which they are to turn. The Esquimaux of Alaska appear to have no stereotyped driving cries, but merely to use various oaths promiscuously as they come to mouth.

During one of the author's sledge expeditions, where the going was very bad in consequence of the yielding of the snow, the dogs could hardly be got along with the whip, so two Esquimaux who accompanied him took turns to run in front of the team, trailing a fish tied to a string. The dogs struggled to get at the herring, always out of their reach, and excellent progress was made.

Some interesting details concerning the habits of Esquimaux dogs are given by the author. The instant halt is called by the driver the dogs throw themselves to the ground with their snouts between their forepaws; they rise again to stretch, and then lie down again at once. Two Newfoundland dogs which belonged to the *Polaris* pack gradually assumed similar habits; but before lying down they always turned round and round in their resting-place, like all dogs except the Esquimaux breed, for the author never saw an Esquimaux dog do this. Mr. Darwin, as will be remembered, has explained this habit of turning round before lying down, invariably to be observed in other domestic dogs, as a survival of the instinct of the wild ancestor, which leads him to form a bed in the grass by this means. Every one has heard of the extraordinary voracity of the Esquimaux dogs; they will even sometimes snap off a piece of their master's flesh if carelessly exposed. One day, on board the *Polaris*, the porcelain door-handle of one of the cabins fell off with the usual square rod of iron attached to it. Five or six of the dogs made a rush at it, there was a momentary struggle, the dogs were hastily driven away, but the door knob was already swallowed. The dog that ate it was none the worse, nor the handle either in the end. An Esquimaux told the author that the following were the points to be noticed in selecting a good dog:—a broad breast, short ears, strong legs, large feet, low loins, and a moderately long tail. The tail must not bend too near its root, as this shows the loins to be weak.

The descriptions of the Esquimaux and their habits throughout the work are worth reading. The most interesting are those relating to the Ita Esquimaux, inhabiting the north shore of the Foulke Fjörd, with whom the author and his companions spent their second winter. They consisted of nine men, three women, and eight children, who crowded at night the small house built by the shipwrecked party, and as there was no room for them usually to lie down, slept sitting with their backs against the walls. The floor measured only twenty-two feet by six, yet had to accommodate thirty-four persons, and once thirty-eight. It was no use erecting a tent for the visitors under the lee of the house; they preferred the close quarters inside.

The author's principal friend was Awatok, the priest of the tribe. He usually accompanied him when it was his watch, on his hourly rounds to the meteorological instruments. "We walked generally arm-in-arm, and when there was no snow drifting sang the tune of the spirited student's song, 'Was kommt dort von der Höh',' using bum—bum—bum instead of the words. After a little while he learnt to hum the tune fairly well." After some time the natives built snow huts near the *Polaris* house, and settled for the winter. The first to do this was one named Stokirssuk, but called "Jimmy" by the *Polaris* people. He was born near Cape Searle, about 650 miles south of Port Foulke. Whilst he was a youth he and his father left their home and wandered north and reached Cape Isabella, where they fell in with an Esquimaux tribe, of whose existence they had been ignorant. Here Jimmy married a wife with tattooed face, and five summers before the arrival of the *Polaris* had moved up thence to Ita, in a company consisting of a woman's boat and four kajaks. He had forgotten how many persons composed the expedition. They found Capt. Hayes' lifeboat on Littelton Island and destroyed it, and discovering the observatory at Port Foulke which Hayes had left filled with provisions and other things, lighted a fire there to cook birds. Unfortunately close to the fireplace was a canister full of powder; the observatory was blown up, and several persons killed and wounded. Jimmy related, his face beaming with laughter, how his father-in-law was killed, and indicated with a movement of his hand how the old fellow was shot up into the air. A dog which had accompanied Jimmy during all his wanderings was still fresh and strong.

Another noticeable native of the band was Majuk Kane's former companion; he was always hungry and a beggar. He named his youngest son, scarcely six weeks old, Dâkta-kè, which meant no more or less than Doctor Kane. This he did in order to flatter the *Polaris* people and ingratiate himself. Sometimes he brought a walrus liver or a few tongues, and got bread or tinned meat for them, at others a skin to get a harpoon for it. But in some moment, when unwatched, he would eat the tongues himself and carry off the liver again; but he did it so innocently that it was impossible to be angry with him. At one time during the winter the Esquimaux were nearly starving, yet one of them—Awatok—would not beg for his family, and when a present was at last sent to him of bread and bacon, had already killed five of his dogs to keep his wife and children alive. His strength of character and power of self-denial were remarkable.

The Ita people have no boats, and do not possess the bow and arrow, although words for these things still exist in their language. These facts show a very remarkable degradation, especially in a hunting people. Jimmy alone had a bow and three arrows. They had often been mended, and being very seldom used, were in a wretched condition, and Jimmy himself was a very bad shot.

One burial took place during the stay of the author. The corpse was wrapped in skins placed on a sledge, and buried in the snow with the face turned westwards. After the body was covered the sledge was turned over on top of it, and the hunting implements of the deceased laid by it. The men plugged their right nostril with hay, and the women their left, and these plugs were worn for several days, and only taken out when the wearers entered a hut. When it is possible a heap of stones is usually raised over the corpse. The nineteenth chapter is devoted to an ethnological sketch, in which the culture and characteristics of the various Esquimaux tribes are compared.

A good many musk-oxen were met with, and the author gives a valuable account of the habits of this animal. None of those killed by the *Polaris* people had a very marked musk smell. The author is uncertain whether this peculiarity is to be attributed to the very high latitude in which they were obtained, or to their having been killed out of the breeding season. No difficulty was found in distinguishing the tracks of these animals from those of reindeer, although some former observers have not found this easy. In all the herds there are from ten to twenty cows to one bull. Their voice is somewhat like the snorting of the walrus, and never resembles in the least the cry of the goat or the sheep. When danger approaches they never give signal with their voice, but only by stamping or striking their neighbour with their horns. They have dire combats with bears sometimes, and often come off victors.

A report, as will be remembered, was spread by newspapers at the time of the return of the expedition, that the *Polaris* had discovered walnut driftwood in the high north, and gave the author as an authority for the statement. Nothing however but coniferous wood was in reality

found. One of the most important matters contained in the book is the author's account of a Bathybius-like albuminous substance which he discovered in the mud composing the sea-bottom, at a depth of ninety or ninety-five fathoms north of Smith Sound. The specimens of mud were brought up in a water-bottle apparatus, about a large spoonful being obtained each time of sounding. This mud was very sticky, and showed itself under the microscope to consist of a yellowish gray mass with numerous opaque lime particles embedded in it. If some of the mud was left to rest in a hollowed out glass slip for some time the albuminous masses exhibited unmistakable amœboid movements, and took into their substance particles of larmine. The substance is named *Protobathybius*.

We cannot follow the author further. His book is well worth reading, and only escaped notice here sooner through accident. It is well illustrated throughout. He takes exception on the ground of priority to the name Palæocrystic Sea, which Sir George Nares conferred upon the expanse which the Americans had previously named Lincoln Sea. He states that, owing to the neglect of his work by an assistant, numerous serious errors occur in the official volume of scientific results of the *Polaris* expedition already published, especially in the meteorological department. These are corrected in the appendix to the present volume, which contains also much other scientific matter. He finds fault throughout his book with the conduct of the ice-master of the *Polaris*, S. O. Buddington, and considers that the ship might have reached higher latitudes if, on two occasions which he believes were favourable, a push had been made northward. He accuses Buddington of not even going up into the crow's-nest as often as he should have done to examine the state of the ice. Some official correspondence which passed on board the ship on these questions of the management of the expedition is given in the book. The manner in which the meteorological observations were kept up after the shipwreck, and the devotion with which Dr. Bessels attempted, though in vain, to sledge far north after the wreck from *Polaris* house are highly creditable.

The book is dedicated to the Arctic explorer, Capt. A. H. Markham, R.N., who with great kindness, and at very considerable inconvenience, shared his cabin on board the whaler *Arctic* with Dr. Bessels on the voyage to Dundee, the *Ravenscraig* having fallen in with the *Arctic* on the whaling-grounds.

H. N. MOSELEY

THE COMET

THE comet which, so far as we are yet informed, was first astronomically observed in the southern hemisphere on May 29, is now well under observation in these latitudes, and as its position will become more and more favourable, it will be a mere question as to how long our telescopes will show it, what data may be obtained for an accurate determination of its orbit. The elements appear to have some resemblance to those of the great comet of 1807, to which reference was made in Dr. Gould's early telegram from the observatory at Cordoba, but the identity of the comets appears highly improbable after Bessel's classical memoir containing a rigorous investigation of the orbit of the comet of 1807, which he followed until the perturbations of the known planets had ceased to be sensible. We may briefly recall the circumstances attending the appearance of that body and one or two main results of Bessel's investigation. According to Piazzi it was first detected by an Augustine monk at Castro Giovanni in Sicily on September 9, but the first regular observation was made on the 22nd of the same month by Thulis at Marseilles. From this time the comet's positions were determined at every opportunity by Bessel, Olbers, Oriani, and others until the end of February, 1808, and on the 18th of the following month Wisniewsky, favoured by a very acute vision and the clear skies of St. Petersburg, observed the comet again, and succeeded in fixing its position until the 27th. In consequence of a notification from Olbers, that with powerful telescopes there might be a possibility of observing the comet again as the earth overtook it to some extent in October and November of the same year, Bessel, then working with Schroeter at Lilienthal, closely examined its track with reflectors of 15 and 20 feet focal length, and on November 9 did succeed in finding an extremely faint nebulosity near the computed place of the comet, which he could not find subsequently, but as the position differed 12′ from that assigned by an orbit which he considered very exact, he came to the conclusion that the object he observed was not the comet of 1807, but another one which happened to be in the vicinity, and which was not seen elsewhere. The discussion of the six months' observations of the comet appears in the masterly treatise to which we have referred, viz., "Untersuchungen über die scheinbare und wahre Bahn des im Jahre 1807 erschienen grossen Kometen," published at Königsberg in 1810. The method of determining the perturbations of a comet due to planetary attraction, which is detailed in this memoir, was long practised by the German astronomers in similar cases.

Bessel inferred from his researches that at the perihelion passage of the comet on September 22 it was moving in an ellipse, with a period of revolution of 1714 years, which was reduced to 1685 years at the date of Wisniewsky's last observation, and continuing his computation of the perturbations to March, 1815, when the effect of planetary attraction had become very small, he found the period further reduced to 1543 years.

The general aspect of the comet now visible as viewed in an excellent *Cometen-sucher*, reminds us of the appearance of the comet of June, 1845, discovered by Colla, which was observed under very similar circumstances, and it may be mentioned that Encke stated at the time that the comet of 1845 reminded him strongly of the great comet of 1819, which passed across the sun's disk on June 26.

The present comet appears to have been at its least distance from the earth about June 21, and should soon present a material diminution of brightness. In perigee its distance would be about 0·3.

[Since the above was in type we have received observations from Dr. Elkin, of the Royal Observatory, Cape of Good Hope: After a week of overcast sky the comet was found there on May 31. Mr. L. A. Eddie, F.R.A.S., of Graham's Town, saw it on May 27, and others claim to have seen it two days earlier. On June 4 the tail was 6° long, coma 20 minutes, and nucleus 20 seconds in diameter; the comet was as bright as α Columbæ.]

The following opinions of American astronomers have appeared in the *Daily News*. That paper, with wonderful journalistic enterprise, has not hesitated to telegraph nearly a column of matter from America on this subject:—

"Prof. Stone, of the Cincinnati Observatory, thinks it is not the comet of 1812, because of its not moving in a southerly direction, but that it may possibly be that of 1807. Professors Eastman and Skinner, at the Naval Observatory, succeeded in getting some fair observations of the comet on Friday night, although the night was not altogether favourable. Prof. Skinner describes the comet as having an extremely bright nucleus, which presented a very ruddy appearance. The observers did not know whether this appearance was normal, or was due to the prevailing atmospheric conditions. Prof. Skinner estimates the tail, which is fan-shaped, at about eight degrees in length. It was also ascertained that in twenty-two minutes the comet travelled three seconds in arc, and in an hour nine seconds, giving it a daily rate of travel northward of about three degrees thirty-six seconds. Computing its motion from its position when discovered,

Prof. Skinner found that in two days and a quarter the comet moved about ten degrees. On Saturday morning it became distinctly visible at 1·45, and could be seen until the sun rendered invisible all the stars except Venus. The astronomers at the observatory maintain their opinion that this comet is identical with the one recently observed by Dr. Gould in Buenos Ayres. Prof. Skinner describes it as a much finer comet than Coggia's, which appeared in 1874, and brighter than any since that which appeared in 1843.

"Prof. Newcomb said that as all the observations made on the comet of 1807 showed it to have a period of nearly 1700 years, it seems out of the question that under any circumstances the same comet could have returned in so short a time as seventy-four years, unless it has passed in the vicinity of some larger planets, which it could not have done. From Dr. Gould's telegram it may be inferred that the comet was very near the orbit of that of 1807 when he observed it. Prof. Newcomb is inclined to think that it is a case of two comets moving in nearly the same orbit, rather than the return of the same comet. One reason for this is that if it had been a periodical comet returning every seventy-four years, it could not have failed to have been observed on former occasions, because it would have returned in 1734 and in 1760. In neither of these years was any such comet observed. The position of its orbit is such that it could hardly have failed to be seen had it returned. Prof. Henry Draper has photographed the comet. To obtain such a photograph as he would like, he said, the plate ought to be exposed for at least an hour, but he had succeeded in getting an exposure of only seventeen minutes. The result, however, was satisfactory, in so far that it demonstrated the possibilities of photographing a comet. It showed the nucleus and the coma and part of the tail. He will try again to obtain a longer exposure. He wishes to take a larger photograph if possible, to examine more carefully the structure of the tail; but the larger the photograph, the more difficult it is to obtain, owing to the diffusion of the light. If he succeeds in obtaining two good photographs he will next turn his attention to its spectrum, which is much more difficult to photograph than the orb itself.

"Prof. Bois, of Dudley Observatory, at Allebuy, secured a number of valuable observations. He says that at two o'clock on Saturday morning its appearance both to the naked eye and in a telescope was magnificent. The head of the comet was very bright, and the tail thirty or forty minutes broad, extending nearly 20° toward the North Star. The tail was very diffused and nebulous, spread out in fan-like form. Looking in the great telescope of thirteen inches aperture, a multitude of details became revealed which are not visible to the naked eye. The head was there seen to consist of a condensed nucleus, apparently about as large as Jupiter seen in a telescope, but of far greater intensity of light. A spray of brilliant rays spread out from the nucleus on the side nearest the sun, then, turning backward, mingled with the elements which form the tail. This resembled the jet of a fountain very closely in its general features. The tail itself extended in a direction diametrically opposite the sun. The whole field of the telescope was filled with glowing nebulosity. I am inclined to think that this comet has not been seen before this year, in modern times at least. It is probably the same comet as seen in South America. It is now certain that this is not the long-expected comet of 1882. It is almost equally certain that it is not the comet of 1807. The period of the comet of 1807 is about 1,700 years. Prof. Swift of Rochester says the comet grows smaller and brighter in nucleus, showing that it is approaching the sun. The head is active, and the tail does not obscure the stars. He thinks it will be visible several weeks. He cannot yet determine if the comet was ever before seen. Great activity is apparent in its head."

M. Janssen has presented to the Academy of Sciences, at its sitting of June 27, the *cliché* of a photogram of the comet, which was taken with the large telescope he described a few weeks ago, constructed for the purpose of astral photography. He obtained also a series of photograms of the nucleus, for which he varied the time of exposure. The results prove that the brightness is not more than that of a star of the fifth magnitude. On the photograph, which will be printed in NATURE, and which our correspondent has examined, the stars are visible through the tail. M. Faye delivered a speech praising the success realised by his colleague, and remarking that it was the first time that a comet had been photographed. The opinion that the tails of comets are merely an illusion, as professed by Seneca in his "Quæstiones Naturales," seems to gain ground, owing to the extraordinary transparency of these appendages.

WE have received the following communications:—

THE following positions which I obtained of the path of the comet may interest your readers:—I saw it first on

FIG. 1.

Wednesday, June 22, at 10.55, during a break in the clouds for about a minute. It was then brighter than it was last night, the 27th, when I saw it well, the sky being clear. The colour is of an orange tinge, and the tail extends to about 10°, but can only be seen so far by sweeping across it. It has changed wonderfully since I first observed it, as will be seen by the drawings which I send you. The No. 1 Drawing shows a most singular appendage round the nucleus, in shape like a milkmaid's yoke, the nucleus occupying the hollow, which was black. From the nucleus two horns projected; they were as bright as the nucleus.

The first envelope on the preceding side took a sudden bend from the circular form to a straight line, while the outer envelope retained its parabolic form.

Last night, the 27th, I was surprised to find a great change had taken place—the yoke-shaped central luminosity appeared as if it had turned round in the nucleus

Fig. 2.

Fig. 3.

and occupied the usual position, while the nucleus itself had thrown out a bright tail, which gave it the appearance of a small comet lying across the bright envelope (Figs. 2, 3). The outer envelope on the following side was interrupted in its continuity, or seemed wanting.

The positions I got are as follows:—

	G.M.T. h. m.	R.A. h. m. s.	N.P.D. ° '
June 23	11 0	5 33 59	44 55
25	10 0	5 41 41	36 52
27	10 0	5 50 34	29 50
27	12 0	5 55 54	29 42

so that last night it crossed the path of Brorsen's comet of April 30, 1879. R. S. NEWALL

Ferndene, June 28

Photographic Spectrum of Comet

On Friday night (June 24) I obtained, with one hour's exposure, a photograph on a gelatine plate of the more refrangible part of the spectrum of the comet which is now visible. This photograph shows a pair of bright lines a little way beyond H in the ultra-violet region, which appear to belong to the spectrum of carbon (in some form) which I observed in the visible region of the spectra of telescopic comets in 1866 and 1868. There is also in the photograph a continuous spectrum in which the Fraunhofer lines can be seen. These show that this part of the comet's light was reflected solar light.

This photographic evidence supports the results I obtained in 1868, showing that comets shine partly by reflected solar light, and partly by their own light, the spectrum of which indicates the presence in the comet of carbon, possibly in combination with hydrogen.

Upper Tulse Hill, June 27 WILLIAM HUGGINS

COMET 1881 *b* was well observed at the Royal Observatory, Greenwich, on June 24 and 25. Its position was determined on both evenings with the altazimuth and transit-circle. The following are the places deduced from the meridian observations (uncorrected for parallax and aberration):—

	G.M.T. h. m. s.	R.A. h. m. s.	N.P.D. ° ' "
1881 June 24	11 25 40	5 38 38·94	40 36 39
25	11 25 55	5 42 51·06	36 39 30

The observation on June 25 indicates the following corrections to the ephemeris computed by Mr. Lohse from the elements communicated by the Emperor of Brazil (Dunecht Circular, No. 21):—Correction in R.A. + 9m., in Dec. + 6° 23'. On June 24 the head was estimated to be brighter than Vega or Arcturus, notwithstanding its low altitude, and on June 25 it appeared decidedly brighter than Arcturus, the star being at about 10° greater altitude than the comet. The tail, which was slightly curved (convex towards the preceding side), was traced to a distance of about 8° on June 24, and 10° or more on June 25, its general direction pointing to the star 2 Ursæ Minoris, about 3° east of Polaris. With the Sheepshanks equatorial (6¾ inches aperture) the head showed the want of symmetry that has been remarked in some other comets. On June 24 the preceding side was much the brighter, there being a strong brush or arc of light on that side, with a bright fan close to the nucleus and a much smaller arc on the following side, the two arcs appearing to spring from the nucleus on opposite sides, and higher up to interlace. A very remarkable feature was a straight wisp of light extending from the nucleus nearly along the axis of the tail. On June 25 this had become much less striking, and the appearance of the head had entirely changed. The following side was then much the brighter, and the general appearance was that of a parabolic envelope, with a much brighter unsymmetrical parabola placed within it, the latter having its focus on the following side of the nucleus, and its axis turned round in the direction *n p s f* from that of the tail.

The greater part of the head gave a bright continuous spectrum, obliterating the usual cometary bands, but one portion showed three bands, in the green, blue, and violet respectively. Measures of the principal band in the green showed that it coincides with the band in the first spectrum of carbon (blue base of flame) at 5165, and not with that of the second spectrum (vacuum-tube) at 5198. The bands in the blue and violet appeared to correspond, as nearly as could be estimated, with bands in the first spectrum of carbon. These observations were made with the half-prism spectroscope mounted on the 12¾-inch equatorial, a dispersive power of about 18½° from A to H being used, with a magnifying power of 14 on the view-telescope, as in the measures of star-motions in the line of sight. No decided polarisation was detected either in the head or the tail. Cloudy weather has prevented any observation of the comet since June 25.

W. H. M. CHRISTIE

Royal Observatory, Greenwich, June 28

IN a letter to the *Times* of the 25th inst., Mr. Ranyard says of Comet *b* 1881, at present visible in the northern heavens:—"In general brightness it decidedly outshone the star Capella. . . . With a direct-vision spectroscope of five prisms, and a 3½-inch telescope, its nucleus and head gave a continuous spectrum, on which I could not detect any bright bands." Last night, June 27, shortly before midnight, the brilliancy of the nucleus had considerably decreased, and yet with a five-prism direct-vision spectroscope I could see most clearly, along with the continuous spectrum, three green bands, not only in the nucleus, but also in the surrounding coma. Two of the bright lines were still strong in the neighbourhood of the nucleus, even where the continuous spectrum was very faint. I used an ordinary Browning spectroscope on an 8-inch achromatic. A small McClean's spectroscope gave only the continuous spectrum, as seen by Mr. Ranyard. This was, I think, due merely to the brightness of the continuous spectrum; for in the Browning instrument the bright lines over the continuous spectrum were not very conspicuous with a wide slit, but on narrowing the slit the green bands became much brighter than the rest of the spectrum.

On June 25 the night was rather cloudy, but good observations for position were obtained by observing the transit of the comet and of δ Aurigæ over the wires of an eyepiece of the equatorial. The mean of four wires gave R.A. 5h. 42m. 51·34s., at G.M.T. 12h. 11m., the North Declination being 53° 30' 57"·6.

On June 27, towards midnight, the double envelope surrounding the nucleus was clearly defined in the telescope, as was also the bright bundle of rays, which spread out in the direction of the sun and extended to a point in the coma about half way between the bounding lines of the inner and the outer envelopes. The direction of these bright rays, which were very vivid, was not quite opposite the direction of the tail, and the latter was very slightly curved.

A transit of the comet, *sub Polo*, was observed last night, June 27, at G.M.T. 11h. 38m., and this, combined with the corrected reading of the meridian circle, gives the following position:—R.A. 5h. 52m. 46·31s., North Declination 60° 13' 40"·6. The length of the tail, clearly discernible to the naked eye, was last night about 9°.

S. J. PERRY

Stonyhurst Observatory, Whalley, June 28

THE following places have been obtained with the transit-circle when the comet passed *sub Polo*:—

Date.	Greenwich Mean Time of observation. h. m. s.	Observed Right Ascension. h. m. s.	Observed North Polar distance (uncorrected for parallax). ° ' "
(*a*) June 23	11 31 54·2	5 35 55·2	44 53 20·6
(*b*) ,, 24	11 30 42·6	5 38 39·9	40 35 33·7
(*c*) ,, 25	11 30 58·3	5 42 52·2	36 38 27·4
(*d*) ,, 27	11 33 2·8	5 52 50·2	29 46 5·8

Remarks.—(*a*) The nucleus distinct but nebulous. Tail bright, and estimated 15° in length. Observation good.

(*b*) Observation difficult, owing to cloud.

(*c*) Nucleus better defined than on June 23, but not so bright. Length of tail estimated at 15°. Observation good.

(*d*) Observation fair, very cloudy. Tail 12°-15° in length

Radcliffe Observatory, Oxford E. J. STONE

THE comet was examined spectroscopically here last night at 11·30. The nucleus gave a bright continuous spectrum, while the coma and brighter portions of the tail gave the three least refrangible hydrocarbon bands superposed on a faint continuous spectrum. On moving the slit of the spectroscope towards the fainter part of the tail the bands died out, leaving a faint continuous spectrum, which again gradually faded away as the end of the tail was approached. I have not measured the position of the bands, but they are sensibly the same as those from an alcohol flame. GEORGE M. SEABROOKE

Temple Observatory, Rugby, June 28

NOTES

THE Lords of the Committee of Council on Education, in reply to an application for aid to science teachers attending the classes of the Mason Science College two days a week, agree to pay three-fourths of the fees for the chemical and physical laboratories and for biology and histology, for a limited number of teachers, on condition that satisfactory terminal reports of their progress (ascertained by examination) and of their conduct be received at the end of the Michaelmas, Lent, and Easter terms. Applications for the privilege must be made to the Secretary, Science and Art Department, not later than August 31. The selection will rest with that Department. One-fourth of the fee for the whole session must be paid by the student on entrance; and the remaining three-fourths will be paid by the Department in equal instalments at the commencement of each term, if the reports are satisfactory.

THE fine library of the late M. Chasles is to be sold by public auction between June 28 and July 18. It contains no fewer than 3936 works, or about 15,000 volumes, and is one of the most complete libraries of mathematical works in existence. The precious manuscripts and various works of history and philology will doubtless be eagerly sought by amateurs. There is, among other works, a Geography of Ptolemy of Alexandria, printed at Rome in 1490, containing geographical maps which are the first engraved with copper plate (1478). The collection includes eighteen different editions of Archimedes, and the works on Euclid number sixty-six. The astronomical works of the sixteenth, seventeenth, and eighteenth centuries are fully represented, as also those on astrology, alchemy, &c.

THE programme of an excursion by the Geologists' Association to the Lake District, from Monday, July 18, to Saturday, July 23, has been issued. Keswick will be the centre of operations till Friday, when the Grasmere and Ambleside district will be visited. Saturday will be given to Windermere.

WE have also before us an attractive programme of a marine excursion to Oban and the West Highlands of Scotland by the Birmingham Natural History and Microscopical Society. The party leave Birmingham on the evening of July 1, and go direct by Greenock and the Kyles of Bute to Oban, which is made the centre for various excursions by sea or land, till July 12. Facilities for dredging will be afforded. The party will include some able naturalists.

IN a recent issue we gave some account of the Ben Nevis Observatory (so-called). The system has been in operation since June 1, and the daily observations by Mr. Wragge are published in the *Times*. This gentleman begins his magnanimous toil up the hill every morning at 5 o'clock. After spending about an hour on the top (9 to 10) in taking observations with the scanty stock of instruments fixed on stands protected by a stone screen, he gets home again by about 2 in the afternoon. In the early part of June the path up the mountain was often deep in snow and enveloped in mist, but Mr. Wragge has marked out the track with a succession of cairns. Anything more disgraceful to British science than this state of things, as representing our present achievement in the way of regular mountain-observations, it is difficult to conceive! A comparison with what has been accomplished in other countries, notably America, where well-equipped observatories are now to be found at vastly greater heights than the top of Ben Nevis, is sufficiently humiliating for us.

THE May number of *Naturen* gives the first of a series of papers, by Prof. Axel Blytt, on the "Theory of the Immigration of the Norwegian Flora at Different Earlier Geological Periods." In this paper the author, who is well known as the highest authority among Scandinavian botanists, describes the character of the flora, which, considered generally, is represented by only a small number of genera. At an elevation of 4000-4500 feet above the level of the sea the interior and southern districts exhibit dwarf forms of the willow and birch, with juniper; between 3000-3500 feet the first birch woods appear in the same districts, while firs and pines begin a few hundred feet lower. Here and there the high and barren fjalds of the interior near the glaciers are broken by the occurrence of blooming oases of plants of Arctic continental forms, which, after having lain buried for months under the snow, awaken to new life with the return of the summer sun. To the interior also belongs a boreal flora of small deciduous trees, including the oak, ash, alder, lime, &c., which penetrate as far as 2000 feet, and in the Inner Sogu district occur the only woods of elm and wild cherry to be found in Norway. The subboreal belt, including several Spiræas, *Fragaria collina*, *Artemidia campestris*, *Thymus chamadrys*, &c., is limited to the Lower Silurian formations in the eastern districts. The western coast-lands between Stavanger and Christiansund are the habitats of an Atlantic flora, including *Erica tetralix*, and several of the rarest Norwegian plants, but here Calluna, Sphagna, and Carices, with turf beds, constitute the principal forms. The most southern littoral belt near Christiansund presents a sub-Atlantic flora, while a number of sub-Arctic forms appear scattered over the whole of Norway. Prof. Blytt considers that the sporadic occurrence of the various continental and insular forms of the flora of Norway points to the conclusion that the climate has undergone various secular changes since the Glacial period, the continental forms having immigrated during the continuance of drought, when the peninsula was connected with neighbouring continents, while the appearance of the insular forms was contemporaneous with rainy periods.

THE decree appointing sixty-five French members of the Congrès d'Électricité has been signed by the President of the Republic, and will soon be published. Foreign Governments will appoint all their own members. Reporters and the public will not be admitted to the Congress; an official report will be published by the general Committee. Some French papers have already condemned such practice in strong terms. No jurymen will be appointed by the exhibitors, and the latter will have no direct influence on the verdicts. It is proposed to consult the Congress on certain measures of general interest, *e.g.* the adoption of electrical units. The electric railway station will be placed inside the building. For want of time, no viaduct will be constructed, and the rails will be laid on the common roads. The space allotted to English exhibitors on the ground-floor has been largely occupied. In addition to this space each of the British light exhibitors will have on the upper floor a special saloon to illuminate with his own system. The right of publishing and selling the French Catalogue has been purchased by the printers and publishers of *La Nature*, rue de Fleurus. The sale of scientific papers will be authorised, but will take place exclusively through their agency.

IN an old book—"Thomæ Bartolini Acta Media et Philosophica Hafniensa Anno 1674, 1675, et 1676," Herr Budde has

come upon a passage which bears on the history of the diving-bell. Bartolini there writes: "Singulare instrumentum invenit descripsitque Franziscus Kesler Wetzlariensis in secretis suis Oppenheimii editis 1616, capite VI., quod Wasserharnisch vocat, quo tuto ambulemus in fundo maris, legamus ibidem, scribamus, edamus, potemus, cantemus, sine periculo vitæ longiori tempore, omnia pergamus, thesauros eruamus et abscondamus," and so on. Of the two figures one represents the interior—a rough framework of wood, having straps with which the diver secures himself in the bell; the other (see *Wied. Ann.*, No. 5) shows the exterior, an inverted vessel of tumbler shape having five or six small circular windows at the top, while the man's legs project little below. According to Poggendorff (it is stated) the oldest book in which the diving bell is mentioned is of 1664, and it refers to a work of Taisnier (as the source of information), the date of which Poggendorff does not give.

We learn from *L'Électricité* that there is being made near the Palais de l'Industrie a basin, 16 metres in diameter, which will be put at M. Trouvé's disposal for exhibition of his boat driven by electricity. In the centre will be an electric light on a pedestal. At various points within the Palais de l'Industrie will be placed (under the direction of MM. Ranvier, Berger, and Fontaine) models of statues as supports for the electric light in its various forms.

The Municipal Council of Philadelphia has granted to a Company the right to place 3000 kilometres of wire on posts for telephonic purposes. No tax is imposed, but a limit is set to the subscription. The fire-telephones in Berlin have proved so useful, that the municipal authorities are increasing the number. In Paris the development of telephonic lines amounted to 9121 kilometres, the extent of wire being double this.

An electromagnet of enormous dimensions has lately been made by Herren von Feilitzsch and Holtz for the University of Greifswald. The case is formed of twenty-eight iron plates bent into horseshoe shape, and connected by iron rings so as to form a cylinder 195 mm. in diameter. The height is 125 ctm.; the total weight 628 kilogr. The magnetising helix consists of insulated copper plates and wires having a total weight of 275 kilogr. (For further details see *Les Mondes* of June 23.) With fifty small Grove elements the magnet will fuse in two minutes 40 grammes of Wood's metal in the Foucault experiment. The plane of polarisation is rotated in flint glass after a single passage, &c. The core of the largest magnet hitherto known, that of Plücker, weighed 84 kilogr. and the wire 35 kilogr.

Mr. W. Mattieu Williams, F.R.A.S., F.C.S., author of "The Fuel of the Sun," "Through Norway with a Knapsack," &c., has been appointed to the management of the Royal Polytechnic Institution, Limited, and will commence his duties at once.

Mr. H. C. Russell, Government astronomer, has just sent home his report on the results of rain and river observations made in New South Wales during 1880. In regard to the latter part of the subject Mr. Russell remarks that it seems impossible to doubt that an unlimited supply of water passes away underground, more indeed than would suffice to make the western districts of the colony a well-watered country, and all that is wanted to make the supply available is a judicious use of the boring-rod. The report is illustrated by an interesting rainfall map of New South Wales, and another on which are given curves showing the height of the western rivers during the year.

Four shocks of earthquake occurred at Agram on the night of June 22-23; rather severe shocks were felt on June 22 at Bohyhad (10.20 p.m.) and Szegszard (11 p.m.) in Hungary.

A skeleton of an *Ursus spelæus* was found this week in a cave near Spanheim (Germany).

The arrangements for the International Medical and Sanitary Exhibition are now complete; the offices are removed from the Parkes Museum to the Exhibition Buildings at South Kensington. The Right Hon. Earl Spencer, Lord President of the Council, has accepted the office of president, and will be present at the opening ceremony on Saturday July 16. The Exhibition is to be complete on Wednesday, July 13, and the judges will make their examinations for the awards on the two days previous to the opening.

In consequence of the increasingly numerous cases of myopia developed in French schools through bad arrangement of seats and distribution of light, the Minister of Public Instruction has nominated a commission named De l'Hygiène de la Vue dans les Ecoles, whose object will be to study the influence of the material conditions of school arrangement on the progress of myopia, and to discover the means of counteracting the evil.

A careful study of the chief methods in use for the chemical examination of potable water, so far as organic matter is concerned, has been undertaken by order of the U.S. National Board of Health. Medical men throughout the country, and others interested in sanitary matters, have been requested to report to Dr. Mallet of Virginia University any well-marked case of disease from impurities in drinking-water, and to forward samples of such water.

The additions to the Zoological Society's Gardens during the past week include two Red-handed Tamarins (*Midas rufimanus*) from Demerara, presented by Mr. John Pe què; a Stanley Crane (*Tetrapteryx paradisea*) from South Africa, a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. J. Sexton; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Sir Hubert Sandford; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mr. Martin Smith; a Marsh Harrier (*Circus æruginosus*) from Malta, presented by Mr. J. Wolfe Murray; a Lesser Kestrel (*Tinnunculus cenchris*), South European, presented by Mr. William Brodrick; an Undulated Grass Parrakeet (*Melopsittacus undulatus*) from Australia, presented by the Countess of Ellesmere; two Gerbillus (*Gerbillus*, sp. inc.) from Algeria, presented by M. Alphonse Milne-Edwards; a Long-headed Snake (*Xenedon rhabdocephalus*), a d'Orbigny's Snake (*Heterodon d'Orbignyi*) from South America, presented by Dr. A. Stradling, C.M.Z.S.; a Red-throated Amazon (*Chrysotis collaria*) from South America, deposited; three Moustache Monkeys (*Cercopithecus cephus*), a Diana Monkey (*Cercopithecus diana*), a Talapoin Monkey (*Cercopithecus talapoin*), two Green Monkeys (*Cercopithecus callitrichus*), a White-collared Mangabey (*Cercocebus collaris*), a Grey-cheeked Monkey (*Cercocebus albigena*), two Water Chevrotains (*Hyomoschus aquaticus*), a Crested Guinea Fowl (*Numida cristata*) from West Africa, a Tamandua Anteater (*Tamandua tetradactyla*), a Peba Armadillo (*Tatusia peba*), a Red-billed Toucan (*Ramphastos erythrorhynchus*) from Brazil; a Hawk's-billed Turtle (*Chelone imbricata*) from the East Indies, a Puff Adder (*Viper arietans*) from Africa, purchased; a Horned Tragopan (*Ceriornis satyra*), an Impeyan Pheasant (*Lophophorus impeyanus*), bred in the Gardens.

BIOLOGICAL NOTES

Rhythmic Contraction of Voluntary Muscles.—It has been recently observed by Herr W. Biedermann (Vienna Acad. *Sitzungsberichte*) that if the sartorius muscle of a curare-poisoned frog, prepared at a low temperature, be put in a solution of 5 gr. NaCl, 2 gr. Na_2HPO_4, and 0·4 to 0·5 gr. $NaCO_3$ in 1 litre water, it shows, after a longer or shorter time of rest, *rhythmic* contractions, which continue regular a certain time for each part of the immersed muscle. Then occur periods of rhythmic contractions, separated by longer or shorter pauses, and often varying in character. These phenomena last a long time; with a con-

external temperature for days. The author saw preparations still pulsating strongly and regularly on the fourth day. (The liquid, in that case, must be often renewed.) These rhythmic contractions of a voluntary striped muscle, indicating chemical stimulus, awaken special interest, through their similarity to the long-known contractions of the apex of the heart, separated and without ganglia, in blood-serum. The heart-muscles, it is known, are also striped.

THE GORILLA AND THE CHIMPANZEE.—Mr. H. von Koppenfels, who is now engaged in explorations in the Gaboon district of Western Africa, in a letter published in the last number of the *American Naturalist*, states that he has good evidence of the existence of crosses between the male gorilla and the female chimpanzee. "This," he says, "settles all the questions about the gorilla, chimpanzee, Kooloo-Kamba, N'schigo, M'bouvé, the Sokos, Baboots, &c." Herr v. Koppenfels observes that the "French *savants* seem to have a special predilection for creating new species from variations in the form of the skull, such as often occur in this group of animals. There is but one district which forms the range of the gorilla, and this is situated in the western part of Equatorial Africa, and here it exhibits no varieties, while the chimpanzee is found all over Tropical Africa, and naturally exhibits considerable variation. The chimpanzee of Northern Guinea differs essentially from that of the southern portion of the same country, and, according to Livingstone, the 'Soko' differs from both, but is still a chimpanzee. Du Chaillu's Kooloo-Kamba, N'schigo, and M'bouvé are *not* distinct species, and this traveller, who is certainly a man of merit, but is too credulous, has been imposed upon by the mendacity of the natives, which beggars description. The names N'schigo, M'bouvé, Koola, Baboo, Soko, Quia, and Kooloo-Kamba are only different designations of the chimpanzee by different tribes. The mongrel progeny of the male gorilla and female chimpanzee discovered by me is found but in individual cases, and as such deserves no special name."

SALIVARY GLOBULES.—Prof. Stricker of Vienna, by examination of salivary globules under high-power lenses (obj. No. X. of Krafft and Seivert), has obtained the following results:—He cannot accept the supposition of a so-called Brownian (molecular) movement in salivary corpuscles. He has found the globules to consist of a complete, distinctly visible network. The granules, which have been seen under low powers of the microscope, appear on close inspection and carefully focussing to be thickened points of intersection of the threads forming the reticulum. There is a permanent fluctuation of the threads during the life of the corpuscle. By the action of concentrated salt-solutions the fluctuation ceases gradually and the reticular arrangement disappears.

FISH MORTALITY IN THE GULF OF MEXICO.—From time to time since 1844 a widespread destruction of all sorts of marine creatures has occurred along certain well-marked-out tracts in the Gulf of Mexico. In 1854 the fishes suffered all along the southern shore; in 1878 there was again an excessive mortality; in 1879 the plague again appeared; while in 1880, we learn from the recently-published report of Inspector Ingersall to Prof. S. F. Baird, it has been very intense. The poisoned waters occur in streaks or patches, sometimes near to one another, at other times many yards apart. These seem to drift with the flow of the tide, and ultimately become diluted. The most probable solution of this strange phenomenon is to suppose that eruptions of noxious volcanic gases arise through the bottom of the sea; certain it is that the marine life on the sea-bottom suffers first. Sponges, sea-anemones, mollusks, and the ground fish die in mass, and apparently at once. Upwards the deadly pestilence mounts, and the small fish swimming at or near the surface are killed by thousands, and float lifeless on the water. The large surface fish would seem to escape, and rarely is a mullet to be found destroyed. Fishing in such districts has to be abandoned, even although in the pure streaks the fish abounded, for should a smack fill its well with the results of a successful catch it had to run the gauntlet of the broad patches of the poisoned waters, and if any of these were encountered, and entered the well, a few moments would suffice to bring about the death of every fish in the cargo. The keeper of the Egmont Lighthouse writes on February 21 in this year: "As the tide came in on October 17, 1880, there were thousands of small fish floating on the water, most of them quite dead. The next day the fish were dying all along the shore; between October 25 and November 10 the stench was so horrible that it was impossible to go on the beach. Sending my family to Manatee, the assistant-keeper and myself shut ourselves up in our rooms, and kept tar, coffee, &c., burning day and night in order to stand it. The peculiar smell was like bilge-water. The fish I noticed dying acted as if crazy, darting around in every direction, then giving up and floating ashore. After a very heavy gale from the south-west the bad and good waters got mixed up, and soon all the fish caught were fat and nice." As the cause of this strange phenomenon is still problematical, some discarding the idea of the evolution of subterranean gases, believing it to be the result of a poisoning of the waters by an excess of rain-water discharged into the Gulf by the rivers, others that it is owing to the water being saturated with the tannin derived from decomposing roots and stems of palmetto, sumach, oak, &c., it would seem highly desirable that Prof. Baird should institute a series of observations as to the chemical constituents at different times of the waters of these districts.

ON THE NECTAR-SECRETING GLANDS IN SPECIES OF MELAMPYRUM.—The cow-wheats are a familiar group of plants, of which several species are to be found native. E. Ráthay, while investing the subject of the secretion of sugar by plants, was attracted by the appearance of swarms of ants evidently collecting some sweet material from the little dark puncta on the bracts of *Melampyrum arvense* (purple cow-wheat). These puncta, even under a hand-lens, are seen to be little round disk-shaped bodies, which proved to secrete a sugary secretion for which the ants came. In a memoir on this subject these gland-like disks are described and figured as they occur in *M. arvense*, *M. nemorosum*, *M. pratense*, and *M. barbatum*. These bodies have long since been observed by the systematic botanists; they form part of the trichomic development of the epidermal system of the bracts, and may be described as consisting of a short foot cell, attached to the centre of which is a circular disk. This latter is composed of a single layer of seven-sided cells. According to their function these structures in the species of Melampyrum mentioned belong to the epidermal glands of De Barry, since they secrete upon the upper side of their disk, between the cuticle and cell-membranes of the seven-sided cells, a liquid which, through the bursting of the cuticle, gets out, and is sought by the ants and eaten. The secreted fluid contains at least 2 per cent. of a kind of sugar which is not reducible by oxide of copper. The history of the development of these structures is practically the same as that of other similar formations. The purpose which they serve to the Melampyrum would seem neither to be explained by the hypothesis of Belt and Delpino as to the meaning of the extra floral nectaries, nor according to the hypothesis of Kerner concerning the same. Ráthay further adds that the moistness which almost always appears over these structures is quickly, on removal, renewed; that this moistness increases so much as to form drops when the plants are protected from the approach of ants, &c.; and that this drop-formation is repeated several times if the drops are from time to time removed (Vienna Academy *Proceedings*, vol. lxxxi. 1880).

CHEMICAL NOTES

M. RAOULT states in *Compt. rend.* that the oxides of barium, strontium, and calcium rapidly absorb carbon dioxide at a high temperature: much heat is evolved in the reaction, the temperature of the mass in the case of barium oxide being as high as 1200°, according to a pyrometric determination.

MM. CAILLETET AND HAUTEFEUILLE have determined the densities of liquid oxygen, nitrogen, and hydrogen (*Compt. rend.*) by liquefying these gases mixed with carbonic anhydride and with nitrous oxide, and basing their calculations on the assumption that the mixed liquids are without action on one another. The density of liquid oxygen at $-23°$ (pressure $= 300$ atmos.) was found to be 0·89 from experiments with carbonic anhydride, and 0·94 from experiments with nitrous oxide: at 0° the numbers obtained were 0·58 and 0·65 respectively. Liquid nitrogen at $-23°$ gave numbers corresponding with the density 0·44, while at 0° the density was 0·37. The density of liquid hydrogen was 0·033 at $-23°$, and 0·025 at 0°. Dividing the atomic weights of the three elements by the densities at $-23°$, the *atomic volume* of oxygen is found to be 17, of nitrogen 31·8, and of hydrogen 30·3.

HERR O. LÖW describes experiments with fluorspar from Wölsendorf (*Berliner Berichte*), which seem to show that the liquid contained in the cavities of this mineral consists of free

fluorine. When the mineral is broken up, a strong chlorine-like odour is perceptible; when heated with sulphur, an odour resembling that of sulphur chloride is evolved; the liquid in the mineral decomposes sodium chloride and iodide, with formation of chlorine and iodine respectively. On addition of dilute potash it yields a solution which instantly decolorises indigo solution. When the mineral is moistened with ammonia water, powdered, the liquid filtered off, neutralised with sodium carbonate and evaporated, a residue is obtained which, on addition of sulphuric acid, evolves hydrofluoric acid. Herr Löw thinks that the fluorine is produced by dissociation of cerium fluoride in the mineral.

THE application of potassium oxalate as a precipitant for many heavy metals, both in qualitative and quantitative analysis, is described by Herr von Reis in the *Berichte* of the German Chemical Society; the quantitative results obtained are very accurate.

THE reaction of bleaching powder on alcohol, which results in the formation of chloroform, is not thoroughly understood. M. Béchamp details experiments (*Annales Chim. et Phys.*), according to which no oxygen is evolved during the change, but only after the primary change is complete, and a secondary change begins when the reacting bodies have acquired a high temperature. The formation of chloroform is represented by M. Béchamp by the following equation, $2C_2H_6O + 4Ca(OCl)_2 = CaCl_2 + 2H_2O + 2Ca(OH)_2 + (HCO_2)_2Ca + 2CHCl_3$.

IN *Annali di Chimica* Signor Chiappe states that he has found spots of minium (Pb_3O_4) on various marble monuments, on parts of which bands of lead have been fastened. He supposes that by the action of the air and rain lead carbonate is produced, this is absorbed by the marble, and when exposed in places to the sun's rays it is decomposed with production of minium.

A VARIETY of coal, said to be the most highly-carbonised member of the coal series hitherto described, has been fonnd near Schunga, on the western shores of Lake Onega (*Jahrbuch für Mineralogie*); it contains about 91 per cent. carbon, 7 or 8 per cent. water, and 1 per cent. ash. This coal is extremely hard and dense, has an adamantine lustre, is a good conductor of electricity, and has a high specific heat (0·1922). Although containing as much carbon as the best graphites from Ceylon, it is not a true graphite, inasmuch as it is not oxidised by potassium chlorate and nitric acid, but behaves towards those reagents like an amorphous coal.

FEDER AND VOIT have carefully repeated the experiments of Hallervorden on the effect of feeding with ammonium carbonate (*Zeitschrift für Biologie*). The results confirm the statement of the last-named author, that in dogs ammonium carbonate is converted into urea, and also show that ammonium acetate undergoes a similar change.

IT has been asserted that the employment of sodium nitrate in manures facilitates the solution and removal from the soil of plant-foods: Herr Fiedler has recently examined this subject experimentally, and he thinks himself justified in concluding that nitrates do not dissolve out any considerable quantities of plant-foods from the soil; that, within certain limits, absorption of phosphoric acid is favoured and absorption of potash slightly impeded by sodium nitrate; and that the same salt exerts a solvent action on *dibasic* phosphates of calcium, iron, and aluminium, but not on the *tribasic* phosphates of these metals.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday last, Lieut.-Col. C. E. Stewart, of the Bengal Staff Corps, read some portions of a paper which he had prepared on the country of the Tekke Turkomans and the Tejend and Murghab Rivers. Col. Stewart, it may be remembered, is one of the officers who was accused, in a recent official despatch from St. Petersburg, of "haunting the oases" in the Turkoman country: this paper was consequently looked forward to with much interest. He left Constantinople in April of last year, and proceeded in the first instance to Ispahan, where he spent two months and a half in the Armenian quarter of Julfa, making preparations for his journey, as he had determined to travel in the disguise of an Armenian horse dealer. On September 30 he went to Ardakan, where he assumed his disguise, and travelled in a north-easterly direction along the edge of the salt desert to Meshed, afterwards crossing the mountains to Mahomedabad. The account of this part of his journey, with its numerous adventures, Col. Stewart was unfortunately obliged to omit, owing to the length of his paper. Deregez, in which Mahomedabad is situated, is in the most northern part of North-east Persia beyond the mountains, and is some sixty-five miles long and forty broad; as it projects into the Turkoman country, it is a most favourable position for collecting information respecting the neighbouring country to the Caspian on one side, and to Merv on the other. Col. Stewart made Mohamedabad his head-quarters from November 25 to January 15, and during this time moved about in Deregez, but never crossing the Persian frontier, and obtained much interesting information by diligent inquiry among the Persian officials and the Turkomans whom he met. This particularly applies to the Merv district—for he denies the existence of a town of Merv—and the Murghab River. Col. Stewart also explained very clearly the Russian line of advance, and the present and future position of the railway question. It may be interesting to add that his disguise was completely successful, and entirely deceived even the Persian servant of Mr. O'Donovan, the enterprising correspondent of the *Daily News*, who is now detained in the Merv district.

THE fiftieth and last volume of the Geographical Society's *Journal* is chiefly occupied with Mr. C. R. Markham's history of the fifty years' work of the Society, which is at once valuable and entertaining. In it will be fonnd detailed the actual circumstances attending the establishment of the Society, about which some misapprehension has hitherto prevailed. This took place in July, 1830, and the Society is therefore now fifty-one years old: after passing through many vicissitudes, which at one time threatened its very existence, it now numbers 3394 ordinary Fellows, and is the largest and wealthiest institution of the kind in the world. Mr. Markham, we may add, has been able to reproduce its first list of 460 Fellows, dated August 4, 1830. In a voluminous appendix, equal in length to the history, he furnishes complete lists of officers from the commencement, references to obituary notices of distinguished men, lists of explorers and geographers who have received medals, grants in aid of their work, &c., and of the papers and maps published by the Society. Lastly there is some interesting information respecting the Hakluyt Society. The few remaining pages of the volume contain notes on two maps of the Andaman Islands by Mr. E. H. Man and Lieut. R. C. Temple, and on the history and origin of the word "Typhoon," by Dr. F. Hirth, tables of altitudes in East Central Africa computed from 317 observations taken by Mr. Joseph Thomson during his recent East African expedition, and a narrative of a journey overland from Amoy to Hankow by Mr. E. F. Creagh. From a brief prefatory notice we learn that the issue of the *Journal* is to be discontinued, and that in future "elaborate papers of more than ordinary length and great value" will be published as supplements to the monthly *Proceedings*.

FROM the *Colonies and India's* Queensland notes we learn that Mr. Watson, in command of the Transcontinental Railway Survey, had crossed the Worna and Workingham Creeks in safety, and reported the soil magnificent and the grass splendid. The floods had however "sadly hampered the expedition," and this fully bears out the remarks made in NATURE, vol. xxiv. p. 114, as to the route for the line laid down on the Government map. It has lately been announced that General Fielding and Mr. J. Robinson, C.E., have gone out to make what is presumably an independent survey for a line, and we hope they may be able to find a more suitable route. The arrival of Mr. Watson's party at Point Parker, on the southern shore of the Gulf of Carpentaria, has since been announced by telegraph.

THE new *Bulletin* of the Bordeaux Society of Commercial Geography contains an address recently delivered before it by Capt. Gallieni, on his expedition, chiefly for surveying purposes, from the Senegal to the Niger. It is accompanied by a sketch-map of the region, on which the routes of the expedition are laid down.

PROF. ROWLAND'S NEW THEORY OF MAGNETIC ACTION

PROF. ROWLAND has lately published in the *American Journal of Mathematics* (vol. ii., No. 4; vol. iii., Nos. 1 and 2) a series of papers on "The General Equations of Electromagnetic Action with application to a New Theory of Mag-

netic Attractions, and to the Theory of the Magnetic Rotation of the Plane of Polarisation of Light." The papers, in addition to what is stated in their title, contain the mathematical consideration of that action of magnetism on electric currents which was lately discovered by Mr. Hall, and it is proved in them that, if Maxwell's theory of light be true, this action will explain the magnetic rotation of the plane of polarisation of light. These papers will no doubt be very extensively read, both on account of the interest of their contents and the great reputation of their author, and a brief discussion of them may therefore not prove uninteresting to the readers of NATURE.

We shall commence with the "New Theory of Magnetic Attractions." This theory is of the simplest kind, and obviously suggested by the mathematics of the subject. Since the magnetic induction is related to the distribution of the vector potential of magnetic induction in exactly the same way as the angular rotation of an element of fluid is to the distribution of velocity in the fluid, Prof. Rowland suggests that the magnetic field consists of a perfect fluid, whose velocity at any point is represented in magnitude and direction by the magnetic vector potential at the point, the vortex lines in this fluid are the lines of magnetic induction, and the velocity of angular rotation, is proportional to the magnitude of the magnetic force. Again, since 4π times the electric current is related to magnetic induction in the same way as magnetic induction to the vector potential, Prof. Rowland considers that an electric current consists of, as it were, vortices of vortices, or in other words, that certain irregular distributions of the vortices constitutes currents.

Maxwell has proved that the forces existing in the magnetic field could be produced by a certain distribution of stress in a medium filling the field. This stress in the simplest case consists of a tension along the lines of force equal to $\frac{H^2}{8\pi}$, along with a pressure at right angles to the lines of force equal also to $\frac{H^2}{8\pi}$, H being the intensity of the magnetic force.

Prof. Rowland goes on to show that this state of stress exists in the medium, which, according to his theory, fills the magnetic field; his proof is as follows:—"Conceive the fluid in a tube to be rotating around the axis with a certain velocity, and suppose the ends of the tube to be closed with movable pistons. Then, if the pistons are left free, there will be a centrifugal force against the sides of the tube proportional to the square of the velocity of angular rotation. If the walls are flexible and the piston immovable, then there will be a force tending to press the pistons in, and proportional also to the square of the velocity. According to our theory the magnetic force is the velocity of rotation, and so we have in the medium a tension along the lines of force and a pressure at right angles to them." Prof. Rowland does not seem to have noticed that this explanation requires the vortices to be of a finite size. It is easy to prove that in a cylindrical vortex of radius a, density ρ and angular rotation ω, the intensity of pressure on the circumference of the cylinder is greater than the mean intensity of pressure on the ends by $\frac{\rho\omega^2 a^2}{4}$; but if this is to explain the magnetic attractions the difference must be $\frac{H^2}{4\pi}$. Hence if $H = c\omega$, where c is a constant, we must have $\pi\rho a^2 = c^2$, $a = \frac{c}{\sqrt{\pi\rho}}$; we thus get a definite value for a, and the vortices must not be capable of division into bundles of smaller radius than a. Thus the fluid by which Prof. Rowland explains magnetic action cannot be the indefinitely divisible fluid treated of in theoretical hydrodynamics. It is worthy of remark that in the theory of magnetism put forth by Maxwell in the *Phil. Mag.* for 1861-62, and which agrees with the theory we are considering in explaining magnetic force by the angular rotation of a fluid, the vortices have a finite size, being done up as it were into cells, the space between the cells being filled with particles whose motion, according to Maxwell, constitutes electric currents.

Let us now go on to the explanation Prof. Rowland gives of the production of the magnetic field. He says: "Let the nature of electromotive force be such that it tends to form vortex-rings immediately round itself, not by action at a distance, but by direct action on the fluid in the immediate vicinity. The first ring will then move forward, another one will form, and so on until the whole space is filled with them, when there will be equilibrium." The consequences of this explanation, vague as it is, are somewhat startling. In the first place it is clear, from the properties of vortex motion, that every chain of particles of the fluid which possess rotation at any time must at some previous time have been in the immediate vicinity of the electromotive force; and since according to the theory there is rotation of the fluid at every point in the magnetic field, it follows that in the time taken to set up the field every particle of fluid in it has been in the immediate neighbourhood of the electromotive force. But magnetic disturbance is propagated, according to Maxwell's "Theory of Light" (which Prof. Rowland accepts), with the velocity of light; hence the streams of the fluid must be flowing with the velocity of light, and in addition every particle of fluid in the field must have rushed through the small space occupied by the seat of the electromotive force in the short time it takes to establish the magnetic field. Another difficulty which Prof. Rowland does not explain is the following: If we take a small element of electromotive force we know that to agree with the distribution of magnetic force all the vortex-rings must have the same sense of rotation; but if vortex-rings have the same sense of rotation they move through the fluid in the same direction, so that these vortex-rings when produced would all move off in the same direction, and thus leave one half of the field without rings, *i.e.*, without magnetic force. Again, the way in which these rings spread out so as to fill the field would seem to be in contradiction to the laws of vortex-motion; but as the author says he is investigating the dynamics of the subject, we may leave further comment on this point till the result of his investigation appears.

The explanation of the stress in the medium which we have referred to before is the only application of the theory worked out by Prof. Rowland. He does not explain by it any of the phenomena of induction, nor does he get from it any connection between statical and current electricity; yet he does not hesitate to speak of his theory "as one link in the chain, the first three links of which have been added by Thomson, Helmholtz, and Maxwell."

We must now leave this part of the subject and pass on to that portion of the paper which treats of the general equations of the electro-magnetic field. The mathematics of this is merely an application of the theory of the vector-potential to currents. The most important feature in the treatment of the subject is that Prof. Rowland always writes the product of the conductivity into the electromotive force instead of the intensity of the current, and claims that this is an important advance; but if there is any difference either Ohm's law must not be true, or Prof. Rowland must mean by electromotive force something different from that meant by ordinary users of the term. Prof. Rowland asserts that in an unlimited medium the action is not between magnets and currents, but between magnets and electromotive forces; he bases this assertion on the theorem that in an unlimited medium unclosed electric currents have no magnetic action. It is hard to see how this proposition can be true, for the current through any area is measured by the line integral of the magnetic force round the boundary of the area; but if the magnetic force is everywhere zero, then the line integral of it round any curve must vanish, and thus the current at any point must vanish. The proposition is based on reasoning of the following kind: the force between an electric point (by an electric point he means a point from which electricity is streaming, in fact what is usually called a source) and a magnetic pole must by symmetry be along the line joining them. But a magnetic pole of any size is always accompanied by one of the opposite sign, and the two form a vector quantity; and we think from the relation that one pole necessarily bears to another, it is not safe to reason about it as if it were a purely scalar quantity. Prof. Rowland himself acknowledges what is equivalent to this, for after saying that the force due to the unclosed currents on each pole of the magnet is zero, yet he says there is probably a force on the magnet as a whole tending to place it across the currents.

Although we think that the reasoning given for the assertion that the action is not between magnets and currents, but between magnets and electromotive forces, is unsatisfactory; yet we think that, understood in a certain sense, the proposition is mathematically true. For we can prove directly from the ordinary expressions for the magnetic action of currents, that if we have a source and a sink of equal intensities ($4\pi m$) placed close together, the magnetic action of the currents produced is

the same as that due to a current of strength *m* flowing along the short line joining the source to the sink. Now the current at any point produced by a source and sink placed close together at a distance *ds*, is exactly the same as the magnetic force at the same point produced by a magnet joining the source and sink, whose moment is *m ds*, and direction of magnetisation along the line joining the source and sink. Hence if we have any system of currents in the field, and find by the application of the methods given by Sir W. Thomson in his paper on "Inverse Problems" the distribution of magnetism which would produce a magnetic field such that the magnetic force at any point was equal in magnitude and direction to the current at the point, the magnetic action of the system of currents will by the proposition just stated be the same as that due to currents whose intensity and direction coincide with the intensity and direction of the magnetisation producing the said magnetic field. Thus instead of currents occupying the whole of the medium, we have only to consider currents occupying a limited portion of it. This is, we think, all that can be fairly stated about this point, and it will be seen that, to say the least, Prof. Rowland's statement that "the action in such a medium reduces itself to an action between magnets and electromotive forces instead of between magnets and currents," is not a clear way of putting it. Prof. Rowland in this part of the subject introduces a new term, viz., magneto-motive force; this is a force supposed to exist between two magnetic poles so as to cause the same number of lines of induction to pass between the points as to flow out of either of them; it is proportional to the magnetisation, and seems only introduced for the sake of making more evident the fact that currents are related to electromotive forces like lines of induction to magnetisation, or with the new terminology to magneto-motive forces. This was pointed out by Maxwell in his paper on "Faraday's Lines of Force" published in the Cambridge *Transactions* for 1856.

The last part of the paper, which is also the most interesting, contains the explanation, by means of the new action discovered by Mr. Hall, of the magnetic rotation of the plane of polarisation of light. By adding to the old expression of the electromotive force a term representing the force discovered by Mr. Hall, Prof. Rowland obtains an expression for the rotation of the plane of polarisation of exactly the same form as the one given by Maxwell in § 829 of the "Electricity and Magnetism."

J. J. Thomson

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Cambridge.—The report of the Botanic Garden Syndicate states that during the past year valuable additions have been made to the collections of ferns and orchids, and many choice stove and greenhouse plants have been received. The collection of hardy, herbaceous, and alpine plants has been much increased, and the rockery furnished with many rare alpine species. The genera Iris, Narcissus, and Helleborus have received special attention. During the year, 1594 labels have been written in large letters. The curator, Mr. Lynch, has extended the correspondence of the Gardens with botanic gardens, nurserymen, and private cultivators: 2600 plants have been received, and 1285 packets of seeds.

In consequence of the decision of the Duke of Devonshire in favour of the legality of the recent vote of the Senate admitting women to the Previous and the Tripos Examinations, the first lists in which the names of women who have passed the Previous and any Tripos Examination, have appeared in the University *Reporter*. In the Natural Sciences Tripos, Part 1, Class 2, is the name of Miss Anelay of Girton. In the Previous Examination twelve Girton students and two Newnham students have passed in one or more parts of the examination.

Local Lectures.—In spite of the removal of several important districts from the scope of these lectures by the establishment of local colleges, the numbers attending lectures during the past winter have been 4369 as against 5009 in the preceding winter; and the reduction in numbers is due to the absence of the South Wales centre from the lists, the Syndicate having been unable to make adequate arrangements for this district, owing to their engagements elsewhere. South Wales is again to be vigorously worked in the coming session. Dr. R. D. Roberts of Clare College has been appointed Assistant Secretary for the purposes of the local lectures. The courses of lectures on physical science subjects in the past winter have included Mr. Teall's on Early Man in Western Europe, and the Origin of Rocks and Scenery of the British Isles at Nottingham and Derby, Mr. J. E. Marr's on Geology at Carlisle and Penrith, Mr. E. Carpenter's on the Science and History of Music at Nottingham, and on Light at Chesterfield, Mr. Carr Robinson on Gases and on Chemistry at Hull, and by Mr. H. N. Read on Botany at the Crystal Palace.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, June.—The flight of birds and the mechanical principles involved, by A. C. Campbell.—Recent advances in photography, negative and positive, by J. Carbutt.

Journal de Physique, June.—On registering apparatus for atmospheric electricity and terrestrial magnetism, by M. Mascart.—On radiophony (third memoir), by M. Mercadier.—On the contraction of galvanic deposits and its relation to Peltier's phenomenon, by M. Bouty.—Projection of the Lissajous figures with differences of phase variable at will, by M. Crova.—Production of electric currents in any system of fixed conducting wires, by M. Brillouin.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xiv. fasc. viii.-ix.—On the question whether American vines may be imported from phylloxerised or suspected districts without risk, by Count Trevisan.—Difference of longitude between the observatories of Genoa, Milan, Naples, and Padua, by Prof. Celoria.—On the stocking of Italian lakes with fishes, by Prof. Pavesi and Dr. Sulzer.—Toradelphia of a scorpion, by Prof. Pavesi.—Monstrosity of a fresh-water Crustacean (*Astacus fluviatilis*), by Prof. Maggi.—Cremation and legal medicine, by Dr. Biffi.

Rivista Scientifico-Industriale, No. 9, May 15.—Two new applications of the electric light, by Prof. Ferrini.—Mercury air-pump, by S. Serravalle.—New method of qualitative chemical analysis, by L. Mauri.

Atti della R. Accademia dei Lincei, vol. v. fasc. 12.—Description of a terrestrial *trombe* which occurred in 1456, by S. Blaserna.

SOCIETIES AND ACADEMIES

London

Royal Society, June 16.—"On the Reversal of the Lines of Metallic Vapours. No. VIII. (Iron, Titanium, Chromium, and Aluminium.)" By Professors Liveing and Dewar.

In their last communication on this subject the authors observed that iron introduced as metal or as chloride into the electric arc in a lime crucible in the way which had proved successful in the case of many other metals, gave no reversals. They succeeded however in reversing some ten of the brightest lines of iron, mostly in the blue and violet, by passing an iron wire through one of the carbons, so as to keep up a constant supply of iron in the arc. Considering the great number of iron lines, and that so many of them are strongly represented amongst the Fraunhofer lines, it seemed somewhat surprising that it should be difficult to obtain a reversing layer of iron vapour in the arc inclosed in an intensely heated crucible. A like remark might be made respecting titanium, which is almost as well represented as iron in the Fraunhofer lines, but has heretofore given no reversals. Almost the same might be said of chromium, except that the number of chromium lines is so much less than than that of either of the other two metals.

They have since found that most, if not all, of the strong lines of these three metals may be reversed by proper management of the atmosphere and supply of metal in the crucible. Indeed with regard to iron the method employed with other metals was successful so far as the ultra-violet rays were concerned, though it failed for less refrangible rays. When iron has been put into the crucible through which the arc of a Siemens' dynamo-electric machine is passing, and then fragments of magnesium dropped in from time to time, most of the strong ultra violet lines of iron are reversed. The magnesium seems to supply a highly reducing atmosphere, and to some extent carry with it the iron vapour. It also produces a good deal of continuous spectrum, at least in certain regions, and against this the iron lines are often depicted on the photographic plates sharply reversed. In this way the authors have observed the reversal of the strong

iron lines about the solar lines L and M, four strong lines below N, the line O, all the strong lines from S_2 to U inclusive, and two strong groups still more refrangible.

Potassium ferrocyanide introduced into the arc instead of magnesium gives a reversal of the same lines as are mentioned in the foregoing paragraph.

Iron wire fed in through a perforated pole gives reversals of the highest group (wave-length 2492 to 2480), but with the lines so much expanded as to form broad absorption-bands instead of lines.

With a vertical arrangement of the carbons and a stout iron wire in the axis of the lower (positive) carbon, many more lines in the visible part of the spectrum are seen expanded and reversed. This effect is sometimes enhanced by leading into the crucible through the upper carbons, which is perforated for the purpose, a very gentle stream of hydrogen gas; the stream must be no more than is just sufficient to give a tiny flame at the the mouth of the crucible; a stronger stream diminishes the amount of metallic vapour, probably by its cooling action, and lessens the effect. By this treatment some of the strongest lines of iron remain reversed for some time, the weaker lines are seen to expand and be reversed for a few seconds at a time, when, from a change in the intensity of the current, or some other reason, a larger amount of metal is volatilised and shows itself by burning in brilliant scintillations at the mouth of the crucible.

A list of the iron lines reversed, 136 in number, designated by their approximate wave-lengths, is given in the paper.

When the perforation of the lower carbon is filled with titanium cyanide instead of the iron wire the titanium lines come out very brilliantly and steadily, and many of them, especially in the green and blue parts of the spectrum, are expanded and reversed. A list of twenty-nine lines observed to be reversed is given in the paper.

In the case of chromium, introduced into the crucible either as oxide or as bichromate of ammonia, there were no reversals until a gentle current of hydrogen or of coal gas was led in through the perforated carbon. This brought out the triplet in the green, wave-lengths 5207, 5205, 5203, sharply and steadily reversed, and likewise the three strong lines in the indigo, wave-lengths 4289, 4274, 4253; also a triplet near N at wave-lengths about 3578, 3593, 3606, apparently coincident with strong lines in Cornu's map of that part of the solar spectrum, and a rather strong double line just below O at about wave-length 3446. The reversal of another chromium line at about wave-length 3217 is doubtful. A triplet at wave-lengths 2799·8, 2797, 2794, is more easily reversed than any other of the chromium lines. This triplet is generally strongly developed whenever a compound of chromium is introduced into the crucible, so that the authors conclude that it is due to that metal, but it is sometimes visible in the photographs when other chromium lines are not seen. A still more refrangible chromium line, wave-length about 2779·6, is also frequently reversed by a gentle current of hydrogen.

The two aluminium lines near S are frequently reversed when a fragment of the metal is dropped into the crucible, the less refrangible line, wave-length 3091·5, being more strongly reversed, and continuing reversed for a longer time than that at wave-length 3080·5.

Chemical Society, July 16.—Prof. Roscoe, president, in the chair.—The following papers were read:—On the isomeric acids obtained from coumarin and the ethers of salicylic aldehyde, by W. H. Perkin. The author has studied the action of various agents on these bodies. The α body (from coumarin) is converted into the β body by heat or light. In general the effect of chemical action on the α acid is to convert it into the same compound as that yielded by the β body. Bromine forms an exception, and two isomeric dibromides were obtained. The author concludes that as the α body has a lower boiling-point, density, refractive index, and is less stable than the β acid, it is probable that its molecules are farther apart and that the difference of distance is probably between the radical and the hydroxyl. The derivatives from propionic and butyric coumarin were studied.—Notes on naphthalene derivative, by H. E. Armstrong and G. Lowe. The authors have continued their investigations as to the action of sulphuric acid on naphthalene, and confirm their previous statement that three and not two disulphonic acids may be obtained. An isomeric β naphtholsulphonic acid was prepared by dissolving β naphthol in cold concentrated sulphuric acid.—On the synthesis of ammonia, by G. S. Johnson. The author reasserts that pure nitrogen, free from nitric oxide, when passed with hydrogen over spongy platinum, forms ammonia. If however the nitrogen be previously passed through red hot asbestos no ammonia is formed. This indicates the existence of an active allotropic nitrogen analogous to ozone.—On the alkaloids of nux vomica, by W. A. Shenstone. The author has prepared pure brucine, but concludes that the so-called Igasurin has no existence.—Notes on photographs of the ultra-violet emission spectra of certain elements, by W. N. Hartley.—On the sulphates of aluminium, by S. U. Pickering.—On two new oxides of bismuth, by M. M. P. Muir, Bi_4O_7 and Bi_4O_7, prepared by the action of aqueous potassium cyanide on a hot nitric acid solution of bismuth nitrate.

Royal Microscopical Society, June 8.—The president, Prof. P. Martin Duncan, F.R.S., in the chair.—Eleven new Fellows were elected and proposed.—Prof. Paul Reinsch attended the meeting and exhibited specimens of the vegetable forms found by him in the Coal measures.—The president read a paper on some remarkable enlargements of the axial canal in sponge spicula and their causes, accompanied by drawings on the blackboard. Nearly all the spicula obtained from specimens of very deep soundings off Japan were found to have the normal axial canal enlarged in a moniliform or conoidal manner, producing very elegant results. The spicula were of seven or eight kinds, and were mature. The enlargement was found to be invariably accompanied by an open condition of the axial canal or by penetrations, cylindrical in outline, from without, down to the canal. The penetrations were shown to be connected with an organic body resembling the zoospores of an *Achlya*, and granules, organic in nature, were observed within the enlarged canals. Thinning and solution of the spicula, the result of these organisms, were considered, and admitting the influence of great pressure, the president stated that he had never seen anything which led him to believe that there was free carbonic acid gas in the ocean.—A note was read by Dr. Savage calling attention to the changes which took place in nervous tissues in the process of hardening.—Mr. Holmes read a paper on a new British Alga, specimens of which were exhibited.—Discussions also took place on the value of swinging sub-stages on the motion of diatoms.—Dr. Maddox exhibited some micro-photographs of diatoms, and Mr. Powell demonstrated the aperture of his $\frac{1}{8}$-inch oil-immersion objective = 1·47 num. ap., the largest hitherto made.

Physical Society, June 25.—Prof. Fuller in the chair.—Señor Olympio de Barcelos was elected a member.—Mr. Grant exhibited an apparatus for showing the position and direction of the curve of zero electro-dynamic induction. It consisted of two coils of insulated wire mounted on stands, one being fixed while the other was free to revolve round it at a fixed distance.—Prof. W. E. Ayrton explained the determination of the refractive index of ebonite made by himself and Prof. Perry. The result for oxy-hydrogen light was 1·7, but at the suggestion of Prof. Fitzgerald of Dublin this was checked by measuring the polarising angle of ebonite by reflected light. Sunlight was employed in these experiments, and different pieces of ebonite. The result was 1·611. Professors Ayrton and Perry had repeated their former experiments, using the electric light and a battery of 70 volts E.M.F. The result confirmed the one first obtained. They had also determined the index of refraction in the ordinary way from the red rays, which they observed to pass through the prism of ebonite. Result for the least refrangible rays 1·66. Mr. Boys remarked that one could see better through thin ebonite if it was varnished or wetted than when untreated.—A letter was read by the chairman from a sub-committee of the British Association inviting the members of the Society to send exhibits to the jubilee meeting of the British Association at York.—Dr. James Moser read a paper on the microphonic action of selenium cells, in which he argued that the action of the selenium cell in the photophone was that of a microphonic contact or bad joint between the metal electrodes or metal plates of the cell and the selenium. The heat rays of the photophonic beam caused the joint to expand and contract; hence the variation in the current passing through the receiving telephone. Dr. Moser also exhibited a piece of selenium which increased, not diminished, in electric resistance when light fell upon it. He further showed a standard Daniell cell of the gravity type, which consisted of a glass vessel containing the copper plate at the bottom immersed in sulphate of copper solution, and the zinc plate at the top immersed in sulphate of zinc solution, and a clear line of demarcation between these solutions was produced by suspending an independent piece of zinc midway between the plates, so as to decompose all the sulphate of copper which diffused upward to that point.

Prof. Mcleod said that he had produced the same result by surrounding the zinc plate with a cage of copper wire connected to the copper plate. Copper deposited on the cage and the cell was in constant use. Dr. Lodge said that arrangement would not however serve as a standard of electromotive force, because all the copper plate should be in the copper solution. In his cell the copper and solution are both in a test tube immersed in the zinc solution, and diffusion has to take place up this test-tube and down the cell so as to enter a second tube, open at the bottom, in which the zinc is placed.—Dr. Guthrie showed a new experiment to the effect that when a magnet is suspended over a disk of copper and the disk is rotated the magnet is repelled upwards. The experiment was shown by suspending a horse-shoe magnet from one end of a scale beam, counterweighted. As a possible explanation he suggested that the vertically-resolved force of the induction-current before the magnet might be greater than that behind the magnet.—The Secretary read a paper by Prof. Balfour Stewart and Mr. W. Strode, on results obtained by a modification of Bunsen's calorimeter described to the Society in January last year. With a new instrument made by Casella they have determined the mean specific heat of iron to be 0·1118, and that of sulphur 0·1756, the true values being given as 0·1138 and 0·1776. The advantage of the method is its simplicity, and the fact that very small quantities of the substance may be used.—Dr. Lodge then explained experiments by Mr. Sutherland, showing that a Daniell cell keeps its E.M.F. very constant when heated, because the thermo-electric effect at the junction of the zinc with the solution is balanced by that at the junction of the copper with the solution. After remarks by Dr. Moser and Prof. Perry, the Society separated until November next.

Paris

Academy of Sciences, June 20.—M. Wurtz in the chair.—The following papers were read:—Observations on the simultaneous reduction of two bilinear forms, by M. Jordan.—On the preparation of aldol, by M. Wurtz.—Fresh discovery of native sulphur in the soil of Paris, by M. Daubrée. This occurred during the laying of drains in the rue Meslay. The case seems very similar to that previously recorded.—On a new thermograph, by M. Mercadier. The instrument consists of a cylindrical brass reservoir prolonged into a capillary tube of red copper, which opens into a Bourdon tube. The whole is filled with oil, and closed. The dilatation or contraction of the oil with varying temperature affects the curvature of the Bourdon tube, and thereby a recording lever. Two such instruments may be used simultaneously to give the curves for a deep and a peripheric part of the body. It is proved that in vaso-motor disorders the animal temperature undergoes variations in opposite directions in the central and peripheric parts. Inanition cools both the centre and periphery, while certain maladies seem to increase the production of heat, for they heat both parts.—On M. Roudaire's project of the interior sea; reply to M. Cosson, by M. de Lesseps.—On osseous grafts, by M. Ollier. He calls attention to Mr. MacEwen's success (in Glasgow) in reconstituting a portion of the humeral diaphysis by means of six cuneiform bony fragments taken from the tibias of young children having rachitic incurvations. The osseous tissue was transplanted complete. The antiseptic method was employed. (A note by Mr. MacEwen describes his mode of procedure).—Microscopic phenomena of muscular contraction; transversal striation of smooth fibres, by M. Rouget. It is demonstrated that this striation (which occurs only in the state of contraction) is due to the fibre when it contracts getting folded on itself, and then presenting alternate projections and depressions. The fibre-cells in polarised light are uniformly bi-refringent in the smooth state, but in the state of contraction they show in the dark field an alternation of bright and dark bands. It is shown from smooth fibres of the adductor-muscle of the valves in a cephalous molluscs killed by heat, that a fibre which has lost all contractility may still acquire all the peculiarities of structure and optical characters of striated fibres, if any cause produce in it fine and regular folds.—On the thermal laws of the excitative spark of condensers, by M. Villari. The heat developed by this spark (which is that produced against the exciter) is proportional to the quantity of electricity multiplied by the electric thickness, or it is proportional to the quantity of electricity for the fall of potential.—On the heat of formation of oxychloride of calcium, by M. André.—Action of protoxide of lead on alkaline iodides, by M. Ditte.—On the basic carbonates of lime, by M. Raoult. The property of hardening in contact with water is observed in all basic carbonates obtained by heating any lime, pure or not, in carbonic acid, and it is this that chiefly characterises that class of compounds.—Influence of concentration of hydrochloric acid on the dissolution of chloride of silver, by MM. Ruyssen and Varenne. The decrease of solubility as the acid is diluted is rapid and regular. The insolubility seems approximately to be tripled as the titre of the acid is halved.—Action of arsenic and phosphoric acids on tungstates of soda, by M. Lefort.—Researches on tertiary monamines; action of heat on bromide of allyltriethylammonium, by M. Reboul.—On the microzymas of chalk; reply to MM. Chamberland and Roux, by M. Béchamp.—Studies on the coal-formation of Commentry; its formation attributed to transport in a deep lake, by M. Fayol. He here criticises adversely the theory of primitive horizontality of the deposits with general subsidences of the ground. The natural explanation is transport without subsidence. Important industrial interests depend on arriving at an exact theory of formation of coal strata.—M. Daubrée presented the first volume of *Annals* of the School of Mines of Ouro-Preto, sent by the Emperor of Brazil in name of M. Gorceix. This describes some of the mineral riches of Brazil.—M. Tabourin communicated a project for the electric light: he would place in the pedestal supporting the carbons a small magneto-electric machine driven by the force of water in pipes, or by compressed air, or by descent of a weight.

Vienna

Imperial Academy of Sciences, June 17.—L. T. Fitzinger in the chair.—T. Exner, examinations into the localisation in the cortex cerebri of man.—A. Rollett, on the action of salts and sugar on the red-blood corpuscles.—L. Boltzmann, contributions to the theory of viscosity of gases.—On some theorems relating to heat-equilibrium, by the same.—Ign. Klemencic, on the deadening vibrations of solid bodies in liquids.—Dr. K. Friesach, on the transits of Mercury and Venus in 1881 and 1882.—G. Haberlandt, on the collateral vessels in the leaves of ferns.—T. Herzig, contributions to the knowledge of trigenic acid.—A note on cyanuric biuret, by the same.—H. Fürth, on berberonic acid and the products of its decomposition.—G. Goldschmidt, on some new aromatic hydrocarbons.—C. Senhofer, on the direct action of carboxyl groups on phenols and aromatic acids.—C. Senhofer and F. Salay, on the action of hydroquinone on potassium dicarbonate.—C. Brunner, on the action of tolu-hydroquinone on potassium dicarbonate.—T. Zehenter, on some derivates of α-dioxybenzoic acid.—D. T. Woldrich, second report on the diluvial fauna of Zuzlawitz near Winterberg (Bohemia).—T. Pernter, on the daily and yearly course of atmospheric pressure on mountain-summits and in Alpine valleys.

CONTENTS

Page

THURSDAY, JULY 7, 1881

BURMEISTER'S "MAMMALS OF THE ARGENTINE REPUBLIC"

Description Physique de la République Argentine d'après des observations personnelles et étrangères. Par Le Dr. H. Burmeister. Traduite de l'Allemand avec le concours de E. Daireaux. Tome III. Animaux Vertébrés: première partie, Mammifères, vivants et éteints. Royal 8vo. (Buenos Ayres: P. E. Coni, 1879.)

THE veteran naturalist, Dr. Burmeister, has devoted the third volume of his great work on the Argentine Republic to an account of the Mammals, recent and fossil, of his adopted country. As regards the latter series it is well known how long and how laboriously the author has worked on the extinct Mammalian Fauna of Buenos Ayres, and what excellent results have followed on his investigations. The summaries of his various memoirs on this subject contained in the present volume will therefore be much appreciated by those who may not have leisure or opportunity to refer to the originals, and will be especially useful in bringing the chief results already arrived at in a convenient shape before future explorers. Although so much has already been done in this direction, there are few countries in the world that still offer such a promising field for the palæontologist as the pampas of the Argentine Republic, where in certain spots bones of *Megatherium*, *Glyptodon*, *Macrauchenia*, and other extinct monsters seem almost to strew the upturned soil!

As concerns the recent Mammals of Buenos Ayres, Dr. Burmeister has also given us a very useful work, the only previous available authority on the subject being the summary of the Mammal-Fauna contained in the same author's second volume of his well-known "Travels" in the Argentine Republic, which is neither so full nor so complete. But we fear there is also still much to be done before this branch of the subject can be deemed to be satisfactorily known, and that Dr. Burmeister has not in every case made himself acquainted with the most recent investigations published upon several points.

In the first place, as regards the general arrangement of the Mammalian series, Dr. Burmeister will forgive us if we point out that he is a little behind the age. The Cuvierian division of the class into "Unguiculata," "Ungulata," and "Pinnata" was no doubt most in vogue thirty years ago, but we cannot agree with our author that it is still adhered to by "la plupart des zoologistes modernes." It is certainly strange to zoologists of the present day to find, in a work dated 1879, the Marsupials located in the centre of the placental series, and the Seals divorced from the other Carnivores. Again, on referring to the accounts of the more obscure groups of Bats and Rodents, we find a disposition to quote from Rengger and the "Voyage of the *Beagle*" instead of giving particulars obtained from modern specimens. Surely the Museo Publico of Buenos Ayres must have a well-arranged and properly-determined series of the native Mammals, whence particulars respecting their ranges and variations might have been taken.

The recent Mammals of the Argentine Republic, according to Dr. Burmeister's enumeration, are about 112 in number. Of the Quadrumana only four species intrude into the northern provinces, where alone forests are met with, the American monkeys being exclusively arboreal in habits. Of Chiroptera Dr. Burmeister allows twenty species, but there are doubtless more to be discovered when the fauna is worked out. The Feræ are twenty-one in number, embracing the jaguar and puma, both of which extend all over the Republic, and five other smaller species of *Felis*, besides seven different dogs of peculiar types. Five opossums of varying sizes constitute the Marsupial fauna of the Argentine Republic, and are succeeded in Dr. Burmeister's classification by twenty-seven Rodents—here, as is usual, except in Australia, the most numerously represented order of Mammals. Amongst them are the two most characteristic animals of the Argentine pampas—the Patagonian cavy (*Dolichotis patachonica*) and the Vizcacha (*Lagostomus trichodactylus*), which are spread over the whole Republic. The recent Edentata are represented by seven armadillos and two ant-eaters—a feeble remnant of the huge monsters of the same group that once existed in the country. Amongst these the most remarkable is the Pichy-ciego (*Chlamydophorus truncatus*), found in sandy dunes of San Juan and Mendoza, of which, and its singular habits, Mr. E. W. White has lately given us a most interesting account.[1] This diminutive burrowing armadillo is indeed one of the marvels of the class of Mammals. Such are its fodient powers, says Mr. White, "that a man has scarcely time to dismount from his horse before the creature has buried itself to the depth of its own body."

In Ungulates, like the rest of the neotropical regions, the Argentine territory is poor. Dr. Burmeister enumerates only ten, of which one-half are deer of the American type *Cariacus*. Besides these there are only two lamas, two peccaries, and the ordinary tapir of the lowlands (*Tapirus suillus*), which occurs in Tucuman and Corrientes, and concludes the terrestrial Mammal-fauna. Amongst the marine Mammals or "Pinnata," with which Dr. Burmeister, following Cuvier, concludes his list, are included two Seals and fourteen Cetaceans. One of the former (*Otaria jubata*) is well known in this country from the living examples in the Zoological Gardens. The latter have as yet been but imperfectly studied, and several of the species mentioned appear to be rather doubtful.

An atlas, intended to accompany this volume of Dr. Burmeister's important work, is announced to appear in *livraisons* at a later date.

THE ARABIAN DESERT

Gleanings from the Desert of Arabia. By the late Major R. D. Upton. (London: C. Kegan Paul and Co., 1881.)

THE author of this volume was an enthusiastic admirer of the Arabian horse, and seems to have visited the Arab tribes in the neighbourhood of Aleppo and Damascus with the single purpose of seeing and purchasing high-bred animals and acquiring information about the breed. The narrative part of the book is not furnished with dates, but from incidental remarks it appears that Major Upton was at Aleppo in 1875 and at Damascus in 1878. On the former occasion he journeyed

[1] *Proc. Zool. Soc.* 1880, p. 8, "Notes on *Chlamyphorus truncatus*," by E. W. White, F.Z.S.

eastward as far as the Euphrates, but does not seem to have descended below Balis, or to have ever been more than two days' journey from Aleppo. On this tour the traveller was mainly among half-settled tribes, and at the season of his visit the great hordes of pure nomads who sometimes pasture their flocks in the district had withdrawn to the south. Of the author's excursion from Damascus he gives no topographical detail. It was simply a visit to an encampment of the Eastern Anazeh.

From the limited range of these journeys, and from the fact that the writer made no exact observations except on his favourite subject, it will readily be understood that the book has little merit as a record of travel. Except in the matter of horseflesh, Major Upton merely describes in the loose manner of the amateur traveller whatever happened to amuse or strike him as he moved from place to place. He is neither an archæologist nor a naturalist; and though there is no doubt a great deal still to be learned about the less public customs even of the Arabs of the Syrian desert, who have been often visited by good observers, it is not surprising that Major Upton adds nothing on this head to what has been given to us by Burckhardt, and more recently by Lady Anne Blunt and her husband. Some facts may perhaps be gleaned from the remarks upon individual tribes and families, but even here the book is inferior to Lady Anne Blunt's "Bedouin Tribes," while the proper names are printed in such an inaccurate transcription that they must be used with caution.

Of the three parts into which the volume is divided the first and a small part of the second are personal narrative. The main bulk of the second part should have been altogether cancelled, for it consists, not of gleanings from the desert, but of gleanings from Pococke's "Specimen" and the extracts from Abulfeda printed at the close of White's edition of that work. The author, whose credulity is displayed in the earlier pages of the volume by an excursus on the Great Pyramid, based on conversations with a missionary of the Pyramid religion whom he met going out to preach to the heathen, accepts the whole mythical history of Arabia as sober fact, and as he has a theory that it throws great light on the parallel history of the Arabian horse, we are treated to some eighty pages of abridgment and excerpts from Pococke (generally without acknowledgment of the source). Unhappily, Major Upton's knowledge of Latin is that of a backward schoolboy. He frequently misses the meaning of his author, and, to make matters worse, the book has been so carelessly revised for press by the friend who undertook to superintend the posthumous publication that *inquit Jalalo'ddinus* becomes *inquib Jalabô daiuus*, El Makin, El Maka, and so forth, while the words *lingua Arabica qua coelitus descendisse Alcoranus dicitur*, with the marginal note *in Alcor. Sur. XVI.* become "the Arabic language in which the heaven-born Alkoran is said to be descended in Alk." The author is not stronger in Arabic than in Latin, as may be judged from the fact that he derives Hijâz from Hajar, *a stone*, and Kheil, the generic name of the horse, from the pigment Kohl. No reliance therefore can be placed on the transcription of Arabic words, and here again errors of the press have conspired to produce results truly appalling. Of the names of the seven mares of the prophet, for example, not one is quite correct, and the errors include such monstrosities as Sekh for Sekb, Sizez for Lizâz, Haif for Lakhîq. In brief, everything that our author derives from books, and all the historical and geographical speculations which he is so fond of, are absolutely worthless. This blemish affects even the third part of the book, where Major Upton deals with his proper subject, the horse. For his actual observations on the strains of pure Arabian blood are hopelessly entangled with fabulous legend and baseless theories. It is to be observed, moreover, that he admits that his own reading of the information derived from the Anazeh did not always accord with the views of the friend who accompanied him on his journey, an inhabitant of the verge of the desert, and long familiar with the Bedaween. Yet it is clear that Major Upton's knowledge of Arabic was by no means sufficient to enable him to take up an independent position in such matters. Like most men with a hobby, he had a theory to which facts must bend. But what a theory! Nothing less than a mythical history of the Arabian horse, the purest strains of which he traces back first to the time of David, when "the horses of his ancestors were entailed on" Rabî'atu-'l-faras, and then to Salâmân, the fourth in descent from Ishmael. That all authentic notices of the horse in Arabia point to a comparatively late introduction of that quadruped is of course indifferent to our author, who presumably had never heard of the researches of Hehn, Guidi, and others in this field.

Probably no European except Mr. Blunt can speak with real authority on the complicated subject of Arabian horse-breeding. Major Upton however takes no notice of what Mr. Blunt has written so well and fully on the topic, and on points where the two accounts diverge the uninitiated will hardly fail to prefer the clear and lucid statements of one who saw far more of the desert and is not biassed by theory. Lovers of the horse will however peruse with interest Major Upton's notes on the characteristic features of the Arabian breed illustrated by descriptions of individual animals.

W. ROBERTSON SMITH

OUR BOOK SHELF

Easy Lessons in Botany, according to the Requirements of the Revised Code, 1880. By the Author of "Plant Life." (London: Marshall Japp and Co., 1881.)

NEITHER better nor worse than the innumerable other little books of the same kind. The morphological part consists of the usual enumeration of descriptive terms, with coarsely-executed diagrams. The histology and physiology are very weak. The cell-nucleus is defined (p. 27) to be "a portion of the protoplasm denser than the rest," which may or may not be the case, but we are further informed, which is a more doubtful statement, that "it is this part of the protoplasm which grows." The following is at any rate a dogmatic way of stating the facts:—"By the addition of nitrogen and sulphur (taken up in water by the roots) to the constituent parts of starch, protoplasm has the power of forming *albumenoids*" (*sic*). If this is in accordance with the requirements of the Revised Code it only shows what tyranny in science is compatible with free institutions. On p. 32 we learn that "carbonic acid gas . . . finds its way . . . into the *spiral* vessels, which convey it to the cells of the *fibro-vascular* bundles." Very good; the Revised Code ought to know. But surely as a matter of argument there is a screw loose about the following sentence:—"As the store of albumen

is undivided the grain of wheat is said to be *Monocotyledonous*" (p. 42). Not even the solemn name of the Revised Code can enable us to digest this without distress.

Plant-Life. Popular Papers on the Phenomena of Botany. (London: Marshall Japp and Co., 1881.)

THIS is a most attractive-looking book by the same author as the dismal little tractate just noticed. It might have been hoped that it would have made clear some of its dark sayings. But they all seem to be *ipsissimis verbis*, sugared over with copious extracts from all sorts of people, from Thoreau and Kingsley to Mr. Worthington Smith, Dr. Masters and Mr. Darwin. On p. 30 we have "The carbon absorbed from the air is combined with the cell-sap and forms a substance called starch," which is even harder doctrine than anything in the "Easy Lessons." Much is said about *Equisetaceæ* and the hygroscopic movements of the elaters of their spores. An unfortunate microscopist is quoted from *Science Gossip* of such a remote date as 1878, who is of opinion that "the ultimate cause of this movement is quite unknown . . . . most probably it takes place by the contraction and expansion of the cells of which the elaters are composed." Of course it is well known that the spores are unicellular and the elaters are simply strips of the spirally torn outer cell-wall. The book, with all its blundering accounts of *Englena* (sic), *Claydonia* (sic), the "lovely *Closterium*" which "consists of two cells," and the like, may stimulate the curiosity of those who know nothing of plants to know more and better. It is at any rate interesting to find that Prof. Schwendener's lichen-theory has found its way to popular books, even though it is introduced with the remark that "concerning" gonidia "a humorous theory was promulgated a few years ago, but met with the ridicule it deserved." The book has 148 illustrations drawn by the author, which scarcely do justice to the "specially prepared rolled paper" provided for them.

The London Catalogue of British Mosses and Hepatics. Published under the direction of the Botanical Record Club. Second Edition. (London: David Bogue, 1881.)

THIS is a handy list on the well-known model of that formerly issued by Mr. Hewett Cottrell Watson for British flowering plants. It gives the distribution through the eighteen provinces into which Mr. Watson divided Great Britain for the purpose of ascertaining the range of British plants.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Dr. W. B. Carpenter and Mr. W. I. Bishop

I AM sorry to find that Dr. Carpenter is "greatly surprised" at my allusion to the effect which has been produced by the circulation of his letter to Mr. Bishop, for in making that allusion I was under the impression that this letter had been put to a use other than that which Dr. Carpenter could have either intended or desired. If, as it now appears, I was wrong in entertaining this impression, it is needless to say that I am willing to apologise for having so far given it public expression; and in this case I can only infer that my error arose from an unfortunate difference in the estimate which we have respectively formed touching the scientific importance of the phenomena which Mr. Bishop has displayed. Such physiological and psychological interest as these phenomena present appeared to me to call for investigation in the ordinary way, *i.e.* by one or a few competent persons; it did not occur to me that they were of so much scientific value as to call for such "an assembly of gentlemen" as that which met at Bedford Square. Therefore, in writing my report, I took it for granted that Dr. Carpenter would have concurred in the "regret" which I expressed that his friendly recommendation should have been, as I thought, so far misused by Mr. Bishop as to constitute a general advertisement to scientific men; and my expression of regret was thus intended to show that I did not suppose Dr. Carpenter was to be considered intentionally responsible for the excitement which Mr. Bishop has succeeded in creating. It would no doubt have been wiser had I ascertained Dr. Carpenter's views upon this subject before assuming that they were the same as my own, and I do not yet quite understand whether he considers Mr. Bishop's manifestations worthy of all the attention which they have received. But in any case I hope that Dr. Carpenter will accept as more satisfactory an expression of further "regret," when I say I am very grieved to find that my allusion to his relations with Mr. Bishop, although intended as a friendly allusion, does not appear to have met with his approval.

GEORGE J. ROMANES

Re W. I. Bishop

LET any one read carefully Dr. Carpenter's account of the card trick exhibited to him by Mr. Bishop; let him suppose that Mr. Bishop had two packs of cards, the one an ordinary pack for exhibition to the company, and the other a pack containing fifty-two cards, all alike (the backs of both packs being of the same pattern). Let Mr. Bishop now perform the trick with cards from the latter pack, and his success can be readily explained. But grant that Mr. Bishop had only one pack of ordinary cards: even then it is possible that the explanation of the trick is not hard to find.

Dr. Carpenter allows that Mr. Bishop *may* have known where the selected card was placed. Take Dr. Carpenter's diagram on p. 188, and let No. 11 be the card known to Mr. Bishop, and which is to be finally discovered by Dr. Carpenter. "Drop your left hand on any row you wish *taken away*," says Mr. Bishop to Dr. Carpenter. Suppose, *by chance*, B, D, and A successively dropped on and removed, as in the instance given by Dr. Carpenter, then the upper pair of row C, then 15, we have 11 left and the trick done.

Suppose that C is selected first. Mr. Bishop may now assure Dr. Carpenter that the card wanted is in that row, and that he has forced Dr. Carpenter to select it. The chances are equal that Dr. Carpenter will in his next selection drop on that pair in row C, which includes 11. Should Dr. Carpenter in his third choice drop on 11, a most convincing proof of Mr. Bishop's will compelling power will have been exhibited.

Should Dr. Carpenter however drop on 15, Mr. Bishop has merely to ask him to put it aside, and turning up the remaining card to exhibit it as the chosen and identified card. By a combination of the two methods of removing and leaving, Mr. Bishop can provide for all cases, and can perform a trick well known to schoolboys.

Dr. Carpenter, as I read his letter, tells us how Mr. Bishop acted when he himself was the subject of the experiment. If Dr. Carpenter can declare that the rows of cards, pairs of cards, and single cards *dropped in* were *in all three experiments* removed, I must confess that the laws of probabilities are against me, and that there seems to be strong proof of Mr. Bishop's power of *will-compelling*, a power which, as far as I have heard, Mr. Bishop has not yet publicly claimed to possess.

If Mr. Bishop *did not* know where the selected card was placed, Dr. Carpenter must invent a name for Mr. Bishop's new power of discovering a card, the position of which neither Mr. Bishop nor "the subject of the experiment" knew.

We can all regret with Dr. Carpenter "that Mr. Bishop did not offer for like careful testing experiments," &c.

I had the pleasure of attending a *public* performance given by Mr. Bishop in Edinburgh, on which occasion Mr. Bishop, much to the entertainment of a crowded hall, exhibited the legerdemain by which he had duped the subjects of, I believe, the before-mentioned experiments.

At this entertainment Mr. Bishop also showed how spiritualists performed such feats as knocking nails into boards, putting rings on scarves, &c., while their hands were tied together behind their backs and secured to a post. Prof. Turner, of the University of Edinburgh, explained to the spectators (no doubt at Mr. Bishop's request) that Mr. Bishop *seemed* to be enabled to perform those feats by the peculiar conformation of the bones and muscles—perhaps both—of his shoulder and arm.

We are told by newspaper correspondents that to this physical gift Mr. Bishop has added the power of reading and getting pictures of his subjects' thoughts, and now Dr. Carpenter endows him with the power of controlling the wills of his subjects, or—"*may*" teste—with some unnamed power still more mysterious. To Mr. Bishop as the successor of the Westminster whale or of Master Pongo, no one can have the slightest objection. Mr. Bishop as a *great* scientific phenomenon will, I fear, require better backing than the *careful testing* of Dr. Carpenter, and letters of introduction from scientific and medical men in Edinburgh who received Mr. Bishop, and in their turn gave him letters of introduction as a clever conjuror who performed by mechanical means feats of strength and agility attributed by spiritualists to their immaterial familiars.

THOMSON WHYTE

Merchiston Castle School, Edinburgh, July 2

Mind-Reading versus Muscle-Reading

SEVERAL years ago I had the opportunity of witnessing in a private circle of friends some experiments on so-called "thought-reading," even more striking than those recently described in your columns and elsewhere. An attentive observation of these experiments led me to question the accuracy of that explanation of the phenomenon with which Dr. Carpenter has made us so familiar, namely, unconscious muscular action on the one side, and unconscious muscular discernment on the other. After making the most extravagant allowances for the existence in some persons of a muscular sense of preternatural acuteness, here still remained a large residuum of facts wholly unaccounted for on any received hypothesis. These facts pointed in the direction of the existence either of a hitherto unrecognised sensory organ, or of the direct action of mind on mind without the intervention of any sense impressions. Such startling conclusions could not be accepted without prolonged and severe examination, and it was solely in the hope of stimulating inquiry among those who had more leisure and more fitness for the pursuit than myself that I published the brief record of my experiments which, some years ago, brought derision and denunciation upon me. As no physiologist came forward to give the subject the wide and patient inquiry it demanded, I went on with the investigation, and for five years have let no opportunity slip which would add to the information I possessed. A letter addressed to the *Times*, asking for communications from those who had witnessed good illustrations of the "willing game," brought me in, at the time referred to, a flood of replies from all parts of England, and down to the present time fresh cases are continually coming under my notice. Each case that seemed worthy of inquiry was, if possible, visited and investigated either by myself during the vacation, or by a friend on whom I could rely. It is true that many long journeys have been taken and much time has been spent without a commensurate reward, but this was to be expected. Still, after casting out cases which might or might not have been due to "muscle-reading," there remained abundant evidence to confirm my belief in the insufficiency of Dr. Carpenter's explanation. Until this evidence is published, which it will shortly be, and the accessible cases are examined and reported upon by a competent and impartial committee, I simply ask the public to suspend their judgment on this question. And to show that this is not an unreasonable request on my part, I here give a few particulars of a remarkable case which reached me only a few months ago, and was carefully investigated by myself last Easter.

A clergyman in Derbyshire has five young children, four girls and one boy, aged from nine to fourteen years, all of whom are able to go through the ordinary performances of the "willing game" rapidly and successfully, *without the contact of the hands or of any communication besides the air between the person operating and the subject operated on*. More than this, letters and words, or names of places, of persons, and of cards, can be guessed with promptness and accuracy; the failures in any examination not amounting to one in ten consecutive trials. The failures, I am assured by the father—and there is no reason to doubt his veracity—form a far smaller fraction when the children are not embarrassed by the presence of strangers; for example, the parents assured me that their children, before I arrived, told correctly seventeen cards chosen at random from a pack, without a single failure, and after that correctly gave the names of a dozen English towns indiscriminately selected. I will however only ask attention to what came under my own observation, which in brief was as follows:—

One of the children, Maud, a child of twelve, was taken to an adjoining room, and both the doors between fastened. I then wrote on paper the name of some object *not in the room* (to prevent unconscious guidance by the *eyes* of those who knew the thing selected), and handed this paper round to those who were present. Not a word was allowed to be spoken. I myself then recalled the child, placed her with her back to the company, or sometimes blindfolded her before bringing her into the room, and put her in a position where no whisper or other private communication could reach her undetected. In from two to twenty seconds she either named the object I had written down (the paper, of course, being concealed) or fetched it, if she could do so without difficulty. Each child was tried in succession, and all were more or less successful, but some were singularly and almost invariably correct in their divination of what I had written down; what was more curious, the maid-servant was equally sensitive. This led me to try other experiments with those who knew the words chosen: and the father was found to be pre-eminently the best willer, and to be in fact almost as necessary for success as the sensitive "guesser"; further experiments showed that a battery of minds, all intently fixed on the same word, was far more successful than one or two alone. Apparently a *nervous induction* of the dominant idea in our minds took place on the passive mind of the child, and the experiments recalled the somewhat analogous phenomena of electric and magnetic induction. There seemed to be a veritable exoneural action of the mind.

I am quite prepared for the chorus of sceptical laughter which will greet this statement. That there should be disbelief is quite natural; a desire for further inquiry is all I ask for. To those who, with a single eye for truth, even if it be in collision with received opinions, are anxious to know if every possibility of error or deception was removed, permit me to add the following additional experiments. Instead of allowing the child to return to the drawing-room, I told it to fetch the object as soon as it "guessed" what it was, and *then* return with it to the drawing-room. Having fastened the doors I wrote down the following articles one by one with the results stated: *hair-brush*, correctly brought; *orange*, correctly brought; *wine-glass*, correctly brought; *apple*, correctly brought; *toasting-fork*, wrong on the first attempt, right on the second; *knife*, correctly brought: *smoothing-iron*, correctly brought; *tumbler*, correctly brought; *cup*, correctly brought; *saucer*, failure. On being told this object the child said, "Saucer came into my head, but I thought you would never ask for that after asking for a cup, so I wasn't sure what it was." Then names of towns were fixed on, the name to be called out by the child outside the closed door of the drawing-room, but guessed when fastened into the adjoining room. In this way Liverpool, Stockport, Lancaster, York, Manchester, Macclesfield were all correctly given; Leicester was said to be Chester; Windsor, Birmingham, and Canterbury were failures. I might give many other similar trials, for I spent three long evenings testing the children; but these results and the attempts made to answer the many questions that at once started to the mind, such as the effect of distance, &c., must be left for the present. Meanwhile, at the suggestion of Mr. Romanes, I have arranged for a small committee of scientific experts to visit the family, and verify or disprove the conclusion to which I have arrived, which is certainly opposed to that drawn by Mr. Romanes from his experiments on Mr. Bishop (NATURE, vol. xxiv. p. 172). Whether Dr. Carpenter will find in this case "a precise confirmation" of everything he has said on the subject I cannot say.

W. F. BARRETT

July 3

A Case of Slow, Sub-Tropical Discharge of Earth-Electricity, and the Sun Recognisant thereof

IN the course of yesterday afternoon, in the midst of a sky otherwise clear and exquisitely blue, a large cloud of unusual shape and character began to form in the upper regions of the atmosphere vertically over, but very far above, the southern slope and even most elevated mountain tops of Madeira, and remaining there, as it did, most fixedly more than half the day, so contrary to the locomotive habits of ordinary clouds, it soon attracted the attention, and presently the fears, of most of the inhabitants.

As seen from this place, between 1h. and 3h. p.m., there was little more than a single dense cloud of peculiarly rounded outline and somewhat elliptical figure, stretching from the western horizon to within 10° or 15° of the zenith; but as time advanced,

other and successively smaller clouds were formed directly under the first, having symmetrical and concentric outlines therewith, while the central vertical axis, which might be conceived as passing through the whole series, remained unchanged and fixed in space. This central fixity, too, of them all continued, together with the infinite smoothness of the outlines of all the smaller lower strata of cloud, although the largest and uppermost one visible to us began to put forth a variety of fringes of cirro-cumulous character; and, as tested by the spectroscope before sunset, all the lower smooth-rimmed clouds were remarkable for the large quantity of watery vapour they contained, and held fast too, for no rain fell. As sunset approached every one was gazing at the strange phenomenon of a cloud-congeries of most portentous size and absolute fixature above the trade-wind, probably also the anti-trade region; and after sunset the most gorgeous coloured illuminations through all the ranges of scarlet-red, red, crimson-red, ultra-red, and then dun-coloured and grey passed from member to member of the series, distinguishing the various heights of its strata one above the other; while the greatness of the general height was shown, even long after darkness had set in, by a faint lunar-like illumination of the northern outline of the whole. But by ten o'clock that began to fail, and the system of superposed clouds was beginning to contract on its central axis, and faded away, without leaving its place, before morning.

In so far we had been witnessing, though without any positive light of its own, a vertical series of disks of cloudy matter, extremely like the lower end of the successive, transverse, discous arrangements seen in a gas vacuum-tube of large dimensions, when the electric discharge from a powerful induction-coil is passing through it; and we were inevitably reminded thereby that the cosmical electric theory of M. Gaston Planté (of "secondary batteries or storage" fame) justifies an escape of the earth's interior electricity from time to time into planetary space, and more particularly to the sun.

Was there, however, in this case any symptom of the sun exciting, or calling for, any such discharge, and from this part of the earth?

The sun was undoubtedly in the Northern Tropic, and the highest northern declination for the year had just been reached; but for a fortnight or more past the solar spot manifestations had generally been weak, almost fading away. This I knew well, having taken a picture of the sun-spots every day (Sundays excepted) since I have been here. However, though the appearances were as poor as they well could be on June 21, 22, and 23, yet on Friday, June 24, there was a little improvement, some new, though small symptoms appearing in either solar tropic. On Saturday, June 25, these new features were confirmed and slightly increased. But what were they on Sunday, June 26, when the extraordinary cloud-arrangement was hanging so long above Madeira?

I, who am here merely as a private amateur in a different subject, know not; but on Monday morning, so early as 5h. 30m. a.m., I was astonished and delighted at the solar scene then presented. The spots first caught sight of on Friday were now well advanced and much developed; a new group with extensive double ramifications had also appeared in the same tropic nearer the equator; while finally, near the middle of the sun's disk in the south tropic, were two large spots, with connections extending over 60,000 miles in length of solar surface, and indicating more solar energy to have been thereby rapidly, if not suddenly, manifested within the last forty hours, than anything which I, at least, have witnessed for a very long time past.

PIAZZI SMYTH,
Astronomer-Royal for Scotland

Jones's Hotel, Quinka do Corvalho,
Funchal, Madeira, June 27

P.S.—The grand, and now circumpolar, comet was not neglected here on the same night.—P. S.

Carbonic Acid Gas not Free in Sea Water

IN a short paragraph in NATURE, vol. xxiv. p. 176, it is stated that Tornö, in the Norwegian Deep-Sea Expedition, had found "carbonic acid both in a gaseous and basic form."

For some time past I have doubted whether there was any free carbonic acid gas in the deep water where pressure should make its presence felt. Lately, in a paper to the Royal Microscopical Society, I have demonstrated that if there is any carbonic acid in the sea water at great depths, its dissolving action is not equal in rapidity and intensity to that exercised by a microscopic Thallophyte which bores into and bisselves sponge spicules from within. Moreover amongst deep sea deposits I find perfect organisms which have long been dead, which have been penetrated by parasites and covered here and there by foraminifera, and yet in exposed parts, the ornamentation is perfect. There is no evidence of erosion.

Now on carefully examining into Tornö's essay I come to a different conclusion to the writer in NATURE, and I find that the able Scandinavian denies the existence of free carbonic acid in the sea.

The following notes, which I made in abstracting Tornö's "Chemi" of the Norwegian North Atlantic Expedition, Part II., may be interesting:—

The carbonic acid gas, driven off by the process of boiling sea water, when collected, varied in a most marked manner; it was always appreciable, and the quantity was sometimes large. The pressure was that of the atmosphere. Under different conditions, and when the gases were boiled out in a vacuum created by steam, and of course at a lower temperature, the quantity of carbonic acid gas was often *immeasurably small*. Moreover the quantity varied.

Jacobsen, by distillation, succeeded in expelling the whole amount of carbonic acid contained in a quarter litre of sea water, and found that North Sea water contained 100 mgr. per litre. The neutral carbonates in the residuary deposit contained about 10 mgr. per litre. Hence a very small proportion of the carbonic acid driven off by distillation, could have been present in bicarbonates. Vierthaler had asserted that the carbonic acid in sea water was got out of the bicarbonates by boiling.

If the carbonic acid is free and absorbed by the sea water in a free gaseous form, it is remarkable that it should not be more readily got. Jacobsen supposed that sea water has a peculiar property of retaining its carbonic acid, owing to the presence of the chloride of magnesia. Buchanan was led to believe that most of the salts were in some degree distinguished by the property of determining the retention of carbonic acid in the sea. He especially insisted on the importance of the sulphates, and asserted the mean amount of carbonic acid present in the waters of the Southern Seas to be 43·25 mgr. per litre.

Tornö, following Jacobsen, found the amount of carbonic acid gas present in the water of the track of the northern cruise of 1877 to be about 100 mgr. a litre, but got 12 mgr. per litre as a variation in the amount.

He was struck with the improbability that sea water should possess so remarkable a power of retaining mechanically one gas and exert no corresponding influence on others, and then he found that sea water had *an alkaline reaction*. He began to believe that some of the neutral carbonates had been decomposed during the boiling, and had evolved much of the carbonic acid gas.

He then proved by experiment that the saline mixture in sea water, on the temperature being raised to the boiling point, decomposed neutral carbonates, and that all previous experiments with the object of measuring the carbonic acid in the sea water had been faulty. He was influenced by some experiments on the determination of carbonic acid gas in mineral water, and applied the method to sea water.

He found the total amount of carbonic acid gas in a specimen to be 97 mgr. per litre, and the proportion forming neutral carbonates to average about 53 mgr. The difference, 44 mgr., cannot occur free as gas, but will unite with the carbonates to form bicarbonates. Hence Jacobsen's experiments could be explained on the assumption that sea water contains no trace of free carbonic acid, but as much as 53 mgr. per litre forming carbonates, and only 44 mgr. forming bicarbonates.

On page 35 he states: "*If we bear in mind that sea water is an alkaline fluid which does not contain the smallest trace of free carbonic acid.*"

What a comfort this must be to globigerina and coral reefs!

June 27 P. MARTIN DUNCAN

Symbolical Logic

I AM afraid I share the proverbial obtuseness of my countrymen in the matter of jokes. I really did not at first see the point of Mr. Venn's humorous suggestion that "an attitude of slight social repression" should be observed towards troublesome authors of new proposals. Now however that Mr. Venn has kindly pointed it out to me (see NATURE, vol. xxiv. p. 140), I see the joke perfectly and can laugh at it heartily.

As for the little parenthesis which offended me, I am sorry I noticed it, and hope Mr. Venn will forgive the passing irritation which it produced. What he means by the words "I knew that he was very anxious that the fact should be known," I do not quite understand; but the matter is too unimportant for further comment.

With regard to the "crowning triumph" quotation or misquotation, I can only congratulate Mr. Venn on the adroitness with which he eluded the dilemma in which I quite thought I should place him. In my simplicity I expected that he would answer *Yes* or *No* to my question; but Mr. Venn was not thus to be caught.

It is but fair to own that the critical remarks which I made on Mr. Venn's book in my last letter, though perfectly just as far as they go, are somewhat one-sided. As I only spoke of points on which he and I differ in opinion it could not well be otherwise. His book contains much other matter which I did not touch upon at all, and of which I entertain a very high opinion. His diagrammatic method especially is most ingenious, and his exposition of it is lucid and attractive. The limits of its application in actual practice are, as he himself points out, rather narrow; but within those limits, and for purposes of illustration and verification, it is undoubtedly an important contribution to the science of logic.
HUGH McCOLL

Boulogne-sur-Mer, July 2

How to Prevent Drowning

THOSE who have followed the correspondence commenced in NATURE by Dr. MacCormac may be interested in the following extract from an essay, "Pourquoi les Bêtes nagent naturellement," which occurs oddly enough in a book entitled "Observations sur les Plantes et leur Analogie avec les Insectes," published at Strasburg in 1741 by Guido Augustin Bazin, a physician of that place:—

"Lorsqu'un homme qui n'a point appris à nager tombe dans l'eau, il n'y a point de doute que s'il pouvoit tenir son corps dans une position verticale et fixe, et porter ses jambes en avant, comme il fait lorsqu'il marche sur la terre, il ne pût nager naturellement aussi bien que, les bêtes, les habiles nageurs le font souvent pour leur plaisir. Nous connoissons un peuple entier qui ne nage pas autrement, ce sont les Hottentots; voici ce qu'en dit Mr. Kolbe, dans une bonne description qu'il nous a donnée du Cap de bonne Espérance:—'Aussi faut-il avouer qu'ils (les Hottentots) sont les meilleurs et les plus hardis nageurs que j'aye jamais vû. Leur manière de nager a même quelque chose de frappant, et je ne sçache pas qu'aucune nation s'y prenne de la même façon. Ils nagent tout droits; leur col est entièrement hors de l'eau, aussi bien que leurs bras, qu'ils étendent en haut; ils se servent des pieds pour avancer, et pour se mettre en équilibre, mais je n'ai jamais pu sçavoir comment ils les font jouer. Tout ce qu'il y'a de sûr, c'est qu'ils avancent très vite. Ils regardent en bas, et ont presque la même attitude que s'ils marchoient sur terre ferme.' Mais cette attitude est impossible à un homme qui ne s'est pas point exercé à la prendre, parce que les mouvements de l'eau, et l'incertitude de son corps, toujours vacillant dans un liquide, le tirent à tout moment de la direction verticale, et l'entraînent malgré lui en avant ou en arrière" (pp. 44, 45).
W. T. THISELTON DYER

Resonance of the Mouth Cavity

I HAVE not tried Mr. Naylor's experiment, but from the account which he gave of it I could not see that any novel fact was involved, nor do I now see that the fact of "the different rates of vibration being *already in the air*" alters materially the conditions of the case. Whether the sounds are produced by the clatter of wheels, the impact of the thumb-nail upon the teeth, or by the vibrating tongue of a jew's-harp, the part played by the mouth-cavity in selecting the notes of a tune is substantially the same.
GEORGE J. ROMANES

Storage of Energy

LIKE many others, I have given much thought to the accumulation of force, and have felt much astonished at the account of Faure's battery, if it is to be so called, although of course such a development was to be expected from the time that Planté made his.

I see that men immediately rush to waterfalls, rivers, and tides to obtain the power for accumulation when they leave coal and wood; my ideas are rather in the direction of wind; and I have often pictured our country covered, like that around Zaandam, with windmills. The wind is not constant, but more so than most of our efficient streams, and it is found at every spot. The power is quite unlimited, and we can moderate the action of the machinery whenever we obtain the requisite force. Storage has hitherto been required. I have imagined our windmills pumping up water to great reservoirs, but we have not yet learned to make reservoirs for water except at an enormous expense and in unprotected valleys; other imaginings have come into many minds, but if we have a really true and safe storage, such as described, the wind will become our fire to warm us, our steam to drive us, our gas to light us, and our universal servant. The wind will drive our mills, too (except when a fog comes, lasting so long that our stores of power fail), with sufficient storage, inconstancy will cease to trouble us, whilst every valley may have its lights and every mountain-top its beacon, and darkness will scarcely trouble mankind in this new-coming world of light. We have heard of the golden servants of Vulcan and the mechanical slaves of the great Khan. What will be the result when every man has the wind at his command and the lightning at his service by friction, like Aladdin? It seems to me that the wind is the great power that we shall next use, and that Prince—the power of the air—shall be bound to serve us for at least a thousand years.

The Dutch have long made windmills, but when over in Holland a few years ago examining a little, I was unable to find the books wanted on the subject.

The fact that coal can be carried will not affect the question if wind is used. Wind carries itself. We shall seek our power from the heavens instead of the infernal pits, and a race of healthy, ruddy faces will take the place of the blackened and degraded countenances from mines.

I wish to show that we have excess of power in the wind. Will this new accumulator, of which I know nothing from personal experience, serve us to keep it? To keep it a few hours is a great point. Coal becomes secondary if we accumulate the force of the wind, and Niagara itself will be no longer wanted. Of course we need machines to use the wind-power. At present coals are cheaper with us; not so in all parts of Holland, and not so in many other places. However, here we have problems enough to solve; do not let us throw cold water on the discoveries of others, or show, as scientific men so often do, our own opinion to be dear beyond the truth among others.
R. A. S.

Explanation of the Female Dimorphism of Paltostoma torrentium

IN his paper on "*Paltostoma torrentium*, eine Mücke mit zweigestaltigen Weibchen" (*Kosmos*, vol. viii. pp. 37-42), my brother Fritz Müller supposes that this species of Blepharoceridæ originally was blood-sucking, but in later times changed its habits and became fond of flower-nectar. In the males, who need only little food, this change of habits and the corresponding change of the mouth-parts was accomplished, my brother supposes, more rapidly and perfectly than in the females, who, maturing eggs and passing the winter, stand in need of more albuminous food than the males do. Whilst therefore in some females of *Paltostoma torrentium* the same change of habits and mouth-parts has taken place as in the males, other females have still more or less continued their original blood-sucking habits and preserved their original blood-sucking instruments.

This explanation given by my brother is not yet proved by any direct observation of *Paltostoma's* habits. He mentions, as an indirect argument for his opinion, that in several Diptera the females have been stated to be blood-sucking, whilst the males take nectar of flowers. It may therefore be worth publishing, that in *Empis punctata* really just the same takes place as my brother's explanation of the female dimorphism in *Paltostoma torrentium* requires to be supposed: males who exclusively feed on flower-nectar, besides females, both enjoying flower-honey and attacking living animals and sucking their blood. Several weeks ago (May 26) a great many males as well as females of *Empis punctata* roved on the flowers of hawthorn (*Cratægus oxyacantha*). The males were exclusively occupied with sucking nectar. Of the females some did the same, whilst others attacked, murdered, and consumed the most clever visitor of flowers among all our Syrphidæ, *Rhingia rostrata*.
HERMANN MÜLLER

ACROSS AFRICA[1]

TO cross Africa has almost ceased to be an extraordinary feat. Indeed it seems evident, the more we know of the Portuguese native traders, that even before Livingstone's memorable first journey, it was no uncommon thing for the Pombeiros to do in the ordinary way of business. Of course some routes are more dangerous than others, and that by which Stanley made his famous march was perhaps the most difficult and dangerous that could be selected. Still the journey performed by Major Serpa Pinto was in many ways remarkable, and perhaps not its least remarkable feature is the characteristic manner in which he tells his story. The Major's narrative is in every respect a contrast to the quiet and sober narrative of Dr. Holub, recently reviewed in these pages. The Major is all excitement and enthusiasm, and his frequent digression to unbosom himself of his feelings under his frequently trying circumstances, though they do not convey much information, are pleasant reading. The expedition of which he was leader was fitted out very handsomely by the Portuguese Government, its object being to cross the continent from the Portuguese settlements in the west to those on the east coast. He was accompanied by MM. Ivens and Capello, but these soon parted from him, and conducted an exploration on their own account, the full narrative of which has yet to be published. Much time was wasted at the outset before the expedition could leave Benguella, collecting carriers and making other arrangements, so that it was January, 1878, before the Major fairly started for the interior. Although much of the ground he traversed had been gone over before, coinciding partly with the route of Livingstone, still he was able to open up a considerable stretch of new country, and most of all to clear up to a great extent the complicated hydrography of the region lying between the West Coast and the Zambesi. While the Major has many interesting notes on the natural history of the country he traversed, and while he seems to have been able to bring to light some new animals and not a few new plants, the main value of his narrative lies in the full details he gives on the geography and ethnology of Western South Africa. He was unable to carry out the original programme of the expedition, having been compelled to turn southwards on reaching the Central Zambesi, reaching the East Coast at Natal. On leaving Benguella the Major proceeded in a south-easterly direction towards the Cunene, before reaching which he turned north-eastwards, proceeding by Caconda to Bihé. After staying here for some time he again turned south-eastwards across the Cuando to the Zambesi, a little below its junction with the Liba, which seems to have more right to be considered the main stream than that which comes from the east. Proceeding down the Zambesi, passing numerous cataracts, he got into trouble among the Barotse, a new king having succeeded to the deposed Sepopo, whom Dr. Holub found ruling the Marutse-Mabunda kingdom at Seshcke. Escaping with bare life, he fell in with the French missionary family Coillard, who gave him all possible succour, visited and attempted to survey the Victoria Falls, and proceeded southwards and eastwards in a leisurely way into country pretty well known, but of which and of its various native states he is able to give us some interesting details. Between the West Coast and the Zambesi the expedition must have crossed hundreds of rivers, many of which Major Pinto has laid down with approximate accuracy in his maps. For he deserves the highest praise for the persistency with which he took his observations under the most trying circumstances, so that to the cartographer his work is of the greatest value. It is no easy matter to discriminate the various watersheds here, and indeed the observations of Major Pinto, combined with those of previous travellers, shows that many of the rivers which flow north to the Congo, south-west to the Atlantic, south by Cubango to Lake Ngami, and south-east to the Zambesi, rise quite close together on what is really a table-land; and in the rainy season it will often be difficult for them to make up their minds which direction they shall take. Major Pinto's numerous maps tend greatly to clear up the complicated hydrography of this region.

The country through which he passed to reach the Zambesi is varied in its aspect and productiveness, though most of it is luxuriantly fertile, and capable of great development. Much of it is however swampy, and even cultivated fertile districts are depopulated, mainly through wars and slave-hunting. Major Pinto tells us much that is interesting on the metal-working, which is common along the first part of his route. There seems to be really a large store of iron in this region, and the natives show considerable ingenuity in working it. There are several chief centres for these operations, and the metal is fashioned into all sorts of implements and weapons.

FIG. 1.—Cabango Man.

"During the coldest months, that is to say June and July, the Gonzellos miners leave their homes and take up their abode in extensive encampments near the iron-mines, which are abundant in the country. In order to extract the ore they dig circular holes or shafts of about ten to thirteen feet in diameter, but not more than six or seven feet deep; this arises most probably from their want of means to raise the ore to a greater elevation. I examined several of these shafts in the neighbourhood of the Cubango, and found them all of a similar character. As soon as they have extracted sufficient ore for the work of the year they begin separating the iron. This is done in holes of no great depth, the ore being mixed with charcoal, and the temperature being raised by means of primitive bellows, consisting of two wooden cylinders about a foot in diameter, hollowed out to a depth of four inches, and covered with two tanned goat-skins, to which are fixed two handles, twenty inches long and half an inch thick. By a rapid movement of these handles a current of air is produced which plays upon the charcoal through two hollow wooden tubes attached to the

[1] "How I Crossed Africa, from the Atlantic to the Indian Ocean," &c. By Major Serpa Pinto. Translated from the Author's manuscript by Alfred Elwes. Two vols. Maps and Illustrations. (London: Sampson Low and Co., 1881.)

cylinders, and furnished with clay muzzles. By incessant labour, kept up night and day, the whole of the metal becomes transformed, by ordinary processes, into spades, axes, war-hatchets, arrow-heads, assegais, nails, knives, and bullets for fire-arms, and even occasionally fire-arms themselves, the iron being tempered with ox-grease and salt. I have seen a good many of these guns carry as well as the best pieces made of cast steel."

The book contains several illustrations of the methods adopted, and the double-bellows used for the furnace is very curious. His observations on the animals met with along his route are valuable, and he has carefully indicated on his map where the principal animals are found. Elephants seem to be abundant enough south-east of Bihé, and lions were met with in considerable numbers as the Zambesi was approached. He also met with the huge and dangerous buffaloes familiar to readers of Livingstone's First Journey. One of our illustrations gives a good idea of an antelope which was met with in the Cuchibi, which the Major thus describes:—

"At one of the turns of the river I perceived three antelopes of an unknown species, at least to me; but just as I was in the act of letting fly at them they leaped into the water and disappeared beneath its surface. The circumstance caused me immense surprise, which was increased as I went further on, as I occasionally came across several of these creatures, swimming, and then rapidly diving, keeping their heads under water, so that only the tips of their horns were visible. This strange animal, which I afterwards found an opportunity of shooting on the Cuchibi, and of whose habits I had by that time acquired some knowledge, is of sufficient interest to induce me for a moment to suspend my narrative to say a few words concerning it. It bears among the Bihenos the name of Quichôbo, and among the Ambuellas that of Buzi. Its size, when full grown, is that of a one-year-old steer. The colour of the hair is dark grey, from one quarter to half an inch long, and extremely smooth; the hair is shorter on the head, and a white stripe crosses the top of the nostrils. The length of the horns is about two feet, the section at the base being semicircular, with an almost rectilinear chord. This section is retained up to about three-fourths of their height, after which they become almost circular to the tips. The mean axis of the horns is straight, and they form a slight angle between them. They are twisted around the axis without losing their rectilinear shape, and terminate in a broad spiral. The feet are furnished with long hoofs similar to those of a sheep, and are curved at the points. This arrangement of its feet and its sedentary habits render this remarkable ruminant unfitted for running. Its life is therefore in a great measure passed in the water, it never straying far from the river banks, on to which it crawls for pasture, and then chiefly in the night-time. It sleeps and reposes in the water. Its diving-powers are equal, if not superior, to those of the hippopotamus. During sleep it comes near to the surface of the water, so as to show half its horns above it. It is very timid by nature, and plunges to the bottom of the river at the slightest symptom of danger. It can easily be captured and killed, so that the natives hunt it successfully, turning to account its magnificent skin and feeding off its carcase, which is however but poor meat. Upon leaving the water for pasture its little skill in running allows the natives to take it alive; and it is not dangerous, even at bay, like most of the antelope tribe. The female, as well as the male, is furnished with horns. There are many points of contact between the life of this strange ruminant and that of the hippopotamus, its near neighbour. The rivers Cubangui, Cuchibi, and the upper Cuando offer a refuge to thousands of Quichôbos, whilst they do not appear either in the lower Cuando or the Zambesi. I explain this fact by the greater ferocity of the crocodiles in the Zambesi and lower Cuando, which would make short work of so defenceless an animal if it ventured to show itself in their waters."

Major Pinto's account of the powerful kingdom of Bihé is full of interest. It is evident from his narrative and those of Dr. Holub and Mr. Joseph Thomson that these African states are in a constant state of unstable equilibrium. Not only are the chiefs and dynasties frequently changed, but an entire population may be removed or reduced to slavery, and its dominant place taken by a conquering people. The Bihenos are probably the most extensive travellers in Africa.

"Where travelling is concerned as connected with trade, nothing comes amiss to the Bihenos, who seem ready for anything. If they only had the power of telling where they had been and describing what they had seen, the geographers of Europe would not have occasion to leave blank great part of the map of South Central Africa. The Biheno quits his home with the utmost indifference, and bearing a load of sixty-six pounds of goods, will start for the interior, where he will remain two, three, and four years; and on his return, after that lapse of time, will be received just as though he had been on a journey of as many days. Silva Porto, whilst engaged in doing business with the Zambesi, was despatching his negroes in other directions, and was trading at the same time in the Mucusso country and in the Lunda and Luapula territories. The fame of the Bihenos has travelled far and wide, and when Graça attempted his journey to the Matianvo he first proceeded to the Bihé to procure carriers. These people have a certain emulation among one another as travellers, and I met with many who prided themselves on having gone where no others had ever been, and which they called *discovering new lands*. They are brought up to wandering from their very infancy, and all caravans carry innumerable children, who, with loads proportionate to their strength, accompany their parents or relatives on the longest journeys; hence it is no uncommon thing to find a young fellow of five-and-twenty who has travelled in the Matianvo, Niangué, Luapula, Zambesi, and Mucusso districts, having commenced his perigrinations at the age of nine years."

Major Pinto has a good deal to tell us of the various kinds of ants he met with on his journey, though the value of his observations is much decreased from his want of a knowledge of entomology. Here is his account of one terrible insect:—

"When the work of cutting down the wood for our encampment commenced I saw a sudden commotion among my blacks, who then took to their heels in every direction. Not understanding the cause of their panic, I immediately proceeded to the spot to make inquiries. On the very place which I had selected for my camp appeared issuing from the earth millions of that terrible ant called by the Bihenos *quissonde*, and it was the sight of these formidable creatures which scattered my men. The quissonde ant is one of the most redoubtable *wild beasts* of the African continent. The natives say it will even attack and kill an elephant, by swarming into his trunk and ears. It is an enemy which, from its countless numbers, it is quite vain to attack, and the only safety is to be found in flight. The length of the quissonde is about the eighth of an inch; its colour is a light chestnut, which glistens in the sun. The mandibles of this fierce hymenopter are of great strength, and utterly disproportioned to the size of the trunk. It bites severely, and little streams of blood issue from the wounds it makes. The chiefs of these terrible warriors lead their compact phalanxes to great distances and attack any animal they find upon the way. On more than one occasion during my journey I had to flee from the presence of these dreadful insects. Occasionally upon my road I have seen hundreds of them, apparently crushed beneath the foot, get up and continue their march, at first somewhat slowly,

but after a time with their customary speed, so great is their vitality."

The author gives some valuable details concerning the Mucassequeres, who seem to be a remnant of one of the primitive African tribes.

"The Mucassequeres occupy, jointly with the Ambuellas, the territory lying between the Cubango and Cuando, the latter dwelling on the rivers and the former in the forests; in describing the two tribes, one may say that the latter are barbarians and the former downright savages. They hold but little communication with each other, but, on the other hand, they do not break out into hostilities. When pressed by hunger the Mucassequeres will come over to the Ambuellas and procure food by the barter of ivory and wax. Each tribe would seem to be independent, and not recognise any common chief. If they do not fight with their neighbours they nevertheless quarrel among themselves; and the prisoners taken in these conflicts are sold as slaves to the Ambuellas, who subsequently dispose of them to the Bihé caravans. The Mucassequeres may be styled the true savages of South Tropical Africa. They construct no dwelling-houses or anything in the likeness of them. They are born under the shadow of a forest-tree, and so they are content to die. They despise alike the rains which deluge the earth and the sun which burns it; and bear the rigours of the seasons with the same stoicism as the wild beasts. In some respects they would seem to be even below the wild denizens of the jungle, for the lion and tiger have at least a cave or den in which they seek shelter, whilst the Mucassequeres have neither. As they never cultivate the ground, implements of agriculture are entirely unknown among them; roots, honey, and the animals caught in the chase constitute their food, and each tribe devotes its entire time to hunting for roots, honey and game. They rarely sleep to-day where they lay down yesterday. The arrow is their only weapon; but so dexterous are they in its use, that an animal sighted is as good as bagged. Even the elephant not unfrequently falls a prey to these dexterous hunters, whose arrows find every vulnerable point in his otherwise impervious hide. The two races which inhabit this country are as different

FIG. 2.—The [illegible]

FIG. 3.—Malanca.

in personal appearance as they are in habits. The Ambuella, for instance, is a black of the type of the Caucasian race; the Mucassequere is a white of the type of the Hottentot race in all its hideousness. Many of our sailors, browned by the sun and beaten by the winds of many a storm, are darker than the Mucassequeres, whose complexion besides has so much of dirty yellow in it as to make the ugliness more repulsive. I regret exceedingly my inability to obtain more precise data concerning this curious race, which I consider to be worthy the special attention of anthropologists and ethnographers. In my opinion this branch of the Ethiopic race may be classified in the group of the Hottentot division. In form it possesses many of the characteristics of the latter, and we may observe in this peculiar race a sensible variation in the colour of the skin. The *Bushmen* to the south of the Calaári are very fair of hue, and I have noticed some who were almost white. They are low of stature and thin of body, but exhibit all the characteristics of the Hottentot type. To the north of that same desert tract, more especially about the salt-lakes, there is another nomad race, that of the Massaruas, strongly built, of lofty stature, and of a deep black, who possess the same Hottentot type, and who indubitably belong to the same group. I was told on the Cuchibi that between the Cubango and the Cuando, but a good deal to the south, there existed another race, in every respect similar to the Mucassequeres, both in type and habits, but of a deep black colour. In consideration therefore of the affinity of character, I have no hesitation in admitting that the Hottentot group of the Ethiopic race extends to the north of the Cape as far as the country lying between the Cubango and the Cuando, passing through sundry modifications of colour and stature, due probably to the conditions under which they live, to altitude, to the great difference of latitude, or even to other causes that are less apparent."

By the time Major Pinto reached the Barotse territory and fell in with the hospitable missionary family Coillard, he had got on to comparatively well-known ground, though the interest of his story is sustained to the very end; and even here he succeeds in adding something to our knowledge of the countries through which he passed. His visit to the Great Falls of the Zambesi, and his illustrations taken from various points, are a material addition to what we know of them from the narratives of Livingstone and Mohr. Some of his observations are worth quoting, especially as, under circumstances of the greatest danger, he succeeded in making a fairly accurate survey. "Mozi-oa-tunia" is a Basuto word, meaning "the smoke is rising," "so that it is very easy to suppose how a name, common among the natives and apparently so apt and appropriate, came to be given by strangers to the cataract itself.

"*Mozi-oa-tunia* is neither more nor less than a long trough, a gigantic crevasse, the sort of chasm for which was invented the word abyss—an abyss profound and monstrous into which the Zambesi precipitates itself bodily to an extent of 1978 yards. The cleft in the basaltic rocks which form the northern wall of the abyss is perfectly traceable, running east and west. Parallel thereto, another enormous wall of basalt, standing upon the same level, and 110 yards distant from it, forms the opposite side of the crevasse. The feet of these huge moles of black basalt form a channel through which the river rushes after its fall, a channel which is certainly much narrower than the upper aperture, but whose width it is impossible to measure. In the southern wall, and about three-fifth parts along it, the rock has been riven asunder, and forms another gigantic chasm, perpendicular to the first; which chasm, first taking a westerly curve and subsequently bending southwards and then eastwards, receives the river and conveys it in a capricious zigzag through a perfect maze of rocks. The great northern wall of the cataract over which the water flows is in places perfectly vertical, with few or none of those breaks or irregularities that one is accustomed to see under such circumstances. An enormous volcanic convulsion must have rent the rock asunder and produced the huge abyss into which one of the largest rivers in the world precipitates itself. Undoubtedly the powerful wearing of the waters has greatly modified the surface of the rocks, but it is not difficult for an observant eye to discover clearly that those deep scarps, now separated from each other, must at one time have been firmly united. The Zambesi, encountering upon its way the crevasse to which we have alluded, rushes into it in three grand cataracts, because a couple of islands which occupy two great spaces in the northern wall divide the stream into three separate branches. The first cataract is formed by a branch which passes to the south of the first island, an island which occupies, in the right angle assumed by the upper part of the cleft, the extreme west. This branch or arm consequently precipitates itself in the confined space open on the western side of the rectangle. It is 196 feet wide and has a perpendicular fall of 262 feet, tumbling into a basin whence the water overflows to the bottom of the abyss, there to unite itself to the rest in rapids and cascades that are almost invisible, owing to the thick cloud of vapour which envelopes the entire foot of the Falls. The island which separates that branch of the river is covered with the richest vegetation, the leafy shrubs extending to the very edge along which the water rushes, and presenting a most marvellous prospect. This is the smallest of the Falls, but it is the most beautiful, or, more correctly speaking, the only one that is really beautiful, for all else at Mozi-oa-tunia is sublimely horrible. That enormous gulf, black as is the basalt which forms it, dark and dense as is the cloud which enwraps it, would have been chosen, if known in biblical times, as an image of the infernal regions, a hell of water and darkness, more terrible perhaps than the hell of fire and light. Continuing our examination of the cataract, we find that the beginning of the northern wall, which starts from the western cascade, is occupied to an extent of some 218 yards by the island I have before alluded to, and which confines that branch of the river that constitutes the first Fall. It is the only point whence the entire wall is visible, simply because along that space of 218 yards the vapour does not completely conceal the depths. It was at that point I took my first measurements, and by means of two triangles I found the upper width of the rift to be 328 feet, and the perpendicular height of the wall 393 feet. This vertical height is even greater further to the eastward, because the trough goes on deepening to the channel through which the river escapes to the south. At that point likewise I obtained data for measuring the height. In my first measurements I had as my base the side of 328 feet, found to be the upper width of the rift; but it was necessary to see the foot of the wall, and I had to risk my life to do so. After the first island, where I made my measurements, comes the chief part of the cataract, being the portion comprised between the above island and Garden Island. In that spot the main body of the water rushes into the abyss in a compact mass, 1312 feet in length, and there, as is natural, we find the greatest depth. Then follows Garden Island, with a frontage of 132 feet to the rift, and afterwards the third Fall, composed of dozens of falls which occupy the entire space between Garden Island and the eastern extremity of the wall. This third Fall must be the most important in the rainy season, when the masses of rock which at other times divide the stream are concealed, and but one unbroken and enormous cataract meets the eye. As the water which runs from the two first falls and from part of the third near Garden Island rushes eastward, it meets the remainder of the third Fall coursing west, and the result

is a frightful seething whirlpool, whence the creamy waters rush, after the mad conflict, into the narrow rocky channel before alluded to, and go hissing away through the capricious zigzag chasm."

FIG. 4.—Mosi-Oa-Tunia (the Victoria Falls).—The West Falls.

In the appendices and throughout the work Major Pinto gives many astronomical and meteorological observations which are of real scientific value. Altogether his work is one of the most attractive and instructive of recent narratives of African travel.

ÉTIENNE HENRY SAINTE-CLAIRE DEVILLE.

WE regret to record a serious loss to French chemistry in the death of the celebrated professor, Sainte-Claire Deville, which occurred July 1, at Boulogne-sur-Seine. Étienne Henry Sainte-Claire Deville was born March 11, 1818, on the island of St. Thomas, in the Antilles, and was of Creole origin. Like most of the youth in the French colonies, he was sent to Paris to undertake a course of study. Of his two brothers who also proceeded to France to enter upon active careers, one, the late Charles Sainte-Claire Deville, devoted himself likewise to science, and we have had occasion more than once to refer to his remarkable geological investigations in these pages. While the Creole element has rarely lacked in the artistic and literary circles of the French capital, we believe that the two brothers in question furnish the only notable instance in which science has profited from the highly imaginative and versatile Creole temperament. It is related of the young Henry that on completing his collegiate studies, he hesitated for a long time in making his choice between music and science. His decision was due in a great measure to the enthusiasm awakened at the time by the brilliant lectures and no less brilliant investigations of Jean Baptiste Dumas. Guided by the counsels of the latter, he equipped a laboratory, and commenced a series of investigations so fertile of results that in a short time he was ranked among the most promising of the younger school of chemists. In 1844 he entered upon professorial duties in accepting the Chair of Chemistry in the Scientific Faculty of Besançon, where, notwithstanding his comparative youth, he was appointed dean of his faculty. In 1851 he was called upon to succeed Balard as Professor of Chemistry at the *École Normale* of Paris. Gladly exchanging the comparative obscurity of a provincial university town for the manifold advantages of a Parisian professorship, he devoted himself with such ardour to the duties of his new position that, after a short lapse of time, the laboratory of the *École Normale* became one of the central points of chemical investigation, not only in France but in all Europe. In 1854 he accepted, in addition to his usual duties, a lectureship at the Sorbonne, which. fourteen years later, was changed for a full professorship. His favourite field of activity remained, however, the *École Normale*, and it was with difficulty, some months since, that he felt himself called upon to relinquish active professorial duties in consequence of rapidly increasing feebleness.

As an investigator, Deville made his *début* in organic chemistry in 1840 with a remarkable study of turpentine oil and various derivatives of the terpenes. His carefully tabulated results form the chief basis of our present knowledge of the different isomeric states of this group. They were followed in 1842 by a research on toluene, the importance of which was only duly felt on the introduction of the aniline colours. After minor investigations of various resins, Deville abandoned organic chemistry

to devote himself almost exclusively to the inorganic branch, and announced in 1849 his first grand discovery, that of nitric oxide. By demonstrating the existence of this interesting and important compound, as resulting from the action of chlorine on silver nitrate, $2AgNO_3 + Cl_2 = 2AgCl + O + N_2O_5$, Deville did much to stimulate the theoretical speculation of the day, especially among the opponents of the school of Gerhardt, whose theories did not recognise the possibility of the existence of monobasic acid anhydrides. After a few years devoted to varied studies of metallic carbonates and new analytical processes, he commenced in 1855 the famous research on metallic aluminium, which proved to be one of the crowning features of his lifework. Furnished with ample means by the munificence of Napoleon III., he was enabled to carry out experiments on a large scale, and so rapid was his success that even in 1855 he displayed at the Exhibition of Paris massive bars of this handsome metal, which previously had scarcely been seen in a pure state. The study of this metal and its metallurgical production, as well as of the various compounds of aluminium, carried out during a series of years, forms one of the most remarkable and complete contributions made to inorganic chemistry within a recent period. Deville's perfected process for the preparation of aluminium, as carried out in the two French and the single English establishment in which alone this metal is obtained, consists essentially in heating the double salt of aluminium and sodium, $AlCl_3.NACl$, with metallic sodium, fluorspar or cryolite being added as a flux. The metal thus obtained in the form of a solid regulus is used for a large variety of objects where lightness, strength, and freedom from oxidation are demanded, and forms the essential part of numerous valuable alloys. It has failed partly to meet the extended use to which Deville looked forward, on account of its comparatively high price and the difficulty of welding the metal. Among other industrial branches which we owe to Deville's efforts to create the manufacture of aluminium, such as the production of bauxit and cryolite, mention should especially be made of the manufacture of metallic sodium, the price of which sank in ten years from 2,000 francs to 15 francs per kilogramme. Deville's researches in this direction and his various methods of manufacture are to be found *in extenso* in his classical work, *De l'aluminium, ses propriétés, &c.*, 1859. In union with Caron he applied in 1863 the method found successful in the case of aluminium to the production of magnesium, and thereby created a second branch of industry. The manufacture of this metal, although confined to an annual production of about ten tons, is fully as interesting and ingenious as that which places aluminium within the reach of the industrial and scientific world. In this connection mention should be made of his exhaustive researches, chiefly in company with Débray, on the metals of the platinum group (1859—1862), in the course of which he succeeded for the first time in fusing large quantities of platinum by means of the oxyhydrogen blowpipe. The phenomena accompanying the high temperatures so all-important in the metallurgical operations just alluded to, gradually assumed a leading place amongst the subjects of Deville's researches. After successfully devising lamps and furnaces by means of which a high degree of heat was attainable, and methods by which the temperature could be measured, he proceeded to study a variety of reactions taking place at temperatures scarcely reached before his time. First among the results obtained in this direction reference should be made to the variety of crystallised minerals prepared artificially, such as willemite, greenockite, zircon, periclase, staurolite, &c. This branch of research has been so ably followed up by scholars of Deville, that but few natural minerals exist nowadays of which artificial counterparts have not been prepared. Of much greater importance were the numerous determinations of the vapour densities of bodies which are ordinarily solid, such as the chlorides of aluminium, of iron, and of various rare metals, by means of which the molecular weights of numerous compounds have been satisfactorily obtained. By far the most important of Deville's thermal investigations, those which have rendered the grandest services to theoretical chemistry, are connected with his noted discovery of the principle of dissociation in 1857. This principle, which explains a variety of hitherto anomalous occurrences among thermal phenomena, may briefly be considered as the property of many compound bodies to undergo partial decomposition under the influence of heat in confined spaces, until the liberated gas or vapour has attained a certain tension greater or less according to the temperature. So long as this temperature remains constant, no further decomposition takes place, neither does any portion of the separated elements recombine. If the temperature be raised decomposition recommences, and continues until a higher tension of the liberated gas or vapour, definite for that particular temperature, is attained. If the temperature falls, recomposition ensues, until the tension of the residual gas is reduced to that which corresponds with the lower temperature. The enunciation of this simple, but far-reaching principle has thrown light upon a number of phenomena, such as the formation of minerals, the apparent volatilisation of solids, &c., and has been the fruitful source of countless novel discoveries.

The number of different subjects touched upon by Deville during his long career of investigation, has been so great that we are forced to simply allude in conclusion to several notable researches, such as that on boron in company with Wöhler (1857), preparation of silicium, and its compounds with copper (1863), a new calorimeter, and the changes attendant upon the mixture of liquids (1870), the examination of a large variety of minerals and natural products, &c.

In reviewing the lifework of Sainte-Claire Deville, we are struck constantly by the predominance of one quality—that of simplicity; a quality so eminently characteristic of the man in his social relations, as well as in his scientific labours, that perhaps no phrase could describe him better than that of the French Bunsen. Like his great fellow-worker across the Rhine, he has been able to find abundant material for the exercise of his genius in attacking the still unsolved problems of inorganic chemistry; like him also he has held himself aloof in a great measure from the polemics prevalent in the modern school of chemists; the same charming simplicity characterises his apparatus, his methods, the few fundamental principles he has enunciated. As a professor Deville was deeply beloved by his students, to whom he was in turn greatly devoted; responding readily to all demands on his time and thought, and making use of his vast influence to further the interests of those who evinced special merit. His proverbial tenderness towards trembling candidates in the public examinations rendered him eminently popular in student circles.

"Voyons, Monsieur, de quoi est composée l'eau? . . . d'O?"

"Xygène," repondait l'élève.

"Et encore? . . . d'hy . . . ?"

"Drogène," ajoutait le candidat.

"C'est cela, Monsieur, merci!"

Sainte-Claire Deville was elected a member of the French Academy in 1861. A year before he had been elected a honorary member of the Chemical Society of London. He was the recipient of numerous other marks of recognition from foreign societies and governments. A few years since he received the commission of preparing the normal international metre measure, a task which brought upon him much labour. While holding aloof from politics, Deville was highly regarded in the business

world, and was a director in the Parisian Gas Company and the Eastern Railway of France. His family relations were singularly happy. He leaves behind him a group of five sons. In addition to the treatise on aluminium already alluded to, Deville was the author, in company with Débray, of an exhaustive work in two volumes on the "Métallurgie du Platine" (1863).

T. H. N.

CONVERSAZIONE AT KING'S COLLEGE

On Saturday, July 2, a brilliant and successful *conversazione*, given by the Council and the Academical Staff of King's College, brought to a conclusion the celebration of the fiftieth anniversary of the opening of the College. In the afternoon H.R.H. the Prince of Wales, accompanied by H.R.H. the Princess of Wales, distributed the College prizes to successful students, and the College rooms were converted into tastefully decorated drawing-rooms and picture galleries, in which were exhibited many very choice pictures and works of art.

The library was furnished with microscopes which had been lent by members of the Microscopic Society. The large entrance hall and the front of the College were brilliantly lighted by three Crompton electric lights, which burnt with remarkable steadiness throughout the evening. In the scientific department, the museum of King George III. contains an unrivalled collection of mechanical and physical apparatus, and is especially rich in apparatus of historic interest. The nucleus of the collection was presented to the College by Her Majesty the Queen in 1843, when the museum was opened by Prince Albert, who then witnessed some of the experiments of Sir Charles Wheatstone on the electric telegraph. Important additions have been made to the collection of apparatus by the Professors of Natural Philosophy, and at his death Sir Charles Wheatstone's valuable collection was bequeathed to the museum. Among the interesting features in the museum are: calculating machines of Cavendish and others, Appoldie centrifugal pump, Newcomen's model of his steam-engine, original forms of Daniell's battery, Siberian loadstone used for his induction spark by Faraday, original Wheatstone's bridge, early forms of stereoscope, early forms of electrical machines, polar clocks and shadow clocks, De Kempelen's talking machine.

From its foundation in 1868 the Physical Laboratory, now called the Wheatstone Laboratory, has been under the direction of Prof. W. Grylls Adams. Among the interesting apparatus exhibited in this department were the Wheatstone Collection of electrical apparatus for exhibition in Paris, dynamo-electric machines, diffraction spectra, an optical bench, showing interference of light, measuring polariscopes, with universal motions for the exact measurement of crystals, and vacuum tubes in great variety, including a very beautiful coronet. The great event of the evening in the Physical Department was the exhibition for the first time in England of the Faure's secondary battery or reservoir of electricity. Two boxes of this battery, which had been previously charged from a dynamo-electric machine, and had then been brought to the College, were capable of heating and keeping heated to bright redness a platinum wire 2 metres long and 1 millimetre in diameter. Six boxes were found to be sufficient to cause Swan electric lamps to glow brilliantly. Twelve of these boxes supplied a pedestal of Lane-Fox lamps, supplied by the British Electric Light Company, and during the evening the Physical Lecture Theatre was brilliantly illuminated by twenty Swan lamps of the latest type with the current from twelve other boxes of Faure's secondary battery. It was shown that by means of these boxes of electricity the lighting of private houses by electricity was already an accomplished fact.

THE COMET

We have received the following communications:—

At about 11h. 0m. G.M.T. on June 29 a transit of the "following" nuclear jet of the great comet over a star of 8m. was observed by Mr. N. E. Green, of 39, Circus Road, St. John's Wood, and by me, with a 12½-inch reflector belonging to Mr. Green. Definition was very good and tranquil. As the star became involved in the jet it gradually increased in size, and when seen through the brightest part of the jet traversed resembled an ill-defined planetary disk about 3″ in diameter. At this moment the comet seemed to have two nuclei similar in aspect and brightness.

The effect of the cometary matter on the star's image resembled that of ground glass, not that of fog; the image of the star, being dilated into a patch of nearly uniform brightness, instead of presenting a sharp central point with a surrounding halo. Cirro-stratus, passing into rain-cloud, produces on the appearance of the sun an effect the counterpart of that produced by the cometary emitted matter on the star. There was not sufficient light for the use of the spectroscope, the star, afterwards identified as B.D. + 65°, 519, being fainter than 8m.

The transit of the jet occupied about 3m., and the star slowly resumed its ordinary appearance and dimensions, the image *contracting* as the centre of the jet left the star behind. A transit of this kind has not frequently been observed, at least under such favourable conditions as to brightness and definition of the objects, and it is to be hoped that others may have been as fortunate as Mr. Green and the undersigned.

If the point which obeys the Newtonian law be a solid body, the observation just recorded seems to show that its true outline would probably be rendered unrecognisable, and its aspect totally altered by the (refractive?) power of the coma and jets. CHARLES E. BURTON

38, Barclay Road, S.W., July 1

The following is an extended list of places obtained with the transit-circle when the comet passed *sub Polo*:—

Date.	Greenwich Mean Time of observation. h. m. s.	Observed R.A. h m. s	Observed North Polar distance (uncorrected for parallax) ° ′ ″
(*a*)[1] June 23	11 30 54·4	5 34 55·2	44 53 20·6
(*b*) ,, 24	11 30 42·6	5 38 39·9	40 35 33·7
(*c*) ,, 25	11 30 58·3	5 42 52·2	36 38 27·4
(*d*) ,, 27	11 33 2·8	5 52 50·2	29 46 5·8
(*e*) ,, 28	—	—	26 49 45 0
(*f*) ,, 29	11 37 39·3	6 5 20·5	24 11 37·9
(*g*) ,, 30	11 41 3·9	6 12 42·2	21 50 26·3
(*h*) July 1	11 45 19·9	6 20 55·5	19 44 41·3
(*i*) ,, 2	11 50 31·9	6 30 4·9	17 52 59·6

Remarks.—(*a*) The nucleus distinct but nebulous. Tail bright, and estimated 15° in length. Observation good.

(*b*) Observation difficult, owing to cloud.

(*c*) Nucleus better defined than on June 23, but not so bright. Length of tail estimated at 15°. Observation good.

(*d*) Observation fair, very cloudy. Tail 12°-15° long.

(*e*) Observed through short break in clouds. Tail 10° in length.

(*f*) Observation very good. Tail 10°.

(*g*) Observation very good. Nucleus smaller and fainter than on preceding nights. Tail 10°.

(*h*) Observation very good. Tail 9°.

(*i*) Very faint, observed through haze. Tail 8°.

Radcliffe Observatory, Oxford E. J. STONE

My chief object in writing to-day is to explain a word in my letter of June 28 (p. 200) that is quite open to misinterpretation. In examining the head of Comet *b* 1881 with a small direct-vision spectroscope and a narrow slit, I saw, on June 27, three bright lines or bands on a faint continuous spectrum. Two of the lines were strong and

[1] The observed R.A. and G.M.T. for June 23, reported in last week's NATURE (p. 200), should be *decreased* one minute, as above.

near together, and of a bluish-green; the third was much fainter and with very little apparent colour, but easily seen as a bright line. I called these "three green lines," as that was the general appearance in the field of view, and I had no intention of fixing the positions of the lines. The words however require explanation, as they would naturally be understood as restricting the bands to a definite part of the spectrum. On July 1, shortly before midnight, I examined the position of these lines more carefully, keeping the slit sufficiently open to secure a fairly strong continuous spectrum from the nucleus in the centre of the field, whilst the bright lines extended along the whole length of the slit. I could then see clearly that the two strong bands were in the green and blue, and that the fainter line was almost at the extremity of the more refrangible end of the spectrum visible in the small spectroscope used, and would therefore be situated in the violet or purple.

The comet on the night of July 1 was very much diminished in brilliancy, but at midnight it could still be traced by aid of a binocular through at least 7°. The nucleus shone as a 2·3 magnitude star, and under a considerable magnifying power it was found to have lost most of the interesting features of June 27. The double envelope, so well defined in outline a few days previous, had disappeared, and there now remained only the bright nucleus bounded towards the tail by two arcs, one on either side of the centre, from which those rays seemed to spring which moved slightly in the direction of the sun, and then bent round to help in forming the tail. A mass of light surrounded the head, but this faded away gradually into a nebulous outline. The light from the tail diminished very rapidly as the distance from the head increased. A *sub Polo* transit of the centre of the nucleus gave, for July 1, 11h. 50m. 7s. G.M.T., the following position uncorrected for parallax and aberration:—

R.A. 6h. 20m. 53s.·51; N. Decl. 70° 14′ 53″·7.

S. J. PERRY

Stonyhurst Observatory, Whalley, July 4

I INCLOSE three drawings of the comet made on

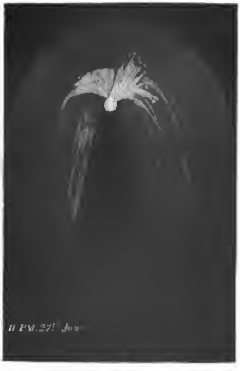


FIG. 1.

the 27th, 29th, and 30th ult. On the 27th the bright jet extending from the nucleus was very plain, and gave the hydrocarbon spectrum very distinctly. No bands were seen in the tail, but only in the immediate proximity of the nucleus. On the 29th the comet was much fainter;

FIG. 2.—A = small star.

the bright jet had disappeared, giving place to a fan, of which the left-hand side was the brighter. A small star was seen through the coma, *a*, which the comet rapidly passed.

FIG. 3.

Last night (30th) the fan-shaped projection had narrowed considerably and apparently extended farther from the nucleus.

A. PERCY SMITH

Temple Observatory, Rugby, July 1

I HAVE made some sketches of this comet, and have taken some photographs with the 3-foot reflector. Particulars of the latter may be useful.

On the night of June 24 the comet, which was very brilliant, presented the appearance of Fig. 1; the nucleus very bright and some 6″ in diameter, and not in the centre of head. Photographs with two minutes' exposure gave a decided impression on the gelatine dry-plate; with twenty-one minutes' exposure the image was very dense,

Fig. 1.—June 24, 12h. to 13h.

and the small bright tail that proceeded from the nucleus comes out well, but owing to the rapid motion in declination the image of the nucleus appears as a trail some quarter of an inch long.

On June 25 the appearance of the comet was altered, the club-footed mass of light had disappeared, and the nucleus presented a rayed appearance.

Photographs were taken with similar results to those obtained on the 24th, but a dense image of the nucleus

Fig. 2.—June 25, 13h. 1m.

was got with one minute's exposure. The intensity of light must quite equal that of a seventh magnitude star. The small bright tail was still very apparent, but between it and the edge of the large tail proper there was a decided dark space on the *p* side. At 13h. 35m. the *f* side was noted to be much the brightest; this change must have taken place very suddenly, as it had been specially noted just before as being the faintest side of the tail. Fig. 2 was taken before this was noticed. Cloudy nights intervened till the 29th. I had in the meantime fitted a fine screw to the plate-holder, and found that by giving this screw a certain calculated part of a turn every half minute for twenty minutes, I got a fair negative (I beg to forward this for your inspection) without any of the distortion caused by the motion in declination.

Fig. 3.—June 29, 13h. 2m.

The comet was observed to have changed to a much more symmetrical form (see Fig. 3). The conditions under which the photographs were taken were not very favourable: the mirror and flat were not at the best as regards polish, and the plates were about two years old.

A. AINSLIE COMMON

THE comet engaged the attention of the Paris Academy of Sciences at their sitting last Monday week, and we give the following extracts from the papers communicated.

Admiral Mouchez writes as follows:—"This comet, which was observed for the first time seventy-four years ago by an Italian monk on September 9, 1807, was observed by Pons eleven days afterwards at Marseilles on the 20th, and remained visible till March 27, 1808; during this long period it was possible to make a large number of observations, so that Bessel could for the first time calculate its elements; and he found that the period of its revolution must be comprised between 1404 and 2157 years, and was probably 1714 years. The calculations being revised and account taken of new perturbations, a period of 174 years was arrived at. The observations made during its second appearance will doubtless render it possible to determine the causes of perturbations or the errors of calculation and observation which have rendered its present return so unexpected.

"M. Tisserand has called my attention to a comet, not catalogued, but cited in Struyck's work, 'Vervolg van de Beschryving der Staatsterren' (Amsterdam, 1753), which appears to have been seen at the Cape of Good Hope in 1733, just seventy-four years previous to 1807; the want of precise observations, doubtless, did not allow of calculating the elements; but the identity of the period and the comet's appearance in the southern hemisphere lead us to suppose that it is the same comet as we observe now, and which, from some cause difficult to conceive, seems not to have been observed in Europe after its passage through perihelion. Perhaps the Dutch, to whom the Cape of Good Hope then belonged, will find in their archives some documents which will enable us to utilise this old observation, to which I have invited the attention of M. Oudemans, the learned and able astronomer of Utrecht."

M. Bigourdan says he first perceived the comet on June 22 at 13h. 30m. The following elements are deduced from the observation at Kiel (where the comet was seen two hours earlier than at Paris) on June 22, and the two following at Paris:—

1881.	Paris Mean Time. h. m. s.	Apparent Right Ascension. h. m. s.	Apparent Declination. ° ′ ″
June 24 ...	9 51 26·0 ...	5 38 21·84 ...	+49 5 31·6
26 ...	10 46 5·8 ..	5 47 22·66 ...	+56 50 2·4

Perihelion passage, 1881, June 16·52806

$$\left.\begin{aligned} \varpi &= 265^\circ\ 22'\ 59'' \\ \Omega &= 270\ \ 57\ \ 51 \\ i &= 63\ \ 26\ \ 57 \end{aligned}\right\}\ \text{Mean equin. 1881·0.}$$

$$\log. q = \bar{1}\text{·}866099$$

Representation of mean observation

In longitude ... ... $(o - C) \cos \beta = -7''\text{·}7$
In latitude ... ... $o - C = -4''\text{·}3$

The last elements obtained by Bessel for the great comet of 1807 are as follows:—

Perihelion passage, 1807, September 18·74537 mean Paris time.

$$\left.\begin{aligned} \varpi &= 270^\circ\ 54'\ 42'' \\ \Omega &= 266\ \ 47\ \ 11 \\ i &= 63\ \ 10\ \ 28 \end{aligned}\right\}\ \text{Mean equin. 1807.}$$

$$\log. q = \bar{1}\text{·}810{,}3158$$
$$e = 0\text{·}995{,}4878$$

With regard to the physical constitution of the comet, M. Wolf points out that while Coggia's comet (1874)—the only large comet visible on the horizon of Paris since spectrum analysis came into use—was at first telescopic, developed rapidly, and disappeared at the most interesting stage, the present comet comes to us already very much developed after its passage through perihelion. The transformations of the nucleus and its envelopes are extremely rapid (as the drawings show). In the large telescope the segmentation of the head, which Bond found in Donati's comet, was distinctly visible on June 24; the smallest instruments did not show it.

"The new comet represents, then, the second period of development of one of these curious stars, of which we have the first only in Coggia's comet. Its study enables us to follow the transformations of the envelopes, and to complete what information the comet of 1874 supplied.

"From the standpoint of spectrum analysis we may now correct a premature conclusion which might be deduced from our observations of Coggia's comet in 1874. That comet, from May 19, presented the continuous and nearly linear spectrum of the nucleus, traversed by the three bright bands characteristic of the light of comets (which I have found in more than a dozen of these stars). But on July 13, the evening of the last observation possible, the three bands had nearly disappeared, while the spectrum of the nucleus was become much brighter.

"Must we therefore conclude that the incandescent gas, carburetted hydrogen or other, to which these bands are due, disappear as the comet is developed, giving place to the light, proper or borrowed, of the nucleus? The observation of the new comet elucidates this. It rises rapidly from the horizon, in the same region of the sky where Coggia's comet descended to disappear, too quickly, below the horizon. Now on June 24 its spectrum, observed with the same instrument as was used in 1874, was reduced nearly to a continuous ribbon given by the nucleus; the nebulosity only gave a broad and very pale band, well terminated on the more refrangible side, diffuse on the other; the other bands of comets did not exist, or at least one could only suspect their existence in the neighbourhood of the nucleus. But yesterday (June 26) the comet was already far from the horizon, and when the sky was pure the three bright bands appeared with great distinctness. The green band especially was bright, longer than the two others, and distinctly limited on the less refrangible side (wave-length 516). On this side it seemed bordered by a dark space, as in the spectrum of Coggia's camet. As in the latter the red is the only colour pretty visible in the spectrum of the nucleus, and it is slightly dilated. The ulterior observations will show whether these bands will continue to develop. We are put on our guard, in any case, against the effect resulting from difference of altitude of the comet.

"The total quantity of light given by the head of the comet is considerable, and many persons have tried to compare it to a star of the first magnitude. In reality its intrinsic brightness is very slight. I had occasion last night, by slightly displacing the telescope, to look at the spectrum of a star of fifth or sixth magnitude; the line of light produced was at least as bright as the spectrum of the nucleus."

Admiral Mouchez having put at M. Thollon's disposal the 14-in. equatorial of the Observatory, the latter made some spectroscopic observations of the comet on the nights of June 24, 25, and 26, with the following results:—

"The nucleus of the comet gives a pretty bright continuous spectrum, on which one can distinguish neither bands nor lines. The nebulosity surrounding the nucleus gives three bands which are detached on a continuous spectrum. One of them is very visible; the others are faint. Their position has been measured with great care. The measurements, repeated a large number of times, are more concordant than I could have hoped.

"The spectrum of bands furnished by the comet so resembles that given by the blue spirit flame that I consider them identical. This identity does not result merely from the aspect of the bands and their ratios of intensity, but also from their absolute position. The spectrum of the comet is, then, the spectrum of carbon or of one of its compounds. The sole difference I have met with is that the violet band given by alcohol is not seen in the spectrum of the comet; the absorption of the atmosphere suffices to account for this difference." M. Thollon is making further observations.

NOTES

THE "Chelini-Memorial" volume takes the form of "Collectanea Mathematica," and is issued under the joint editorship of Professors Cremona and Beltrami (U. Hoepli, Milan). It contains thirty papers by twenty-eight sufficiently representative mathematicians, of whom sixteen are well-known Italian writers; of the remaining twelve, five (MM. Geiser, Kronecker, Reye, Schläfli, and Wolf) write in German, four (MM. Borchardt, Darboux, Hermite, and Mannheim) write in French. Of the three English contributors, Messrs. Cayley (on a differential equation), Hirst (on the complexes generated by two correlative planes) write in English, and Prof. H. J. S. Smith discourses in Latin "de fractionibus quibusdam continuis." There is a likeness of Chelini.

THE Government have appointed the Earl of Crawford and Balcarres Chief Commissioner, and Sir Charles T. Bright, Prof. D. E. Hughes, F.R.S., and Lieut.-Col. C. E. Webber, R.E., as Commissioners at the forthcoming Electrical Exhibition and Congress at Paris.

STUDENTS of Cretaceous geology will regret to hear that Griffiths, the well-known "fossil man" of Folkestone, has been disabled for many months by rheumatism, brought on by constant exposure during the past twenty-five years, in which he has daily extracted from the wet and slippery tract of Gault clay in Eastweir Bay the remarkable series of mollusca with their pearly nacre preserved, plants, corals, crustacea, and reptilian remains that ornament not only the private collection of those who make

the Gault a subject of special study, but the national museums both of this country and of the New World. In addition to collecting by far the most perfect specimens of the Gault fauna and flora hitherto obtained, Griffiths has rendered an important service to science in carefully noting the bed or horizon from which each specimen was procured, which identification has formed the groundwork of the divisions which English geologists have been able to make in the Gault, and the correlation of these zones by M. Barrois and others with deposits occurring on the Continent. In consideration of these results, carried out by a working man under the difficulties of a struggle for life with circumstances, and the rigorous weather of the English Channel coast, it has been thought advisable to appeal to English geologists to raise a small fund which should render it unnecessary for work to be carried on when dangerous to health, and to tide him over present difficulties; towards this end a committee has been formed, consisting of Mr. W. Topley, F.G.S., Mr. F. G. H. Price, F.G.S., Prof. Boyd Dawkins, M.A., F.R.S., and Mr. C. E. De Rance, F.G.S., with Mr. F. G. H. Price of Messrs. Childs' Bank, Temple Bar, as treasurer, who will gladly receive subscriptions.

ACCORDING to annual custom, the specimens added to the Museum of the College of Surgeons are now exhibited in the Council Room of the College, and will remain for inspection until the 13th inst., when they will be distributed in their proper places in the Museum. The number of additions, both to the Pathological and Physiological series, appears to be unusually large. Among the former we notice a novel feature in a collection illustrating vegetable pathology and teratology, prepared by Mr. S. S. Thattock; also a fine series showing the characteristic lesions produced by Indian dysentery, presented by Sir Joseph Fayrer. To the physiological, or rather zootomical, series the inhabitants of the Zoological Society's Gardens have yielded their usual quota of mortal remains, and almost every portion of the internal anatomy of the manatee, the external appearance of which was so familiar, during seventeen months, to the visitors of the Brighton Aquarium, can now be seen, neatly dissected and displayed in spirit. There are also some very beautiful preparations of human anatomy. Among the most striking objects shown in the osteological collection are a magnificent skull of a sea-elephant and a fine articulated skeleton of a sea-lion, both of which were obtained for the museum by the secretary to the Falkland Islands Company, Mr. F. Coleman. A series of skulls and skeletons of Veddahs, the aboriginal inhabitants of Ceylon, have been contributed by Mr. W. R. Kynsey. It is mentioned in Prof. Flower's report that the whole of the Barnard Davis collection, which numbers 1630 specimens, mostly crania, have been cleaned, arranged in the museum, and re catalogued during the year, and are now available for study. The report also refers to the appointment of an additional assistant curator, having special duties in the pathological department of the museum.

THE Anniversary Meeting of the Sanitary Institute of Great Britain will be held at the Royal Institution, Albemarle Street, on Thursday, July 14, at 3 p.m. An address will be delivered by the Chairman of Council, Prof. S. F. B. F. De Chaumont, M.D., F.R.S., entitled, "Modern Sanitary Science;" and the medals and certificates awarded to the successful exhibitors at the Exhibition at Exeter in 1880 will be presented.

AT a meeting of the joint committee of the Edinburgh Town Council, the Highland Society, and the Scotch Fisheries Improvement Association, held in Edinburgh on the 29th ult., a strong opinion was expressed in favour of the proposal to hold a Fisheries Exhibition in Edinburgh, of making it an international exhibition open to all countries, and of having it, if possible, in the Waverley Market, in April next year. An Executive Committee was appointed.

THE University College, Nottingham, was opened on Thursday last, July 30, by His Royal Highness the Duke of Albany, in a brief ceremony. At a luncheon afterwards, given in the Albert Hall, the Duke made a thoughtful speech on the nature and aims of the Institution. We hope to return to the subject.

BELGIUM (according to *L'Électricité*) will take a considerable share in the forthcoming Paris Exhibition. The number of exhibitors is over a hundred. Among other exhibits the Jaspar regulator and the *lampe-soleil* of MM. Clerc and Bureau will compete prominently with the numerous other systems of electric lighting. Of telephone-specialists M. de Locht-Labye will show his pan-telephone in action, and M. Navez's researches will doubtless receive due attention. Meteorological instruments will be specially represented by those of M. Van Rysselberghe, with which the indications of a meteorograph at a distance are registered directly at Brussels Observatory. Col. Leboulangé will exhibit ballistic apparatus of special type, and his ingenious dromometer and dromoscope for controlling the velocity of trains, especially at dangerous points. Various kinds of telegraph wire will be shown, and a special interest will attach to the wires of phosphor bronze from the works of M. Montefiore Lévy; these wires have a conductivity four times that of iron, and their tenacity being also much greater, lines may be made in which the wire section is greatly reduced. An official and special catalogue is being prepared for the Belgian section it will comprise an introductory notice by M. Bonneux on electrical science and industry in that country.

A PRIVATE visit was paid on Saturday last to the Channel tunnel experimental works by Sir E. Watkin, M.P. (chairman of the South-Eastern Railway Company), and a large party of scientific and other gentlemen. Very satisfactory progress was found to have been made at Abbot's Cliff since the last visit. The heading has now been advanced a total length of nearly half a mile. The tunnel is kept well free of water, and a good average rate of work is maintained. The work at the new shaft at Shakespeare's Cliff promises to be even more satisfactory. A very superior boring machine is used, and a more powerful engine is being fitted up to drive it.

THE first general meeting of the Society of Chemical Industry was held on the 28th ult. in the hall of the Institute of Civil Engineers, Prof. Roscoe presiding. After the President's address papers were read on "Recent Legislation on Noxious Gases," by Mr. E. K. Muspratt; "The Brewing of Lager Beer," by Prof. C. Graham; and "Mechanical Furnaces," by Mr. James Mactear. This promising Society already numbers 300 members.

THE Council of the University of Dublin have nominated Valentine Ball, M.A., of the Geological Survey of India, to the Professorship of Geology in the University of Dublin; this nomination however requires to be confirmed by a vote of the Board of Trinity College, Dublin. There were seven candidates.

M. W. DE FONVIELLE, editor of *L'Électricité*, and M. Lippmann, one of his contributors, made a balloon ascent on July 2, shortly after midnight. The descent took place near Rambouillet at a quarter past five, the distance traversed being 48 kilomètres. The balloonists carried with them a small Planté accumulator with a special safety electric lamp constructed by Trouvé, composed of a platinum wire inclosed in a glass tube. While the apparatus did not weigh more than 1 kilog. it gave sufficient light for reading the barometer and thermometer, and writing notes with accuracy. A special luminous compass for aëronauts will be constructed on this plan and sent to the Exhibition of Electricity.

THE number of Chinese in the United States is now proved by the census to be very much less than has been commonly sup-

posed. It is only 105,717 (California possessing 75,122). It is true that the numbers have nearly doubled within the last ten years, but even at that rate they are not of a nature to cause any alarm such as appears to have been felt in some quarters. In the Eastern States the Chinese element is quite inappreciable among the foreign elements of population; New York contains but 942, Massachusetts 256, Illinois 214, &c.

THE scheme, earnestly advocated by the late Sir Thomas Moncreiffe, for providing Perth and Perthshire with a satisfactory natural history museum is now being realised, a handsome building in the Scottish baronial style having been built in South Tay Street with the funds provided. The ground floor contains a lecture-room, library, and laboratory or work-room, and the museum occupies the upper part of the building. To the rear a piece of ground has been secured on which additions more than doubling the accommodation could be built, but meantime the ground is to be used as a garden, in which all the more notable Perthshire plants will be grown. Access from the building can be easily obtained to two much larger lecture-halls than that in the museum, if necessary. The museum is to be strictly confined to the natural history, botany, and geology of Perthshire, excepting a small type collection, and should the project be rightly carried out (by the Perthshire Society of Natural Science) one of the most interesting and valuable local collections should thus be formed. The cost of the building (which is described in the current number of the *Scottish Naturalist*) has been upwards of 1,700*l*., all of which has been subscribed. A further amount is required for furnishing, &c., and for this end a bazaar is to be held about the end of the year.

THE Literary and Antiquarian Society of Perth propose an extension of their Museum in Perth, the only one hitherto existing in the county, by building, at an estimated cost of 3000*l*., a hall behind the present museum, mainly to accommodate the zoological collection (which comprises some 800 vertebrate and 2000 invertebrate forms). It is designed to present in this room a gradational view of animal life. A bazaar in aid of the proposed extension will be held on October 5 and 7. Subscriptions and donations may be sent to Mr. D. Hepburn, solicitor, 12, Charlotte Street, Perth, or to Dr. Bower, R.N., Montreal College, Perth.

AT the concluding meeting of the session of the Geologists' Association on Friday, July 1, a costly timepiece and ornaments were presented to Mr. J. Logan Lobley with the following address:—"The accompanying timepiece and ornaments are presented by the members of the Geologists' Association of London to their treasurer, J. Logan Lobley, Esq., F.G.S., F.R.G.S., in recognition of the valuable services he has rendered to the Association as Honorary Secretary, 1871–74, and Honorary Editor, 1871–81, and of the active interest he has always taken in its welfare and progress."

A BRANCH of the Baturite railway in Brazil has a gradient, which is probably the steepest in the world worked with a locomotive acting by simple adherence. This gradient is about 10 per cent., or 90 to 100 millimetres per metre. The line (described in last week's *La Nature*) is of narrow gauge, and extends from the port of Alfandega on the Atlantic to the town of Fortaleza, about 2 kilometres distance. The locomotive is from the Baldwin works in the United States; it has an adherent weight of 20,000 kilograms, and draws three loaded goods waggons or a single passenger car of the American type at a velocity of 20 kilom. per hour. By always limiting the weight to be drawn to an amount considerably under that of the engine, the regularity of the service on this line has been ensured during the two years it has been in use.

THE first part of a fourth edition of Griffith and Henfrey's useful Micrographic Dictionary has appeared. It is expected that the issue will be completed in twenty-one of these monthly parts, which will include important additions representing recent scientific progress. This work is known to aim especially at helping the microscopic observer to discover what any object is which may be presented to him, and by the aid of the Bibliography to refer to more extensive treatises for further details. A system is also adopted by which one is guided to a general knowledge of particular departments of science. There is an introduction on the use of the microscope. Dr. Griffith is assisted in the editing by the Rev. Mr. Berkeley and Prof. Rupert Jones.

WE have received an excellent specimen number of a new French engineering journal entitled *L'Ingénieur*. The proprietors have acquired the right of reproduction, in France, of articles from our contemporary, *Engineering*, of which articles the new paper will largely consist.

M. FERDINAND DE LESSEPS has been elected president of the Société de Géographie of Paris.

THERE was recently landed at Marseilles a magnificent zebra which the King of Choa, Menelick II., has sent as a present to the President of the French Republic. This zebra, called the *Semaphore*, has been brought from Abyssinia by two Marseillais. The Société de Géographie, to which it was addressed from Aden, has intrusted it to the Marseilles Zoological Garden.

THE evening *fête* of the Royal Horticultural Society was held on the 28th ult. in the Gardens at South Kensington. Coloured lamps were disposed about the lawns, and here and there the cool plash of fountains was to be heard. The Siemens and Maxim electric lights were placed in the upper part of the Gardens, and in the lower part were two tents illuminated by the Brush electric light, and containing the plants of a flower-show, which continued next day. Brilliant effects were obtained with coloured fires behind the trees and the spray of the fountains.

THE Berwickshire Naturalists' Club commemorated the fiftieth year of its existence on the 29th ult. by a meeting at Grant's House, largely attended by members. Excursions were made to different places of interest in the locality, and before dinner Mr. James Hardy, joint secretary, was presented with a valuable microscope and 110*l*. in recognition of his long and arduous services. The Rev. Thomas Brown, Edinburgh, one of the oldest members of the club, presided.

FROM the *Colonies and India* we learn that the Meteorological Conference lately held at Sydney has agreed to a division of Australia into meteorological districts or aspects, to form the basis of weather telegrams and warnings. A cipher code has been arranged for weather telegrams to New Zealand, and the Queensland Government is to be asked to co-operate in the matter.

FROM an approximate summary of this year's census of Victoria, which has just been received from Mr. H. H. Hayter, the Government statist, it appears that the total population of the colony, including Chinese and Aborigines, is now 855,796, against 731,528 in 1871. The Chinese number 11,796, and the aborigines 768, the former showing a decrease of 6299 and the latter of 562.

THE additions to the Zoological Society's Gardens during the past week include a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from India, presented by Lieut. W. V. Anson, R.N.; a Daubenton's Curassow (*Crax daubentoni*) from Venezuela, a Hawk's-billed Turtle (*Chelone imbricata*) from the East Indies, presented by Capt. King; a Rough Terrapin (*Clemmys punctularia*) from Trinidad, presented by Mr. Lachmere Guppy; ten Green Turtles (*Chelone viridis*) from Ascension presented by

Messrs. Weil Brothers; two Yellow Snakes (*Chilobothrus inornatus*) from Jamaica, presented by Mr. Chas. B. Masse; a Squirrel Monkey (*Chrysothrix sciurea*) from Demerara, a Military Macaw (*Ara militaris*) from South America, deposited; a Wapiti Deer (*Cervus canadensis*), two Hybrid Paradoxures (between *Paradoxurus leucomystax* and *P. stigmaticus*), born in the Gardens.

GEOGRAPHICAL NOTES

THE Geographical Society's *Proceedings* this month are chiefly occupied with the anniversary meeting at the end of May, and everything said and done on that occasion seems to have been carefully recorded. The only paper given is that by Mr. Minchin on Eastern Bolivia and the Gran Chaco, and it is illustrated by one of the best maps which the Society has published for some time. The geographical notes supply intelligence of matters which have not hitherto attracted notice in this country, though one at least is of considerable importance. We allude to the recent exploration of the Beni River by Dr. Heath of Wisconsin, which is a distinct addition to our knowledge of the Amazons' system. When fuller details, including Dr. Heath's observations for latitude and longitude, have come to hand, it will be for the first time possible to fix the precise position of the mouth of the magnificent river, best known as the Madre de Dios, which, until a few years ago, was believed by geographers to be a feeder of the Purus instead of the Madeira. Some information is also given as to the progress of exploration between the Rovuma and Lake Nyassa.

M. ABBÉ DESGODINS, who is well known for the excellent geographical work he has done in Eastern Tibet, contributes to *Les Missions Catholiques* the first part of some interesting notes on the marriage and other domestic customs of the Tibetans.

IT may be interesting to mention that in last week's number of the Society of Arts' *Journal* some useful notes are published on gums, resins, and waxes, which Mr. C. G. Warnford Lock has compiled from the journals of recent travellers. Especial prominence is given to india-rubber and the curious fossil resin known as gum copal.

M. ROUX has been intrusted by the Minister of Public Instruction and Fine Arts at Paris with a scientific mission to Tunis, and he has already begun the exploration of the region near the Constantine province of Algeria. He will afterwards undertake topographical and botanical investigations in the country between the Mejerba Valley and Cape Bon peninsula. Under the auspices of the same department M. Lantz is engaged in making natural history collections in some of the unknown parts of Madagascar.

M. BOULANGIER, a French Government engineer, has lately been engaged on a surveying expedition in Indo-China, in connection with the project for a railway. He went by a somewhat circuitous route from the frontier of French Cochin-China across Cambodia to Siam, made an especial study of the basin of the Tonlé-Sap, or Great Lake, which, according to his view, was formerly the head of the Gulf of Siam. The mountains south of Pursat must, therefore, have been an island, but the intervening low country becoming filled up they were joined to the mainland. As the result of his observations, M. Boulangier thinks that the Tonlé-sap will gradually silt up.

WE hear that Mr. Dorward, of the China Inland Mission, returned to Shanghai early in April from a five-months' journey in the province of Hunan. He is the only Protestant missionary who has ever traversed the route by which he returned from Hung kiang to the neighbourhood of the Tung-ting Lake. Mr. Dorward also paid a flying visit to Kwei-yang-fu, the capital of the Kweichow province.

A PROMINENT paragraph in the *Standard* of last Saturday states that the "Geographical Society has received some interesting details of the fate of the Wybrants [*i.e.* Capt. Phipson-Wybrants] Expedition in Mozambique." We understand that there is absolutely no foundation for this statement, and the only effect of it is to inflict cruel disappointment on the relatives of the deceased members of this unfortunate expedition, regarding whose last days detailed particulars are anxiously awaited. Whether these will ever be known is, we fear, more than doubtful. The expedition was a purely private undertaking on the part of the late Capt. Phipson-Wybrants, and though he was aided with a loan of instruments, he was in no sense sent out by the Geographical Society.

THE Brazilian Section of the Lisbon Geographical Society, which was established a short time back, has commenced the publication at Rio de Janeiro of a periodical under the title of *Revista Mensal*. Dr. F. Mendes de Almeida is the editor-in-chief.

THE Bengal Asiatic Society have issued as part of their *Journal* Mr. Longworth Dawes' sketch of the Northern Balochi language, containing a grammar, vocabulary, and specimens of the language.

CIVILISATION AND BARBARISM IN SOUTH AFRICA

AT a meeting of the Anthropological Institute on the 28th ult. Sir Bartle Frere gave a lecture treating of the results of contact of civilised with uncivilised races in South Africa. The first part of the lecture dealt with the historical results of such contact in other countries, and the lecturer, after a sketch of the recent history and present condition of the various South African races, maintained that on the whole natives have increased in numbers as well as improved in physique and in intellectual status by contact with Europeans, and that there was also little real reason to doubt an improvement in moral status. The conditions required to raise and improve races like the Kaffirs were (1) a strong imperial government; (2) freedom from slavery and equality before the law. To secure these two requisites it was necessary (3) to determine whether the standard of moral and social progress shall be that of the European or that of the native races; (4) education according to English standards. The general results arrived at in the lecture were summarised in the following propositions:—(1) It is possible for the civilised to destroy by war the savage races, to expel, or repel, or turn them aside in their migrations; (2) proximity of civilised and savage races has led or is leading to the decay and probable extinction of the Bushman race. But this result is doubtful in the case of the Hottentot races, and is certainly not taking place with regard to the Bantu or Kaffir races; (3) the changes consequent on proximity of civilised and uncivilised races are an approximation to the European type of civilisation; (4) the essentials to such approximation are (*a*) a pax Romana or Anglicana, bringing with it (*b*) protection of life and property, which involves equality before the law, individual property in land, abolition of slavery, abolition of private rights of making war and of carrying arms without the authority of the supreme ruler; (*c*) power of local legislation on European principles, with a view to secure education in the arts of civilised life, taxation sufficient for state purposes, restrictions on the use of intoxicating substances, as measures essential to the full attainment of any one of the preceding objects.

INDIGO AND ITS ARTIFICIAL PRODUCTION[1]

MORE than eleven years ago the speaker had the pleasure of bringing before this audience a discovery in synthetic chemistry of great interest and importance, viz. that of the artificial production of alizarine, the colouring substance of madder. To-day it is his privilege to point out the attainment of another equally striking case of synthesis, viz. the artificial formation of indigo. In this last instance, as in the former case, the world is indebted to German science, although to different individuals, for these interesting results, the synthesis of indigo having been achieved by Prof. Adolf Baeyer, the worthy successor of the illustrious Liebig in the University of Munich. Here then we have another proof of the fact that the study of the most intricate problems of organic chemistry, and those which appear to many to be furthest removed from any practical application, are in reality capable of yielding results having an absolute value measured by hundreds of thousands of pounds.

In proof of this assertion, it is only necessary to mention that the value of the indigo imported into this country in the year 1879 reached the enormous sum of close on two millions sterling, whilst the total production of the world is assessed at twice that amount; so that if, as is certainly not impossible, artificial indigo can be prepared at a price which will compete with the native product, a wide field is indeed open to its manufacturers.

[1] Lecture delivered at the Royal Institution, Friday, May 27, 1881, by Prof. H. E. Roscoe, LL.D., F.R.S.

Indigo, as is well known, is a colouring matter which has attracted attention from very early times. Cloth dyed with indigo has been found in the old Egyptian tombs. The method of preparing and using this colour is accurately described by both Pliny and Dioscorides, and the early inhabitants of these islands were well acquainted with indigo, which they obtained from the European indigo plant, *Isatis tinctoria*, the woad plant, or pastel. With this they dyed their garments and painted their skins. After the discovery of the passage to India by the Cape of Good Hope, the eastern indigo, derived from various species of *Indigofera*, gradually displaced woad as containing more of the colouring matter. But this was not accomplished without great opposition from the European growers of woad; and severe enactments were promulgated against the introduction of the foreign colouring matter, an edict condemning to death persons "who used that pernicious drug called devil's food," being issued by Henry the Fourth of France. The chief source of Indian indigo is the *Indigofera tinctoria*, an herbaceous plant raised from seed which is sown in either spring or autumn. The plant grows with a single stalk to a height of about three feet six inches, and about the thickness of a finger. It is usually cut for the first time in June or July, and a second or even a third cutting obtained later in the year. The value of the crop depends on the number of leaves which the plant puts forth, as it is in the leaves that the colouring principle is chiefly contained. Both the preparation of the colouring matter from the plant, and its employment as a dyeing agent, are carried on at the present day exactly as they have been for ages past. The description of the processes given by Dioscorides and Pliny tally exactly with the crude mode of manufacture carried on in Bengal at the present day.

Dioscorides says:—"Indigo used in dyeing is a purple-coloured froth formed at the top of the boiler; this is collected and dried by the manufacturer; that possessing a blue tint and being brittle is esteemed the most."

The identity of the blue colouring matter of woad and that of the Bengal plant was proved by Hellot, and by Planer and Trommsdorff at the end of the last century. These latter chemists showed that the blue colour of the woad can be sublimed, and thus obtained in the pure state, a fact which was first mentioned in the case of indigo by O'Brien in 1789, in his treatise on calico printing. Indigo thus purified is termed indigotin. It has been analysed by various chemists, who ascertained that its composition may be most simply expressed by the formula C_8H_5NO.

Concerning the origin of indigo in the leaves of the *Indigofera*, various and contradictory views have been held. Some have supposed that blue indigo exists ready formed in the plant; others, that white indigo is present, which on exposure to air is converted into indigo-blue. Schunck has, however, proved beyond doubt that the woad plant (*Isatis tinctoria*), the *Indigofera tinctoria* of India, and the Chinese and Japanese indigo plant (*Polygonum tinctorium*) contain neither indigo-blue nor white indigo ready formed. By careful treatment the leaves of all these indigo-yielding plants can be shown to contain a colourless principle termed indican, and that this easily decomposes, yielding a sugar-like body and indigo-blue. That white indigo is not present in the leaves is proved by the fact that this compound requires an alkali to be present in order to bring it into solution, whereas the sap of plants is always acid. The decomposition is represented by Schunck as follows:—

$$\underset{\text{Indican.}}{C_{26}H_{31}NO_{17}} + 2H_2O = \underset{\text{Indigotin.}}{C_8H_5NO} + \underset{\text{Indiglucin.}}{3C_6H_{10}O_6}.$$

So readily does this change from indican to indigo take place, that bruising the leaf or exposing it to great cold is sufficient to produce a blue stain. Even after mere immersion in cold alcohol or ether, when the chlorophyll has been removed the leaves appear blue, and this has been taken to show the pre-existence of indigo in the plant. But these appearances are deceptive, for Schunck has proved that if boiling alcohol or ether be used, the whole of the colour-producing body as well as the chlorophyll is removed, the leaves retaining only a faint yellow tinge, whilst the alcoholic extract contains no indigo blue, but on adding an acid to this liquid the indican is decomposed and indigo-blue is formed.

What now was the first step gained in our knowledge concerning the constitution of indigo, of which the simplest formula is C_8H_5NO?

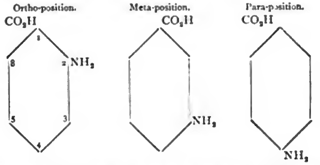
STEP No 1.—This was made so long ago as 1840, when Fritsche proved that aniline, $C_6H_5NH_2$, can be obtained from indigo. The name for this now well known substance is indeed derived from the Portuguese "anil," a word used to designate the blue colour from indigo. This result of Fritsche's is of great importance, as showing that indigo is built up from the well-known benzene ring C_6H_6, the skeleton of all the aromatic compounds, and moreover that it contains an amido group.

Step No. 2 was also made by Fritsche in the following year, when, by boiling indigo with soda and maganese dioxide, he obtained ortho-amido-benzoic acid, or, as he then termed it, anthranilic acid. The following is the reaction which here occurs:—

$$\underset{\text{Indigo.}}{C_8H_5NO} + O + 2H_2O = \underset{\text{Ortho-amido-benzoic acid.}}{C_7H_5NH_2O_2} + CH_2O_2.[1]$$

What light does this fact shed upon the constitution of indigo? It shows (1) that one of the eight atoms of carbon in indigo can be readily separated from the rest; (2) that the carboxyl and the amido-group are in neighbouring positions in the benzene ring, viz. 1 and 2. For we have three isomeric acids of the above composition.

Ortho-position. Meta-position. Para-position.



STEP No. 3.—The next advance of importance in this somewhat complicated matter is the discovery by Erdmann and Laurent independently, that indigo on oxidation yields a crystalline body, which, however, possesses no colouring power, to which they gave the name of isatin.

$$\underset{\text{Indigo.}}{C_8H_5NO} + O = \underset{\text{Isatin.}}{C_8H_5NO_2}.$$

STEP No. 4.—The reverse of this action, viz. the reduction of isatin to indigo, was accomplished by Baeyer and Emmerling in 1870 and 1878, by acting with phosphorus pentachloride on isatin, and by the reducing action of ammonium sulphide on the chloride thus formed.

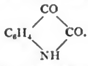
Understanding now something of the structure and of the relationships of the body which we wish to build up, let us see how this edifice has, in fact, been reared. Three processes have been successfully employed for carrying out this object. But of these three only one is of practical importance.

For the sake of completeness, let us, however, consider all three processes, although Nos. 1 and 2 are at present beyond the pale of practical schemes.

These three processes have certain points in common. (1) They all proceed from some compound containing the benzene nucleus. (2) They all start from compounds containing a nitrogen atom. (3) They all commence with an ortho-compound.

They differ from one another; inasmuch as process No. 1 starts from a compound containing seven atoms of carbon (instead of eight), and to this, therefore one more atom must be added; process No. 2, on the other hand, starts from a body which contains exactly the right number (eight) of carbon atoms; whilst No. 3 commences with a compound in which nine atoms of carbon are contained, and from which, therefore, one atom has to be abstracted before indigo can be reached.

Process No. 1 (Kekulé—Claissen and Shadwell).—So long ago as 1869 Kekulé predicted the constitution of isatin, and gave to it the formula which we now know that it possesses, viz.



Following up this view, Claissen and Shadwell, two of Kekulé's

[1] Bottinger, *Deut. Chem. Ges.* 1877, i. 269.

pupils, succeeded in preparing isatin, and, therefore, now indigo, from ortho-nitro-benzoic acid.

The following are the steps in the ascent:

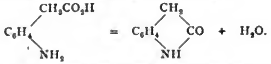
1. Ortho-nitro-benzoic acid acted on by phosphorus pentachloride yields the chloride $C_6H_4(NO_2)COCl$.
2. This latter heated with silver cyanide yields the nitril $C_6H_4(NO_2)CO.CN$.
3. On heating this with caustic potash it yields ortho-nitro-phenylglyoxylic acid, $C_6H_4(NO_2)CO.CO_2H$.
4. This is converted by nascent hydrogen into the amido-compound $C_6H_4(NH_2)CO.CO_2H$.
5. And this loses water and yields isatin, $C_6H_4NH.CO.CO$. (Q. E. D.)

The reasons why this process will not work on a large scale are patent to all those who have had even bowing acquaintance with such unpleasant and costly bodies as phosphorus pentachloride or cyanogen.

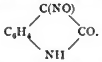
Process No. 2.—Baeyer's (1878) synthesis from ortho-nitro-phenylacetic acid.

This acid can be obtained synthetically from tolnol, and it is first converted into the amido-acid, which, like several ortho compounds, loses water, and is converted into a body called oxindol, from which isatin, and therefore indigo, can be obtained. The precise steps to be followed are:—

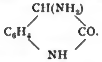
1. Ortho-amido-phenylacetic acid yields oxindol:



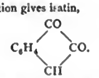
2. This on treatment with nitrous acid yields nitrosoxindol:



3. This again with nascent hydrogen gives amidoxindol:



4. Which on oxidation gives isatin,



(Q.E.D.)

This process, the feasibility of which had also been foreseen by Kekulé, is however not available as a practical scheme for various reasons.

Process No. 3.—This may be called the manufacturing process, and was also proposed by Baeyer. It starts from cinnamic acid, a substance contained in gum benzoin, balsam of Peru, and some few other aromatic bodies. These sources are, however, far too expensive to render this acid thus obtained available for manufacturing purposes. But Bertagnini, in 1856, had obtained cinnamic acid artificially from oil of bitter almonds, and other processes for the same purpose have since been carried out. Of these, that most likely to be widely adopted is the following practical modification by Dr. Caro of Mr. Perkin's beautiful synthesis of cinnamic acid:—

1. $\underset{\text{Toluene.}}{C_6H_5CH_3} + 4Cl = \underset{\text{Benzylene dichloride.}}{C_6H_5CHCl_2} + 2HCl$.

2. $\underset{\text{Benzylene dichloride.}}{C_6H_5CHCl_2} + \underset{\text{Sodium acetate.}}{2CH_3.CO.O.Na} =$
$\underset{\text{Cinnamic acid.}}{C_6H_5CH{=}CH.CO.OH} + 2NaCl$.

But why did Baeyer select this nine carbon acid from which to prepare indigo? For this he had several reasons. In the first place, it had long been known that all indigo compounds when heated with zinc dust yield indol, C_8H_7N, a body which stands therefore to indigo in the same relation as anthracene to alizarin, and Baeyer and Emmerling had so long ago as 1869 prepared this indol from ortho-nitro-cinnamic acid thus:

$$C_9H_6(NO_2)CO_2H = C_8H_7N + O_2 + CO_2.$$

Secondly, the ortho-nitro-cinnamic acid required (for we must remember that indigo is an ortho-compound and also contains nitrogen) can be readily prepared from cinnamic acid, and this itself again can be obtained on a large scale. Thirdly, this acid readily parts with one atom of carbon, and thus renders possible its conversion into eight-carbon indigo.

The next steps in the process are (3) the formation of ortho-nitro-cinnamic acid, (4) the conversion of this into its dibromide, (5) the separation from this of the two molecules of hydrobromic acid, giving rise to ortho-nitro-phenyl-propiolic acid, and (6), and lastly, the conversion of this latter into indigo by heating its alkaline solution with grape sugar, xanthate of soda, or other reducing agent. These reactions are thus represented:—

3. $\underset{\text{Cinnamic acid}}{C_6H_5CH{=}CHCOOH} \quad \text{yields} \quad \underset{\text{Ortho-nitro-cinnamic acid.}}{C_6H_4(NO_2)CH{=}CH.COOH}$.

In this process the para acid is also obtained, and as this is useless for the manufacture of indigo, it has to be removed. This is effected by converting the acids into their ethyl ethers, which, possessing different degrees of solubility, can be readily separated from one another:—

4. This is next converted into the dibromide

$$C_6H_4(NO_2)CHBr.CHBrCOOH.$$

5. And by careful treatment with caustic soda this yields ortho-nitro-phenyl-propiolic acid, thus:—

$$C_6H_4(NO_2)CHBrCHBrCOOH + 2NaOH = C_6H_4(NO_2)C_2.COOH + 2NaBr + 2H_2O.$$

6. $n[\underset{\text{Ortho-nitro-phenyl-propiolic acid.}}{C_6H_4(NO_2)C_2.COOH} + H_2 = \underset{\text{Indigotin.}}{C_8H_5NO} + CO_2 + H_2O]$.

(Q. E. D.)

The last of these reactions is in reality not so simple as the equation indicates. For only about 40 per cent. of indigo is obtained, whereas according to theory 68 per cent. should result. Indeed although, as we have seen, indigo can be prepared by these three methods, chemists are as yet in doubt as to its molecular weight, the probability being that the molecule of indigo contains twice 16 atoms of carbon, or has the formula $4(C_8H_5NO)$ or $C_{32}H_{20}N_4O_4$. Still it must be remembered that according to Sommaruga the vapour density of indigo is 9·45, a number corresponding to the simpler formula $C_{16}H_{10}N_2O_2$.

The artificial production of indigo may even now be said to be within measurable distance of commercial success, for the ortho-nitro-phenyl-propiolic acid, the colourless substance which on treatment with a reducing agent yields indigo-blue, is already in the hands of the Manchester calico printers, and is furnished by the Baden Company for alkali and aniline colours at the price of 6*s.* per lb. for a paste containing 25 per cent. of the dry acid.

With regard to the nature of the competition between the artificial and the natural colouring matters it is necessary to say a few words. In the first place, the present price at which the manufacturers are able to sell their propiolic acid is 50*s.* per kilo. But 100 parts of this can only yield, according to theory, 68·58 parts of indigo-blue, so that the price of the artificial (being 73*s.* per kilo.) is more than twice that of the pure natural colour. Hence competition with the natural dye-stuff is not to be thought of until the makers can reduce the price of dry propiolic acid to 20*s.* per kilo., and also obtain a theoretical yield from their acid. This may, or it may not, be some day accomplished, but at present it will not pay to produce indigo from nitro-phenyl-propiolic acid. Nevertheless a large field lies open in the immediate future for turning Baeyer's discovery to practical account. It is well known that a great loss of colouring matter occurs in all the processes now in use for either dyeing or printing with indigo. It has already been stated that a large percentage of indigo is lost in the "cold vats" in the sediment. Another portion is washed off and wasted after the numerons dippings, whilst in order to produce a pattern much indigo must be destroyed before it has entered into the fibre of the cloth. Moreover, the back of the piece is uselessly loaded with colour. In the processes of printing with indigo the losses are as great, or even greater, and, in addition, such considerable difficulties are met with, that only a few firms (Potter, Grafton in Manchester, and Schlieper in Elberfeld) have been successful in this

process. But a still more important fact remains, that no printing process exists in which indigo can be used in combination with other colours in the ordinary way, or without requiring some special mode of fixing after printing. Hence it is clear that the weak points of natural indigo lie in the absence of any good process for utilising the whole of its colouring matter, and in the impossibility, or at any rate great difficulty, of employing it in the ordinary madder styles of calico printing. Such were the reasons which induced the patentees to believe that although the artificial dye cannot be made at a price to compete with natural indigo for use in the ordinary dye-beck, it can even now be very largely used for styles to which the ordinary dye-stuff is inapplicable.

To begin with, Baeyer employed (Patent 1117) grape sugar as a reducing agent. The reduction in this case does not take place in the cold, and even on long standing only small traces of indigo are formed, but if heated to 70° or upwards the change takes place. Unfortunately this production of indigo-blue is rapidly followed by its reduction to indigo-white, and it is somewhat difficult in practice to stop the reaction at the right moment. But Dr. Caro of Mannheim found that sodium xanthate is free from many of the objections inherent to the glucose reduction process, inasmuch as the reaction then goes on in the cold. Moreover, he finds that the red isomeride of indigo-blue, Indirubin, which possesses a splendid red colour, but has little or no tinctorial power, is produced in less quantity in this case than when glucose is employed. On this cloth, alumina and iron mordants may be printed, and this afterwards dyed in alizarine, &c., or this colouring matter may also be printed on the cloth and the colour fixed by moderate steaming without damage to the indigo-blue. This process is now in actual use by printers both in England and on the Continent, so that, thanks especially to the talent and energy of Dr. Caro, Bayer's discovery has been practically applied within the short space of twelve months of its conception. Operations on a manufacturing scale have been successfully carried on in the Baden Soda and Aniline Works at Ludwigshafen for the last two months, and the directors see no reason why they should not be able to supply any demand, however great, which may be made for ortho-nitro-phenyl-propiolic acid.

The proper way of looking at this question at present is, therefore, to consider ortho-nitro-phenyl-propiolic acid and indigo as two distinct products not comparable with each other, inasmuch as the one can be put to uses for which the other is unfitted, and there is surely scope enough for both. Still, looking at the improvements which will every day be made in the manufacturing details, he must be a bold man who would assert the impossibility of competition with indigo in all its applications. For we must remember that we are only at the beginning of these researches in the indigo field. Baeyer and other workers will not stay their hands, and possibly other colouring matters of equal intensity and of equal stability to indigo may be obtained from other as yet unknown or unrecognised sources, and it is not improbable that these may turn out to be more formidable competitors in the race with natural indigo than ortho-nitro-phenyl-propiolic acid.

Looking at this question of the possible competition of artificial with the natural indigo from another point of view, it must, on the other hand, be borne in mind that the present mode of manufacturing indigo from the plant is extremely rude and imperfect, and that by an improved and more careful carrying out of the process, great saving in colouring matter may be effected, so that it may prove possible to produce a purer article at a lower price, and thus to counterbalance the production of the artificial material.

The potential importance, from a purely commercial point of view, of the manufacture, may be judged of by reference to the following statistics, showing that the annual value of the world's growth of indigo is no less than four millions sterling.

How far the artificial will drive out the natural colouring matter from the market cannot, as has been said, be foreseen. It is interesting, as the only instance of the kind on record, to cast a glance at the history of the production of the first of the artificial vegetable colouring matters, alizarin. In this case the increase in the quantity produced since its discovery in 1869 has been enormous, such indeed that the artificial colour has now entirely superseded the natural one, to the almost complete annihilation of the growth of madder-root. It appears that whilst for the ten years immediately preceding 1869 the average value of the annual imports of madder-root was over one million sterling,

Estimated Yearly Average of the Production of Indigo in the World, taken from the Total Crop for a Period of Ten Years.

	Pounds Weight.	Pounds Sterling.
Bengal, Tirhoot, Benares, and N. W. India	8,000,000	2,000,000
Madras and Kurpah	2,200,000	400,000
Manilla, Java, Bombay, &c.	...	500,000
Central America	2,250,000	600,000
China and elsewhere, consumed in the country ...	...	Say 500,000
		4,000,000

the imports of the same material during last year (1880) amounted only to 24,000*l.*, the whole difference being made up by the introduction of artificial alizarin. In 1868, no less a quantity than 60,000 tons of madder-root were sent into the market, this containing 600,000 kilos of pure natural alizarin. But in ten years later a quantity of artificial alizarin more than equal to the above amount was sent out from the various chemical factories. So that in ten years the artificial production had overtaken the natural growth, and the 3 or 400,000 acres of land which had hitherto been used for the growth of madder, can henceforward be better employed in growing corn or other articles of food. According to returns, for which the speaker had to thank Mr. Perkin, the estimated growth of madder in the world previous to 1869 was 90,000 tons, of the average value of 45*l.* per ton, representing a total of 4,050,000*l.*

Last year (1880) the estimated production of the artificial colouring matter was 14,000 tons, but this contains only 10 per cent. of pure alizarin. Reckoning 1 ton of the artificial colouring matter as equal to 9 tons of madder, the whole artificial product is equivalent to 126,000 tons of madder. The present value of these 14,000 tons of alizarin paste, at 122*l.* per ton, is 1,568,000*l.* That of 126,000 tons of madder at 45*l.* is 5,670,000*l.*, or a saving is effected by the use of alizarin of considerably over four millions sterling. In other words, we get our alizarin dyeing done now for less than one-third of the price which we had to pay to have it done with madder.

To Englishmen it is a somewhat mortifying reflection, that whilst the raw materials from which all these coal-tar colours are made are produced in our country, the finished and valuable colours are nearly all manufactured in Germany. The crude and inexpensive materials are, therefore, exported by us abroad, to be converted into colours having many hundred times the value, and these expensive colours have again to be bought by English dyers and calico-printers for use in our staple industries. The total annual value of manufactured coal-tar colours amounts to about three and a half millions; and as England, though furnishing all the raw material, makes herself only a small fraction of this quantity, but uses a large fraction, it is clear that she loses the profit on the manufacture. The causes of this fact, which we must acknowledge, viz., that Germany has driven England out of the field in this important branch of chemical manufacture, are probably various. In the first place, there is no doubt that much of the German success is due to the long-continued attention which their numerous universities have paid to the cultivation of Organic Chemistry as a pure science. For this is carried out with a degree of completeness, and to an extent, to which we in England are as yet strangers. Secondly, much again is to be attributed to the far more general recognition amongst German than amongst English men of business of the value, from a merely mercantile point of view, of high scientific training. In proof of this it may be mentioned that each of two of the largest German colour-works employs no less a number than from twenty-five to thirty highly-educated scientific chemists, at salaries varying from 250*l.* to 5 or 600*l.* per annum. A third cause which doubtless exerts a great influence in this matter is the English law of patents. This, in the special case of colouring matters at least, offers no protection to English patentees against foreign infringement, for when these colours are once on the goods they cannot be identified. Foreign infringers can thus lower the price so that only the patentee, if skilful, can compete against them, and no English licencees of the patent

can exist. This may to some extent account for the reluctance which English capitalists feel in embarking in the manufacture of artificial colouring matters. That England possesses both in the scientific and in the practical direction ability equal to the occasion none can doubt. But be that as it may, the whole honour of the discovery of artificial indigo belongs to Germany and to the distinguished chemist Prof. Adolf Baeyer, whilst towards the solution of the difficult problem of its economic manufacture, the first successful steps have been taken by Dr. Caro and the Baden Aniline and Soda Works at Mannheim.

H. E. R.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

The Linacre Chair of Physiology and Anatomy, lately held by Dr. Rolleston, and practically a chair of comparative zoology, will now be split into two, being succeeded by chairs of anatomy proper and physiology proper, with a more direct relation to the teaching of those subjects as part of a preliminary medical education, as was intended by Dr Linacre.

Dr. Oliver J. Lodge has been appointed to the Lyon Jones Professorship of Experimental Physics and Mathematics in University College, Liverpool, by the Councils of that College and of the Liverpool Royal Infirmary School of Medicine. Prof. Lodge has been some time Assistant Professor of Physics at University College, London, and is the author of a work on elementary mechanics and various papers of original research.

SOCIETIES AND ACADEMIES

London

Royal Society, June 16.—"On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains," by G. H. Darwin, F.R.S.

The existence of dry land proves that the earth's surface is not a figure of equilibrium appropriate for the diurnal rotation. Hence the interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea-bed rise up, the materials of which the earth is made must be strong enough to bear this stress.

We are thus led to inquire how the stresses are distributed in the earth's mass, and what are magnitudes of the stresses.

In this paper a problem of the kind indicated is solved, by the use of certain results obtained by Sir William Thomson, for the case of a homogeneous incompressible elastic sphere, and the results are applied to the case of the earth.

If the earth be formed of a crust with a semi-fluid interior the stresses in that crust must be greater than if the whole mass be solid, far greater if the crust be thin.

The strength of an elastic solid is estimated by the difference between the greatest and least principal stresses, when it is on the point of breaking, or, according to the phraseology adopted, by the breaking stress-difference. The most familiar examples of breaking stress-difference are when a wire or rod is stretched or crushed until it breaks; then the breaking load divided by the area of the section of the wire or rod is the measure of the strength of the material. Stress-difference is thus to be measured by tons per square inch.

The problem is only solved for the class of inequalities called zonal harmonics; these consist of a number of waves running round the globes in parallels of latitude. The number of waves is determined by the order of the harmonic. In application to the earth the equator referred to may be any great circle, and is not necessarily the terrestrial equator. The second harmonic has only a single wave, and consists of an elevation at an equator and depression at the pole; this constitutes ellipticity of the spheroid. An harmonic of a high order may be described as a series of mountain chains, with intervening valleys, running round the globe in parallels of latitude, estimated with reference to the chosen equator.

In the case of the second harmonic it appears that the stress-difference rises to a maximum at the centre of the globe, and is constant all over the surface. The central stress-difference is eight times as great as the superficial.

Amongst other examples it is shown that if the homogeneous earth, with ellipticity $\frac{1}{232}$, were to stop rotating, the central stress-difference would be thirty-three tons per square inch, and it would rupture if made of any material excepting the finest steel.

The stresses produced by harmonic inequalities of high orders are next considered. This is in effect the case of a series of parallel mountains and valleys, corrugating a mean level surface with an infinite series of parallel ridges and furrows.

Numerical calculation shows that if we take a series of mountains, whose crests are 4000 meters, or about 13,000 feet above the intermediate valley-bottoms, formed of rock of specific gravity 2·8, then the maximum stress-difference is 2·6 tons per square inch (about the tenacity of cast tin); also if the mountain chains are 314 miles apart, the maximum stress-difference is reached at 50 miles below the mean surface. It appears that there is no stress at the surface, but the solution is only approximate, for it does not give the stress actually within the mountain masses, but gives correct results at some three or four miles below the mean surface.

The cases of the harmonics of the 4th and higher orders are also considered; and it is shown that, if we suppose them to exist on a sphere of the mean density and dimensions of the earth, and that the height of the elevation at the equator is in each case 1500 meters above the mean level of the sphere, then in each case the maximum stress-difference is about four tons per square inch. This maximum is reached in the case of the 4th harmonic at 1150 miles, and for the 12th at 350 miles, from the earth's surface.

It is then shown that the great terrestrial inequalities, such as Africa, the Atlantic Ocean, and America, are represented by an harmonic of the 4th order; and that, having regard to the mean density of the earth being about twice that of superficial rocks, the height of the elevation is to be taken as about 1500 meters.

Four tons per square inch is the crushing stress-difference of average granite. From these results it may be concluded that either the materials of the earth have about the strength of granite at 1000 miles from the surface, or they have a much greater strength nearer to the surface.

This investigation must be regarded as confirmatory of Sir William Thomson's view, that the earth is solid nearly throughout its whole mass. According to this view the lava which issues from volcanoes arises from the melting of solid rock, which exists at high temperature at points where the pressure is diminished, or else from comparatively small vesicles of rock in a molten condition.

Zoological Society, June 21.—Prof. W. H. Flower, F.R.S., president, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of May, 1881, amongst which special attention was called to an African Wild Ass (*Equus tæniopus*) from Upper Nubia, and a White-marked Duck (*Anas specularis*) from Antarctic America, both new to the collection.—Mr. R. Bowdler Sharpe exhibited a specimen of *Podilymbus podiceps*, stated to have been killed at Radipole, near Weymouth, in the winter of 1880–81.—Mr. W. A. Forbes read a paper on the Petrel called *Thallassidroma nereis*, by Gould. This, he showed, was not a true *Procellaria*, but must form the type of a new genus, to be called *Garrodia*, most closely allied to *Oceanites*, *Fregetta*, and *Pelagodroma*, and constituting with them a distinct family of "Tubinares," proposed to be called "Oceanitidæ."—Mr. W. A. Forbes read a paper on the conformation of the thoracic extremity of the trachea in the "Ratitæ" birds, noting specially a highly-developed syrinx in the genus *Rhea*, in which respect it differed from all the other genera comprised in that group.—A communication was read from Mr. George F. Bennett, C.M.Z.S., containing an account from personal observation of the habits of the *Echidna hystrix* of Australia.—Mr. G. A. Boulenger read a paper on the Lizards of the genera *Lacerta* and *Acanthodactylus*, prepared after a study of specimens in the British Museum.—Mr. F. C. Selous read a paper on the Antelopes that had come under his observation in Central South Africa. He exhibited a series of skins of the Bush-Buck (*Tragelaphus sylvaticus*), and pointed out their variations in different localities; also specimens of the Poku (*Cobus vardoni*), and the Speke's Antelope (*Tragelaphus Spekii*).—A communication was read from the Rev. O. P. Cambridge, describing some new genera and species of Araneidea.—Mr. Sclater pointed out the generic divisions of the Bucconidæ which he proposed to adopt in his monograph of the group now approaching completion, and characterised a new species of the family under the name *Nonnula cineracea*.—Mr. R. Bowdler Sharpe communicated some notes on new or rare species of Flycatchers lately added to the British Museum, principally from the Gould collection, and which it was proposed to call *Malurus cyanochlamys*, *Siphia obscura*, and *Rhipidura Macgillivrayi*.—A second paper by Mr. Sharpe contained an account of several collections of birds formed by Mr. W. B.

Pryer in the district of Sandakan, in North-Eastern Borneo. Two new species were described as *Lanius Schalowi* and *Dicæum Pryeri*.—Lieut.-Col. H. H. Godwin-Austen read the second portion of his paper on the land shells collected by Prof. J. Bayley Balfour during his recent expedition to the Island of Socotra. It referred to the family *Helicacae*.—Mr. G. E. Dobson communicated some notes on certain points in the muscular anatomy of the Green Monkey *Cercopithecus callithrix*.—Dr. A. Günther exhibited and read a description of a specimen of *Schedophilus medusophagus*, a Mediterranean fish new to the British fauna, lately captured off the coast of Ireland.

Anthropological Institute, June 14.—Major-General A. Pitt-Rivers, F.R.S., president, in the chair.—General Pitt-Rivers read a paper on the discovery of flint implements in the gravel of the Nile Valley, near Thebes. The worked flints were found embedded two or three metres deep in stratified gravel. Much interest has always been attached by anthropologists to this subject on account of its bearing on the antiquity of man. While in Europe we know that the use of stone for implements preceded the employment of metals, and was coeval with many animals that are now extinct, we have hitherto had no certain evidence that this period in northern regions, remote as it undoubtedly was, may not have been contemporaneous with the very earliest phase of Egyptian civilisation, traced backward as it is by the now accepted chronology of Manetho to an antiquity of 7000 years from the present time. Now, however, the evidence of human workmanship has been found in gravel deposits which had become so indurated that the ancient Egyptians were able to cut flat-topped tombs in it, supported by square pillars of gravel, which have retained their form uninjured to the present day, proving an enormously greater age for the flints embedded in the gravel, some of which were chiselled out of the sides of the tombs.—Mr. Alfred Tylor read a paper on the human fossil at Nice discovered by M. Ischa in December, 1880.—Mr. F. E. im Thurn read a paper on some stone implements from British Guiana.—Mr. J. Park Harrison exhibited a collection of Danish and French photographs.—The following papers were taken as read:—Mr. Gerard A. Kinahan, on sepulchral remains at Rathdown, co. Wicklow.—Mr. J. H. Madge, notes on some excavations made in Tumuli, near Copiapo, Chili, in June, 1880.—A number of specimens collected by Mr. Madge were exhibited, among which were two skulls, a quantity of pottery, and a cervical vertebra, in which was embedded a stone arrow-head.

PARIS

Academy of Sciences, June 27.—M. Wurtz in the chair.—The following papers were read:—Observations of Comet *b* 1881 (comet of 1807) at the Paris Observatory, by MM. Bigourdan, Wolf, and Thollon; note by M. Mouchez (see p. 223).—On the prolegomena of a new treatise on meteorology, published in Italy by M. Diamilla-Muller, by M. Faye. The first part is entitled "The Laws of Tempests (according to Faye's theory)," and M. Faye expresses satisfaction that his views seem to be gaining ground. In a letter to the author he suggests that in thunder-storms the source of electricity is not merely charged air (and icy particles) whirling downwards from upper regions, but electricity is developed in the act of gyration (reminding us of a Holtz machine working up a weak charge).—M. Janssen presented a photograph of the comet.—On Fuchsian functions, by M. Poincaré.—On the injuries to vegetation produced in treatment of phylloxerised vines, by M. Catta.—Influence of variations of atmospheric pressure on the duration of oscillations of the pendulum, by M. Saint-Loup. He found an advance of 0·077s. to occur in the day for a lowering of mercury pressure 10 mm. The experiment was of a preliminary nature.—Observations on the comet, and principally on the physical aspect of the nucleus and the tail, by M. Flammarion. He inclines to the view that comets' tails are not material—perhaps an excitation, electric or other, of ether. Their transparence favours this view. He also calculates that the tail of the comet of 1843, at the distance of the earth from the sun, must have swept space with a velocity of 64,000,000 of metres per second. Any molecule of matter flying at such a rate would not remain a single instant dependent on solar attraction, and would not go in a closed orbit.—On the surface with sixteen singular points, by M. Darboux.—On the surfaces for which the co-ordinates of any point are expressed by Abelian functions of two parameters, by M. Picard.—On a general means of determining the relations between the constants contained in a particular solution and those contained by the rational co-efficients of the corresponding differential equation, by M. Dillner.—On the vibratory forms of circular liquid surfaces, by M. Decharme. The internodal distances are inversely proportional to the corresponding numbers of vibrations; and this result is independent of the nature of the liquid. There is the greatest similarity between the vibratory forms in question and those of soapy pellicles of the same diameter.—On the employment of liquid prisms in the direct vision-spectroscope, by M. Zenger. On the anterior plane of a liquid prism he fixes a quartz prism of the same refringent angle, but placed in an opposite direction; the posterior face has, as usual, a plane parallel plate. The loss of light by reflection at the anterior and posterior surfaces is thus reduced to a minimum; the spectra are very intense, and the lines are well defined.—Photography of colours, by coloration of layers of coagulated albumen, by MM. Cros and Carpentier. M. Edm. Becquerel pointed out that it was not an immediate photographic reproduction of images with the natural colours of bodies, but a polychrome working by way of photographic impression, in which the tints of images are varied at will with the shades of the colouring matters used, and are not connected in any necessary manner with the colours of the active rays.—Pneumatic apparatus; pneole, spirelle, by M. de Romilly. In the pneole a jet of water sent upwards (say) by a turbine immediately enters an orifice (larger than that it comes from) of a vertical conical pipe, in which some of the water accumulates; and through this water numerous bubbles of air ascend, but cannot return. The water returns to the turbine by a lateral pipe. The spirelle (also for producing an air current) is entirely immersed in the circulating liquid (say in a turbine). It consists in one case simply of a slit of special position and nature in a tube which rises from the liquid, one edge of the slit is higher than the other.—On silicium, by MM. Schutzenberger and Colson.—On a cyanic ether of borneol, by M. Haller.—On the *rôle* of phosphoric acid in volcanic soils, by M. Ricciardi. This is a reply to M. de Gasparin.—On the volcanic soil of Catania, by M. Tedeschi di Ercole.—Unilateral phenomena, inhibitory and dynamogenic, due to an irritation of the cutaneous nerves by chloroform, by M. Brown-Séquard.—New mode of electric excitation of nerves and muscles, by M. d'Arsonval. In the apparatus described he aims at giving the induction current a mathematically definite value, easy to reproduce, rendering the electric excitation purely mechanical (not chemical), and at having an induced current of neutral direction (no positive or negative pole).—On the etiology and pathogeny of variola in the pigeon, and development of infectious microbes in lymph, by M. Jolyet.—Influence of nature of food on the development of the frog, by M. Yung. The substances tried stand in the following decreasingly favourable order:—Beef, fish, coagulated albumen of hens' eggs, albuminoid substance of frog's egg, vegetable substances (algæ). The two latter do not suffice to transform the tadpole into a frog. A purely albuminous substance suffices.—Metamorphosis of the Pedicellata, by M. Barrois.—On the formation of the cyst in muscular trichinosis, by M. Chatin.

CONTENTS

THURSDAY, JULY 14, 1881

SYMBOLIC LOGIC

Symbolic Logic. By John Venn, M.A., Fellow and Lecturer in the Moral Sciences, Gonville and Caius College, Cambridge. Pp. xxxix. 446. (London: Macmillan, 1881.)

MANY who are interested in the progress of logical science have looked forward to the appearance of this long-expected work as one likely to give them a logical treat. They will not be disappointed. It may be impossible to accept Mr. Venn's opinions as decisive of some points which he discusses, and it would not be difficult to indicate deficiencies; but we have no book which approaches the one before us in the thoroughness with which it opens up the logical questions of the day. With equal industry and ability Mr. Venn has gone over almost the whole literature of logic so far as it contains any germs of the scientific system associated with the name of Boole. Mr. Venn writes professedly as an admirer of Boole, and his work consists to a great extent of the matter of lectures upon Boole's logic, delivered under the inter-collegiate scheme of lecturing, which has now been in operation for about twelve years at Cambridge. Thus the book is substantially an exposition of Boole's Logic, and practically the only one which we have. Boole's own great work, "The Laws of Thought," appeared more than a quarter of a century ago (1854), and has never reached a second edition. It has been much more talked about than read.

If Mr. Venn then had done nothing more than publish a comparatively easy and readable exposition of Boole's profound but difficult treatise, he would have done a good work. But he has done a good deal more, because he has worked out the relation of Boole's system to all discoverable previous attempts at a symbolic or quasi-algebraic treatment of the syllogism, as also to all who have since Boole's time endeavoured to improve upon his system. The writings of almost one hundred logicians have been investigated by Mr. Venn, and not a few of these writers are practically unknown to English readers. If I mention the names, for instance, of Bolzano, Bardili, Dalgarno, Darjes, Lipschitz, Maass, Maimon, Segner, Semler, Servois, Weise, it is unlikely that the reader, unless he has made a very special study of logic, will ever have heard of most of these names before. A great change has taken place in the standard of scholarship expected of authors nowadays. During the last century philosophers calmly wrote down whatever came uppermost in their minds, in complete indifference to their ignorant predecessors. David Hume discovered and expounded the laws of the association of ideas, unconscious that it was all in Aristotle's works. Jeremy Bentham wrote upon logic with sublime confidence, although his reading had been confined to the compendiums of Sanderson and Isaac Watts. Now a man is expected to read everything about his subject before he writes anything. The late Sir William Hamilton of Edinburgh was the ideal of the new method, towards the introduction of which he much contributed. He had all the doctrines of logicians of various schools classified in his common-place books; but when he came to work out his own system of the syllogism, fell disease arrested him before the work was half done. It must require much judgment to use the bibliographic method, as one may call it, to an adequate, and yet not to an excessive extent.

Perhaps the most interesting chapter in the whole book, is the last one, containing "Historic Notes," which are however merely supplementary to a great quantity of historical information given incidentally in the preceding chapters. The table on p. 407 is one of extreme interest. It shows and classifies in the clearest way no less than twenty-five apparently different modes in which logicians from the time of Leibnitz had attempted to represent symbolically the ordinary universal negative proposition, say, no S is P. Boole and Dr. Macfarlane, for instance, express it as denying the existence of the class of things S which is P. Hamilton introduced a clumsy wedge-shaped copula with a stroke across it to express negation; Darjes entirely misused well-known mathematical signs in the expression $+ S - P$. Segner's formula is hardly better, namely $S < - P$. Mr. MacColl's notation, so recently the subject of discussion in the Mathematical Society, the *Educational Times*, and NATURE, is at least convenient, namely $S : P'$, though, as I venture to hold, only a disguised form of the equation $S = S P'$. But this single page gives matter for endless study, and Mr. Venn has conferred a great benefit upon logical students in opening up the subject of logical symbolism and logical method in its full extension, thus hastening the time when some decision can be arrived at.

There is, however, much that is novel in the volume. No author, for instance, has carried the diagrammatic representation of logical relations to anything like the same extent and perfection as Mr. Venn. Starting with the well-known circular diagrams, attributed to Euler, but traced back to earlier logicians, at any rate to Lange, Mr. Venn has succeeded in representing, by interlacing oval figures, the logical relations of four or even more terms. Although opinions may differ as to the value of the method, he has unquestionably worked out a complete and consistent system of diagrammatic reasoning, which carries the Eulerian idea to perfection. He has gone even further and has converted his diagrams into a kind of logical-diagram machine, which allows the elliptic segments representing classes to be selected and rejected mechanically. Of this remarkable device Mr. Venn (p. 122) says that "it would do very completely all that can be rationally expected of any logical machine. Certainly, as regards portability, nothing has been proposed to equal it, so far as I know." The latter statement may be certainly conceded, as the machine, though constructed needlessly large, is only five or six inches square, and three inches deep. So far, however, as I can judge from the somewhat brief and unexplicit description given by Mr. Venn, I cannot see how his machine can perform logical operations automatically. The selections of classes have to be guided and judged by the selector, and all that the mechanical arrangements effect is to select a whole class of elliptic segments at one movement of the fingers. This mechanical diagram, then, is analogous, as Mr. Venn remarks, to what has been described as "The Logical Abacus," but I do not think it can be called mechanica in the same degree as the logical machine.

In connection with these complex logical diagrams arises a curious and almost amusing illustration of the impossibility of knowing all that has been written on a subject. Mr. Venn in the Historic Notes has carefully gone over all logical writings known to him, and concludes (p. 426) that "hardly any attempts have been made to represent diagrammatically the combinations of four terms and upwards. The only serious attempt that I have seen in this way is by Bolzano." This statement is qualified in the Introduction or Preface (p. xxx.) by reference to H. Scheffler's "Naturgesetze," published in 1880. But if Mr. Venn had happened to look much nearer home, into the able "Outline of Logic for the Use of Teachers and Students," by the Rev. Francis Garden, M.A., Trinity College, Cambridge (1867), he would have found at p. 39 a diagram of five interlacing circles representing the relations of five terms. The diagram is thus described at the foot, "Genus A partly overlapped by genera B, C, D, and E, giving for species A B, A B C, A C, A C D, A D, A D E, A E, A C D E." The circles are broken in their unessential parts for the purpose of saving space. Mr. Venn's ellipses are in this respect much more convenient than circles, and the method of shading segments so as to show their propositional treatment to the eye is an important improvement; but the principle on which complex logical relations may be graphically represented is clearly seized by Mr. Garden.

Mr. Venn, although an ardent admirer of Boole, as indeed all advanced logicians must be, remarks (p. xxviii.) that his actual originality (priority?) was by no means so complete as is commonly supposed and asserted. But I am a little surprised to notice that Mr. Venn, although mentioning (p. 9) Thomas Solly's "Syllabus of Logic"[1] in relation to another matter, does not draw attention to the remarkable symbolical expression for the laws of the syllogism given therein. This brief work is throughout highly acute and philosophical.

The really important question which underlies the whole discussion of symbolic logic regards a technical and apparently minor point, namely the exclusive or unexclusive character of logical alternatives. When we say, for instance, that "capital is either fixed or circulating," is it implied *in the mere form of the statement* that capital cannot be at the same time fixed and circulating? Boole held so; or, at any rate, he held that any logical equation of his own system not conforming to this condition was imperfect and uninterpretable. But since Boole's time several logicians have contended that this condition was arbitrary, and in fact an error of Boole's. It is one chief purpose of Mr. Venn's book to uphold Boole's system in its integrity, and he writes in an attitude more or less of protest against subsequent innovators. This question has been noticed by Mr. MacColl in his letter (NATURE, vol. xxiv. pp. 124-126). It is however a question which requires chapters, if not books, for its adequate treatment; it is in fact to be judged by the success of a system, rather than by any simple direct arguments.

In regard to this letter of Mr. MacColl, I may point to the fact that I have already disputed the philosophical correctness of MacColl's symbolic innovations (NATURE, vol. xxiii. p. 485), while as regards the main principles of his calculus, it is out of the question that he should claim novelty. But we may nevertheless regret that Mr. Venn has referred in a slighting tone to investigations which have been carried out with great earnestness and acuteness. Mr. Venn does not speak in the same slighting manner of Prof. Schröder's essay, though I presume it is clear that the latter was as completely forestalled by previous writers unknown to him as was Mr. MacColl. In fact the way in which independent investigators are converging and meeting in a modified Boolian system is strong evidence that the questions so clearly set forth by Mr. Venn are becoming ripe for decision.

W. STANLEY JEVONS

[1] "A Syllabus of Logic, in which the views of Kant are generally adopted and the Laws of the Syllogism symbolically expressed." by Thomas Solly, Esq., late of Caius College, Cambridge. (Cambridge, 1839.)

ASTRONOMY FOR AMATEURS

A Cycle of Celestial Objects. Observed, Reduced, and Discussed by Admiral William Henry Smyth, R.N., K.S.F., D.C.L. Revised, Condensed, and greatly Enlarged by George F. Chambers, F.R.A.S., of the Inner Temple, Barrister-at-Law. (Oxford: The Clarendon Press, 1881.)

THERE can be, we think, little doubt that the publication of Admiral Smyth's "Cycle of Celestial Objects" powerfully stimulated a taste for astronomy amongst amateurs in this country. It was popular in style, and the contents generally were such as possessed interest for the numerous class of readers who neither require nor would appreciate more technical treatises. The gossiping notes interspersed throughout the work had their special attraction for many readers.

Mr. Chambers says he would not have undertaken the task of preparing a new edition of Smyth's work for the press had he not been convinced that there was a widespread desire for it. The copyright of the work, with the Admiral's notes, unpublished drawings, &c., had come into his hands, but there remained the digesting of these materials and interweaving them with the contents of the first edition. His programme he states to have been "so to revise, prune, and amplify Admiral Smyth's *Bedford Catalogue*, as to provide a Telescopist's Manual for Refractors up to, say, 8 inches of aperture, and to embody the progress of the science up to 1880, just as the original edition might have been considered fairly complete for 5 inches of aperture up to 1845." In carrying out this programme he has deemed it essential to include objects in the southern heavens, which we do not command in these latitudes.

It is to be understood that the new edition is confined to the *Cycle* proper, or to the second volume of the original work, the *Prolegomena* being, as Mr. Chambers remarks, for the most part written up to date in the last edition of his "Handbook of Astronomy." The number of objects included by Smyth was 850, the number in the present volume is 1604. Viewing the work as one intended for the guidance of the amateur as to the objects which it may be worth his while to observe, the additions, upon the selection of which considerable pains appear to have been bestowed, nevertheless include many stars that can hardly claim to be so regarded: we allude to such objects as Nos. 252, 334, 335, 346, 371, 396, 737, 974, 1025, 1149, &c. Perhaps a less extended list with fuller

descriptions of such as possess special interest would have been equally acceptable to amateurs generally.

We are not disposed to criticise too closely a volume involving a large expenditure of time and trouble for the benefit of those who occupy their leisure evenings in telescopic observations, but as the author expresses his desire to receive corrections or suggestions for future editions of his work, we will here refer to several defects which we have remarked in a pretty careful examination of it, in the hope that his attention may be directed to the kind of revision by which another edition may be improved. Some of the more remarkable objects appear to be treated with unfortunate brevity; we may instance the fine binary star 6 p Eridani, of which a single epoch is given, without mention of the orbit having been determined by Dr. Doberck, or indeed any intimation that the star is in rapid motion: the first elements were assigned by Jacob. A still more noticeable case is that of α Centauri, one of the most interesting objects in the heavens, which is disposed of in half-a-dozen lines, without reference either to the elaborate investigations of its annual parallax since Henderson's time, to its large and well-established proper motion, or to the numerous orbits which have been computed, more especially those obtained since the passage of the peri-astron by Dr. Doberck and Dr. Elkin. Only two epochs are transcribed, one of them being the comparatively rough result of Gilliss at Santiago in 1851; in no instance would it have been better worth while to extract from the long series we possess, a sufficient number of measures to enable the reader to judge of the motion in the system. A very insufficient notice appears of Σ 518, a binary of which we may soon expect to have approximate elements, and the case of γ Coronæ Australis is quite misrepresented; from the few epochs given at p. 555, it might be inferred that there has been a direct change in the angle of position of about 30° in forty-five years, whereas there has been an actual *retrograde* motion in the angle of nearly 160°, upon which Schiaparelli calculated elements which represent the latest measures closely. Of the four cases where the author has appended orbits, in three (Castor, ζ Cancri, and ξ Ursæ Majoris) they are vitiated by typographical or other error.

Kirch's variable star in Cygnus, which Mr. Chambers calls χ^2, is the true χ Cygni of Bayer, to which letter Flamsteed's 17 Cygni has no claim; the cause of Flamsteed's misnomer was explained by Argelander many years since. The designation χ^2 is calculated to add to the doubt and confusion already existing as to this variable, of which the author unwittingly affords an illustration. The position assigned for 1890 is not that of the variable star (which is Lalande 37835), but is that of Piazzi XIX. 295, wrongly identified with Kirch's star by Piazzi, a circumstance to which, oddly enough, Mr. Chambers alludes in his notes, warning his readers against a mistake which he has himself just made. The correct place of the variable for 1890 is in R.A. 19h. 46m. 21s., Decl. 32° 38'·2.

The story of Cacciatore's supposed distant planet is left where it was by Smyth, the later calculations of Valz and Oeltzen, who showed that the motion indicated by Cacciatore could only apply to a minor planet, not being mentioned; and there are a number of other cases where the information supplied has not been brought up to date.

Mr. Chambers's volume has been handsomely printed at the Clarendon Press, and includes, for a frontispiece, the scale of colours, given by Smyth in his "Sidereal Chromatics," with the view to assist observers, in judging of the colours of the components of double stars.

OUR BOOK SHELF

Botany for Schools and Science Classes. By W. J. Browne, M.A., Lond., Inspector of National Schools. Second Edition, revised and enlarged. (Dublin: Sullivan Brothers, 1881.)

MR. BROWNE is the author of a variety of elementary mathematical books. In preparing this little manual of botany it may be presumed, therefore, that he has had to struggle with the difficulties which must always beset the amateur. The result resembles what one has often unfortunately met with in similar cases before. There is a want of simplicity in the treatment, much that is unessential and unnecessary for students of any grade, a good deal that is only of historical value, and what is worse, not a little that is downright error. This is the more unfortunate, as the questions at the end of the chapters and the examination papers which fill the last pages show that the book has a very definite aim. What, however, it may be asked, is likely to happen to examinees who reproduce such statements as the following? "*Coffee.*—The fruit consists of two halves, nearly hemispherical;" or "*Galls*—excrescences on oak, produced by an excretion thrown out round an egg deposited by an insect" (p. 98). On p. 60 the beech is given as affording an example of a capsule in its fruit; here the author has confounded the involucre with a pericarp. On the same page we find the following remark: "Around the seed . . . there is often developed a quantity of *albumen*, for the nourishment of the seed during germination"; on p. 55, "The germinal vesicle soon develops into the embryo or germ, containing the plantlet." This is on a par with the account of the process of fertilisation on p. 10, "A protoplasmic substance (*fovilla*) flows from the pollen-grain into the ovule and ripens it, so that it becomes a seed." The part of the book devoted to systematic and descriptive botany is better, though often open to criticism. If the writer had carefully studied *Penicillium* he would not have said, "The cells composing the branches (Fig. 89) are spores or *conidia*"; he has apparently been misled by his Fig. 89, which might do for one of the bog-oak ornaments sold in Dublin shops, but is a very inadequate representation of *Penicillium*. The examples of plant-descriptions are not sufficiently full, and are sometimes obscure, as for instance when the anthers of the common daisy are said to be "simple at base." The whole book still wants a thorough revision at the hands of a competent teacher to make it a safe guide for elementary students.

First Lessons in Practical Botany. By G. T. Bettany, M.A., B.Sc., F.L.S. (London: Macmillan and Co., 1881.)

THIS is an excellent little book. Its diligent study by teachers as well as pupils would give descriptive botany the real educational value which is so often claimed for it, and at bottom it no doubt possesses, if only the old type of manuals could be exterminated. What a weight would be removed from examiners' minds if examinees would really take to heart Mr. Bettany's impressive admonition (which should be hung in every examination room where plants are set for description):—"Do not *suppose* or *imagine* facts of structure which you cannot verify." It is really refreshing to come upon a manual, the object of which is to drill students in a healthy scientific method, and not merely to teach them how to impose on examiners with a show of sham and often preposterous knowledge, which has but a temporary hold on the memory and none on the understanding. The only genuine criti-

cism of a manual like this would proceed from one who had actually tested its use. Improvements will gradually suggest themselves; a few friendly suggestions might be even ventured upon offhand. On p. 63, for example, the following definition is open to objection:—"*Trichome*, a generic term for all organs developed by emergence from single cells of the epidermis." The chapter on Floral Diagrams is good. But it never seems to have been suggested that a genuine interest might be given to lessons in botany by making the pupils arrange the actual parts of the flower so as to form the diagram. All that is wanted is a flat square of cork covered with paper, on which four concentric circles are traced. It would be best to have three such squares for each pupil, with three, four, or five radiating lines drawn intersecting the circles, according as flowers with a ternary, quaternary, or quinary symmetry are to be examined. As each successive whorl of floral organs is removed, its parts should be pinned out in their proper relative positions by the pupil. The cyclical symmetry of the flower is clearly brought out in this way, even where it is apparently disguised. Some details in working the method would need a little elaboration, as, for example, the treatment of gamopetalous flowers; but this may be left to the ingenuity of teachers like Mr. Bettany.

Rabenhorst's Kryptogamen-Flora von Deutschland, Oesterreich und der Schweiz. Erster Band: 1 und 2 Lief. Pilze, von Dr. G. Winter. (Leipzig, 1881.)

FEW men have done their fellow-workers in science greater service, even if of a somewhat unobtrusive sort, than Dr. Ludwig Rabenhorst, whose recent death we announced with regret (NATURE, vol. xxiv. p. 108). His "Flora Algarum aquæ dulcis et submarinæ" is an indispensable guide to an immense labyrinth of species and genera which lie scattered up and down botanical literature. These are digested into a methodical enumeration which makes little attempt to be critical, but is content to bring the materials together just as every one who intends to study what has been done in any special group without such an aid must do for himself. Had Rabenhorst attempted more he would never have done the useful work that he did. One very convenient feature of his books is the brief synopsis of the genera of each group, accompanied by outline woodcuts of some leading types. Amongst organisms whose real affinities are often so obscure as the lower cryptogams, the utility of this plan cannot be sufficiently approved. The woodcuts often convey information at a glance which hours of study and comparison would not extract from the descriptions. The present work, of which two parts have so far appeared, is substantially a new edition of the author's "Deutschland's Kryptogamen-Flora," of which the first appeared as far back as 1844. The death of the original author may, it is to be hoped, have no effect on impeding its completion, as different groups are assigned to different hands, Dr. Winter commencing the fungi in the two parts before us. The scope of the whole work will be very much enlarged, but the same convenient features will be perpetuated. A speedy completion will be devoutly desired by all students of European Thallophytes.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Comet

FURTHER measures have been obtained at Greenwich of the position of the least refrangible edge for three of the four comet-bands with the following results:—

	Yellow band.	Green band.	Blue band.
Comet	$5630 \cdot 4 \pm 1 \cdot 6$	... $5162 \cdot 7 \pm 0 \cdot 4$	... $4733 \cdot 9 \pm 1 \cdot 1$
Bunsen Flame	5633·0	... 5164·0	... 4736·0
No. of Obs.	7	... 26	... 6

The identity of the comet-bands with those in the first spectrum of carbon appears to be clearly established, but in each case the comet-band is slightly shifted towards the blue. The displacement of the green band, if real, would indicate an approach of 47 ± 14 miles per second, whereas the comet was actually receding from the earth at the rate of about twenty miles per second. Such a displacement might, of course, be explained by an emission of cometary matter on the side towards the earth, but it would seem more probable that it is due to the circumstance that the edge of the comet-band is not quite sharp, and that a small portion on the red side is cut off. This would apply with still more force to the yellow and blue bands, which indicate somewhat larger displacements towards the blue. The displacements however, though all in the same direction, are not largely in excess of the probable errors. The comet-bands were compared with those given by vacuum-tubes containing cyanogen and marsh-gas, as well as with those of the Bunsen-burner flame, and three forms of spectroscope were used, viz. (1) the half-prism spectroscope with a dispersion of 18½° from A to H, and a magnifying power of 14; (2) the half-prism spectroscope reversed (as for prominence observations), giving a dispersion of 5° from A to H and great purity of spectrum, with a magnifying power of 28; and (3) the star spectroscope with a single prism of flint. No measures were obtained of the band in the violet, which was only seen on two occasions. It appeared to be sensibly coincident with the band in the first spectrum of carbon at 4311.

Mr. Maunder also noted several of the Fraunhofer lines in the continuous spectrum, in particular F (the position of which was determined by comparison with $H\beta$) and two other lines which were respectively near E and a strong double line at 5327.

W. H. M. CHRISTIE

Royal Observatory, Greenwich, July 12

I SUCCEEDED in photographing the comet in Auriga on Friday night, June 24. Since then I have taken several photographs of it. One made last night with an exposure of 2 hours 42 minutes shows the tail about 10° long. There are many stars on the plate, some shining through the tail.

HENRY DRAPER

The Physiology of Mind Reading

I HAVE received from Dr. G. M. Beard of New York (well known for his studies of Trance and related states) a letter in reference to the experiments with Mr. Bishop, of which Mr. Romanes has given an account in NATURE. Dr. Beard, writing before our experiments were carried out, mentions his own investigation, years ago, of much more remarkable performances than Mr. Bishop's, and incloses an article "On the Physiology of Mind-Reading," which he contributed to the *Popular Science Monthly* (New York) as far back as February, 1877. If this article had been shorter I would fain have asked you to reprint it, giving as it does a far more varied record of facts than came under our observation, and a series of carefully-drawn conclusions within which our conclusion falls. I will only say that if I had known of this article I should hardly have thought it worth while to spend time in the trial of Mr. Bishop's powers, or even had the curiosity to attend that first meeting amid the cloud of scientific witnesses.

G. CROOM ROBERTSON

July 9

Mind and Muscle-Reading

KINDLY allow me to correct a printer's error in my letter of last week. In describing the case of so-called thought-reading examined by myself in the clergyman's family in Derbyshire, I wrote: "The failures in *my* examination did not amount to one in ten, and were a smaller fraction when the children were not embarrassed by strangers," &c. The word "my" was printed "any," thus destroying the meaning of the sentence. I will just add that the clergyman in question is an old graduate of Trinity College, Dublin; his integrity is above suspicion, and even did not his position as a Christian minister negative the idea of trickery, the last experiment which I described disposes of this very natural explanation.

W. F. BARRETT

Special Solar Heat-Radiations and their Earth-felt Effects

THE well-filled lectures on Solar Physics by Prof. Balfour Stewart, published in NATURE, vol. xxiv. pp. 114, 150, will undoubtedly promote the study and assist the understanding of those subjects; and if a single one of the many items alluded to was not quite correctly described, that is neither surprising in itself nor likely to do much harm amidst the wealth of information which was at the same time both correctly stated and neatly conveyed. I should not therefore think myself now called on to notice one exceptional paragraph, but that it contains a most singular mistake in attributing to me conclusions from my own Edinburgh observations that are the very opposite of what I have often published between 1869 and the present time. Nor do I propose to make any positive complaint; for I rather admire the honesty of the lecturer who, after arguing for the more spotted periods of the sun's disk being its occasions of strongest heat evolutions, yet stated voluntarily and against himself that a directly opposite conclusion to his had been deduced by me from the unrivalled collection of more than thirty years of rock-thermometer observations on the Calton Hill. That is to say, that a certain eleven-year heat-wave shown by those thermometers coincided with, not the *maximum*, but the *minimum* spotted state of the sun; subject however to what the lecturer termed "a slight," but in reality a two or three year "lagging behind" the visible solar phenomenon.

Now let the sun, at any short-lived epoch, give forth an extra radiation of heat: I cannot imagine any person attempting or expecting to find its effects, after two or three *years*, as an acutely marked phenomenon in daily air and superficial earth-temperature observations. When therefore a very sharp phenomenon was marked on, or by, our thermometers, I looked for its explanation, not to what had occurred and passed away again several years before, but to something in nearly simultaneous progress on the sun. This something too, which I held forth upon even in my first paper on the subject in 1869 to the Royal Society, was ready at hand as a *vera causa*; and I ventured to describe it as "the ascending node" of the eleven-year sun-spot curve, or the time when a new cycle of sun-spots is not only well begun, but is in the act of its most rapid increase for any part of the cycle; just as a soda-water bottle effervesces most violently immediately after it is uncorked, rather than long afterwards, when some of its slowly-formed last bubbles are quietly escaping, and much more so than when it is not uncorked at all. In a letter too, printed in NATURE not two years ago, I showed how a great part of the solar action might be, and even had been immediate on our thermometers, in consequence of the very first action of a renovated sun, being a dispersion of the ordinary clouds *in situ*, whence an extra amount of direct sun-shine on the earth beneath them, producing a dry hot year to the agriculturists there.

The second effects I also showed might be an increased evaporation of distant ocean-surface; the formation thereby and bringing round of greater clouds, heavy rain, and precisely the cold seasons which our Edinburgh thermometers had shown, through thirty years, did generally follow the eleven-year wave of heat. Not, evidently, that the sun was then at a minimum of heat radiation, but that a screen of wet clouds had been drawn between it and that part of the earth where observations were going on.

Now something like this whole sequence of effects has just been experienced in Madeira, all in the course of this week, subsequent to the restored energy of sun-spot manifestation and the earth-answering electric cloud of last Sunday, as I wrote to you next day.

Now that, or the first day after the specified occurrence, proved scorchingly hot, with a blue sky and the maximum shade temperature of the season, thus far.

The second day after, a thick screen of clouds was drawn between us and the sun, while the trade-wind was not only restored on the adjacent sea, but with an excess of violence more like that which is felt about Teneriffe: viz, a more southern, and therefore more sun-governed, island.

But the third day after, not only was the sun again totally invisible on account of cloud, but to the surprise of all Madeira there was a heavy, vertical downpour of rain all day long. Old residents protested that they had never, for ten years at least, known anything of the kind at midsummer season. "Precisely so," I replied; "but in the Cape de Verde Islands still further south, and more under solar dominion than even Teneriffe, you will find that every year, the sun coming to the highest northern declination is marked by heavy tropical rains. Wherefore, if Madeira is now visited in the end of June by Cape de Verde solsticial rains, be assured that the sun is at this moment shining above the clouds over Madeira with much more than his usual annual force."

But though as I write, I would seek to draw the attention of your clever lecturer to unusual solar action being often attended with earth-phenomena which lag behind a few hours only, rather than several years, I do hope he will also obtain a perusal of my paper of 1869 from the Royal Society, Burlington House, London, and take note of the forty or more year cycle, as well as other shorter ones there alluded to; for the standard eleven-year cycle, of which we have now begun a new example, will never be completely definable without knowing on each occasion how far the others are mixed up with it. Thus we had, for instance, in August last year, that eleven-year cycle's *maximum* of temperature which I had pre-announced in print ten years before; but it was very near being lost to observation by occurring not far from the middle of the long-enduring *minimum* of the forty-five years' cycle, whose prime origin is as certainly solar as that of the eleven-year, and even then much shorter cycles of twelve or fifteen days only, of which I have noted several examples since I have been here.

PIAZZI SMYTH,
Astronomer-Royal for Scotland

Jones's Hotel, Quinka do Corvalho,
Funchal, Madeira, July 2

Phenomena of Clouds

THE letter from Prof. Smyth (vol. xxiv. p. 212) recalls to my mind a phenomenon I witnessed several years ago in Arran. I was staying at Strathwhillan, on the north side of Brodick Bay, and looking northward had a full view of Goatfell and Maoldoon. The latter resembles an immense mound heaped up against the eastern side of the former. Snow had recently fallen and coated both. Then a south-easterly wind, coming up and across the firth, caused a cloud to be formed at a considerable elevation above the hills, having its under surface outlined in correct correspondence with the outlining of the subjacent mountains. This contour the cloud retained in seeming fixity for several hours. I attributed its continued existence to the effects of unequal radiation between the cold snow-covered hills and the warmer moisture-laden current above. Whether my surmise was correct, and whether the "central fixity" over Madeira can be referred to the same cause, I leave to the consideration of those more scientifically informed than I. HENRY MUIRHEAD

Cambuslang, July 8

Early English Pendulum Measures

I FIND in a volume entitled "Metrology, or Weights and Measures of Great Britain and France," by P. Kelly, "Master of the Finsbury Square Academy, London," in 1816, a list of some of the old pendulum experiments of the last century, which contains some indications quite new to me. I am in hopes that if you will allow me space enough to make them known I may perhaps hear where further information is to be found. One of the measurements which he of course mentions is that of Graham. It is rather strange that though every one of the old writers mentions Graham's experiments confidently, I have hitherto failed to find any account whatever of those experiments. The other observers mentioned by Kelly—and so far as I know by him only—are "Emerson," "Desaguillieres" [who always wrote under the name of Desaguliers], "Rotherham," and "Sir Jonas Moore." The mention is not a mere hearsay repetition of their names in this connection, as he gives the *lengths* found by each for London.

In direct connection I may remark that every one knows that the pendulum has been over and over again mentioned and treated as an ultimate appeal in case of failure of other satisfactory means of restoring national standards. In fact its *earliest* use was for this purpose *only*—except of course in horology. Is it not then a strange thing that it was *never*—during the whole of the century and a half which so regarded it—used as a medium of comparison of actual national standards? In Graham's time the relation of the French and English units of measure was so uncertain that the pendulum, with all its failings, was quite competent to establish a firmer one. Newton's table

of the variation of the seconds' pendulum with latitude was quite trustworthy enough (not to say correct enough) to furnish the geographical difference between London and Paris lengths. Yet as a fact the pendulum never was so appealed to. Yet to this day it is still not uncommonly taught that the pendulum is the proper *natural* standard of reference. In 1816 of course such was the nearly universal dogma.

I say "never," but perhaps one or other of the above observers may be adduced to confute me. J. HERSCHEL

Collingwood, July 11

Faure's Secondary Battery

IN your issue of last week you gave an account of the *soirée* held at King's College, London, on the evening of July 2, and in this account it is stated that "the great event of the evening was the exhibition for the *first* time in England of M. Faure's secondary battery."

At the *soirées* given by the Mayor of Nottingham on the evenings of June 30 and July 3 in connection with the opening of the College by H.R.H. Prince Leopold, I had the pleasure of exhibiting to large audiences one of M. Faure's new batteries. Sheets of lead were bent up into the form of shallow trays, one foot square and one inch deep; in each of these was placed a layer of red lead, then a layer of flannel, then a layer of red lead, and lastly another lead plate. These trays to the number of six were then piled one above the other, after being filled with dilute acid. The cells being connected in series, were polarised by a 10-cell battery of Grove's cells, and after twenty minutes' charging, had taken up a very large quantity of electricity. At a short lecture given during the evening the charged Faure battery was connected with a Gramme machine, and drove it round with considerable velocity for some minutes. After thus employing part of the charge the remainder was used for heating several inches of platinum wire, and for driving for a few seconds a simple form of magneto-electric engine. These experiments amply convinced those present of the practical character of M. Faure's invention. As I have not had the opportunity of examining one of the original batteries of the inventor, I was obliged to make up this experimental form. It is however a convenient form for lecture-room demonstration, as it permits the structure of the battery to be exhibited to an audience. The enormous superiority of M. Faure's cell over the old form of Planté cell is evident at once on experimenting with it. J. A. FLEMING

The University College, Nottingham, July 10

Earthquake in Van

SINCE my former letter I have had an opportunity of visiting the region most affected by the earthquake of May 30, and have obtained some further particulars about it. Its greatest severity seems to have been felt at the Armenian village of Teghourt, lying at the foot of the Nimroud Dagh, at a distance, judging by eye, of not more than four miles from the edge of the crater. This village has been almost entirely destroyed, with the loss of ninety-three lives. By the same shock about 200 houses were thrown down or more or less damaged in the aggregation of hamlets named Akhlat, some six or seven miles further distant from the Nimroud Dagh. Here however happily only two lives were lost and a few persons were injured. On June 9, in the evening, a second shock took place of less violence, which partially damaged a third village, Sipratzor, lying between the other two. As far as I was able to learn these villages were the only localities in which buildings were actually thrown down, though cracks were caused in walls, &c., in other places. The three villages are all in the direct line between the two great extinct volcanoes of the Nimroud Dagh and Sipan Dagh, which fact leads to the conjecture that there may be a line of least resistance joining the two mountains. All three villages, however, are nearer to Nimroud than to Sipan. The greater severity of the shock at Teghourt, the nearest village to Nimroud, may have been due to the latter having been the centre of the disturbance, but it may also have been caused by the fact that the village is built directly upon the solid rock of an ancient lava-bed. The only observation I was able to obtain of the direction of the earthquake wave was communicated to me at a village lying due east of the Nimroud Dagh. Here it was said that the wave came from the south, which would look as if the centre of disturbance were in the Central Kurdistan mountains, not in Nimroud; but one doubtful observation is of course not enough to establish such a point.

Whilst in the neighbourhood I took the opportunity of visiting the Nimroud Dagh. The mountain rises in a very gentle slope, so that it is possible to ride the whole way up and into the crater. The edge of the crater, where we crossed it, is 2810 feet above the Lake of Van by aneroid and about six miles distant from it; some parts of the walls however rise 500 feet or so higher, the most elevated points being to the north and south. The crater is a vast, nearly perfectly circular, hollow, between four and five miles across, the floor of which is an irregular flat dome, partly covered with herbage and partly with dwarf birch and beech and a creeping yew. Among the undulations of the dome, and especially in the depressed ring between the dome and the walls of the crater, are situated some six or seven tarns. One of these, on the margin of which we stopped to rest, is fed by hot springs, which bubble up at numerous points near its edge. I had no thermometer to ascertain the temperature of the water, but I found that one spring, which rose in a small basin almost cut off from the rest of the lake, was just about as hot as I could bear to keep my hand in. This tarn is 880 feet lower than the edge of the crater where we crossed it, this being the lowest point in the whole circuit. I saw no sign of vaporous exhalation, although local tradition has it that the mountain was active not more than four centuries ago; but time did not permit me to explore the whole of the great interior space. EMILIUS CLAYTON

Van, Turkey-in-Asia, June 20

Meteors

SEVERAL splendid meteors having lately been visible, the following observations may be worthy of note in NATURE. I may add that the most brilliant meteor was the one recorded in your columns (vol. xxiv. p. 189).

June 24, 10h. 28m., a very large bright orange coloured meteor equal to Jupiter appeared near Vega.

At 11h. 29m. a deep orange-coloured meteor, larger and brighter, than Jupiter, crossed the extremity of the comet's tail. It left a short bright streak.

June 25, 10h. 52m., a yellowish-white meteor, as bright as Vega, appeared near ζ Cygni.

At 12h. 4m. a white meteor, as bright as Jupiter, appeared just south of x Draconis, and after pursuing a wavy path, disappeared near γ Ursæ Majoris. It left a short streak.

July 3, 10h. 23m. a yellow meteor, nearly as bright as Jupiter, appeared just east of Polaris, travelled slowly in a wavy path, and disappeared north of β Cassiopei.

It will be observed that three out of these five meteors appeared in that part of the sky occupied by the comet, and also that two of them pursued wavy or zig-zag paths. B. J. HOPKINS

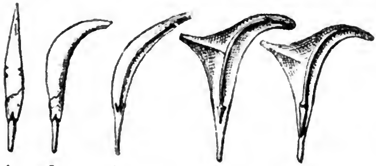
79, Marlborough Road, Dalston, E., July 5

The W-Pattern of Paddles

IN your impression of the 2nd ult. allusion is made to the origin of the W pattern which occurs upon paddles from the Solomon Isles. Without illustration it is difficult to understand the transitions which have taken place, but with the objects before you their history is easily read. I therefore inclose sketches; they are all from the same locality. In Fig. 1 it is seen that the swell of the blade of the paddle has suggested the idea of a fish's body, and accordingly the head with the mouth and eyes of a fish have been carved in their proper place. In Fig. 2 the same occurs, except that the blade is bent, probably to adapt the paddle to steering purposes, or for some other object. These two specimens represent the head of a fish in its realistic form. The progress of ornamentation is from realism to conventionalism. By comparing Fig. 3 with the foregoing it is easily seen that the W represents the mouth and sides of a fish's head reduced to straight lines, the eyes having disappeared. In all the specimens in my possession its position is always that in which the true fish's head occurs in the realistic specimens. In Fig. 4 a further change has taken place, the mouth is omitted, and the sides of the head have been brought together in a point, thus forming a simple triangle. Possibly the idea of a fish's head may have been altogether lost in this stage of the ornament, but in the next example, Fig. 5, the idea revives again, as so frequently happens in like cases, without recurring to the original model. Two eyes are seen to be inserted in the place where one occurred in the realistic specimens, the mouth still being deficient.

There can be little doubt, I think, that this interpretation

affords a true sequence of ideas that have taken place in the minds of the savages who made these things. And it is in complete analogy with the development of ornamentation in other places, of which several examples are in my museum. The interest which attaches to such specimens of savage art and ornament is purely psychological. Taken as the representatives of ideas, and arranged to show the development of ideas, they serve important purposes in the study of social evolution, ex-



1 2 3 4 5

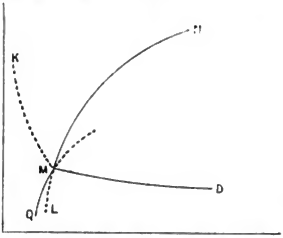
plaining by analogy the law which has operated in producing many otherwise unaccountable conditions of custom, religion, or institutions, of which the successive phases of thought, having never been embodied in tangible forms or committed to writing, cannot be reproduced or arranged in their true order of succession. The sequence therefore is often lost, and wrong causes are assigned to them. A. PITT RIVERS

Hot Ice

HAVING read a paper before the Owens College Chemical Society on January 21, in explanation of Dr. Carnelley's experiments, in which I treated the subject in a similar way to Dr. Pettersson, perhaps I may be allowed to point out one or two differences in my explanation from that given by Dr. Pettersson in NATURE, vol. xxiv. p. 176.

In the first place Dr. Pettersson speaks several times of the point *m* (the triple point) as being 0°·0078 C. below zero, whereas it must be above, because the melting-point of ice rises as the pressure is diminished.

After describing the line *m k*, which I believed then to exist, and which will probably be found really to exist if ice can be



heated, Dr. Pettersson says that in the case of ordinary ice it has been proved that ice does not get hot, and that the limit of the ice-surface is along *l m*, a continuation of the water-steam line *m n*.

Now Prof. James Thomson describes *m* as the point of intersection of three different lines, *n m*, *d m*, and *q m*, the water-steam, water-ice, and ice-steam lines; *m q* being, not a continuation of *n m*, but a separate line, the difference in position being due of course to the latent heat.

I fail to see also how *m k* can be considered a continuation of *d m*, any than more of *n m*. Lastly, after denying the possibility of ordinary ice becoming hot, Dr. Pettersson describes Dr. Carnelley's ice as condensed and not frozen. In those experiments of Dr. Carnelley's which I have seen, the water was frozen round the thermometer, and not condensed on it. The matter therefore seems to stand thus:—If the ice does really become hot, the limit of the ice-surface is most probably along *m k*, whereas if Mr. Hannay and others are correct in stating that the temperatures of the ice and condenser are identical, the limit must be along *m q*, and not along *m l*, which latter is the line denoting the maximum tension of the vapour of water cooled below the freezing point without solidifying, and not of ice below the freezing point.

I would just say also that the idea of an allotropic modification of ice did not occur to me.

SYDNEY YOUNG

The Owens College, Manchester

Note on Piczorhynchus melanocephalus (Ramsay), and Ptilopus viridis (Ramsay), from the Solomon Islands

HAVING lately received several specimens of the Piczorhynchus, which I described under the above name, I find that it is the young of Mr. Tristram's *P. vidua* (see *Proc.* Linn. Soc. of New South Wales, vol. iv. p. 468) from the Solomon Islands. The white collar which commences on the nape is much broader in the young than in the adult, and the feathers of the chest are white, all margined conspicuously with black.

Specimens have been obtained on the island of "Ugi," one of the Solomon group.

I believe the fruit pigeon I determined as *Ptilopus viridis*, from the Solomon Islands, will prove to be the female of *Ptilopus argenteæ*, Gray, of which I have recently seen some fine specimens collected by the Rev. George Brown and Lieut. Richards, R.N., at "Ugi." ED. P. RAMSAY

Anatomical Museum, Sydney, April

THE BRITISH MUSEUM CATALOGUE OF BIRDS[1]

AS has been more than once remarked in our pages, it would require more than one man's lifetime to complete the Catalogue of Birds, if the rate at which the first four volumes were produced had to be continued. Mr. Bowdler Sharpe, who has written these first four volumes, was a young man when he commenced his task, but at the same rate of progress it would have required him to live nearly a hundred years to finish the Catalogue by himself. Dr. Günther has therefore had to seek assistance from outside the walls of the Museum, and has engaged the services of Mr. Seebohm to bring out the fifth volume of the Catalogue, which contains a description of the family *Turdidæ*, containing the Thrushes and Warblers. As Mr. Seebohm has devoted several years to a study of this family, he possesses a special knowledge of his subject probably unequalled by any other ornithologist. It must be remembered that, as in the case of Dr. Günther's Catalogue of Fishes, the Catalogue of Birds is not a mere list of specimens in the national collection, but is in reality a monographic *résumé* of the birds of the world. If we look through the first four volumes of this laborious work we shall find that not only are the species in the British Museum thoroughly described, but that species not included in the collection of that institution are also treated of, and the types of rare birds in Continental museums are fully described; showing that Mr. Sharpe was not content to work solely with the collection under his charge, but that he has compared his notes with the specimens in many of the museums of Europe, and has therefore done his utmost to make the Catalogue in every way complete. But if this is true of the first volumes, it is ten times more so in the case of the fifth, which now lies before us. On turning over its pages we see that Mr. Seebohm has not only visited European museums, but has even

[1] Vol. V., Containing the Family *Turdidæ*, by Henry Seebohm.

been to America for the purpose of examining types, and the result is that up to the date of publication his work must be as complete as personal labour and an unlimited expenditure of time and money could render it. Again, the author's well-known travels in various parts of Europe and Siberia have made him acquainted with the natural history of a number of the species described in his book, and have given him a practical knowledge which must have stood him in good stead at every turn. It is not in this journal only that he will receive the meed due to his energy and perseverance, but he is sure to receive the gratitude of every ornithologist for a monograph of two such difficult families as the Thrushes and Warblers have always proved themselves to be.

Although adopting Mr. Sharpe's classification of the *Passeres*, he finds that this arrangement is artificial; but we are not sure that the arrangement of our author is altogether free from a similar charge. No one who has not studied the birds above mentioned can have the slightest idea of the extraordinary difficulty which the student would experience who tries to classify the Warblers on structural characters only, and we find no fault with Mr. Seebohm when he makes the style of coloration a generic character in these birds. But that the author himself feels a little uncertain in his key to the genera of the Warblers is shown by his introducing some of the genera three times in the *Clavis* under different sections, and it reads somewhat curiously to learn that one of the characters of the genus *Acrocephalus* is to have "the bill acrocephaline (or phylloscopine)"; the truth being that, as in the case of the species of Warblers, the genera so run one into the other that it is difficult, if not impossible, to define the exact natural limits of each. These remarks, however, almost appear hypercritical when one turns to the actual descriptive work of the author, and examines the complete way in which the synonyms are given and the descriptions elaborated, and this with the utmost conciseness consistent with completeness. One thing is evident from the list of specimens, that the British Museum series of these birds is a very full one, and we note with pleasure the constant generosity of the author himself in supplying specimens from his own collection. In the case of a bird like the common Willow Warbler, for instance, the series of specimens embraces nearly every locality whence the species is known, so that its geographical distribution is absolutely illustrated by the skins in the British Museum

In his classification of the sub-family *Turdinæ*, or True Thrushes, colour again plays an important part in classification, but we cannot complain of his arrangement, which seems to be perfectly natural, although we shall not be surprised if some ornithologists urge that some of the genera included in *Erythacus* and *Mrymecocichla* have at least as good grounds for separation on the style of colour as have some of the genera allowed by Mr. Seebohm. But not only will protests be raised on the score of nomenclature of certain species, but the novel feature of hybridisation and imperfect segregation of species introduced by the author will doubtless be subjected to severe tests. His opinions on the imperfections in the code of zoological nomenclature propounded by the British Association are well known, but the critic who attempts the task of dealing with the author on this point must clear himself of the charge (only too true we fear) that he knows of no writer who attempts to carry them out in their entirety. Mr. Seebohm observes (Introduction, p. 11): "I have accordingly adopted the law of priority with the following modifications:—that names which have been extensively misapplied must be rejected, and names otherwise unobjectionable must be retained, if a majority of ornithological writers have used them, even though they may not be the oldest. The adoption of this conformation of the law to the practice of the good old times would also have another immense advantage. It would enable us to omit the authority for the specific name, as all the names would henceforth be *plurimorum auctorum*, and thus the stigma that our names are after all trinomial would be avoided." We must demur to this reasoning, which is heterodox enough to cause the shade of Strickland to arise, and will doubtless bring forward protests from many surviving framers of the British Association code. But we ourselves feel that this practice would very often cause a manifest injustice to the early writers, and we think that this is proved in some instances by Mr. Seebohm himself, as for instance with the name of the Dartford Warbler, which he calls *Sylvia provincialis* (Gmel.), although he admits that Boddaert's name of *Motacilla undata*, published five years before Gmelin's work, and admitted by such authorities as Prof Newton and Mr. Dresser, is referable to the species. Boddaert's name is founded on the *Pitte-chou de Provence* of Daubenton, and Mr. Seebohm himself admits that "the figure is sufficiently good to leave no reasonable doubt as to the species intended to be designated; and Boddaert's name may therefore be held to be 'clearly defined.' Nevertheless there seems to be no sufficient reason why the name in common use should be changed." Here we consider that the long oblivion which enveloped Boddaert's nomenclature was due, not to any fault of Boddaert himself, but entirely rests with subsequent naturalists, who did not consult his work; and that therefore Boddaert has no right to suffer for the shortcomings or laziness of his successors. We are aware that the scarcity of the book makes Boddaert rather an exceptional case, but the principle applies to many of the writings of the fathers. As however the rules of nomenclature must sooner or later be re-discussed by the British Association, we may leave the defence of his principles to Mr. Seebohm himself, feeling sure that no one can read his opinions on this subject without feeling that he has a good deal to say for his view of the case.

One great feature of the present volume is the courage which the author has shown in applying the doctrine of the evolution of species to the birds as they exist at the present day. This principle was to a small extent admitted by Mr. Sharpe in his previous volumes, when he allowed the existence of sub-species, or, as Mr. Seebohm names them, con-species. The great risk that we see in Mr. Seebohm's method lies in the fact that it affords too easy a solution for otherwise difficult problems, but we must remember that the author was himself witness to the inter-breeding of the Carrion Crow and the Hooded Crow in Siberia, and we know that this also takes place in certain parts of Great Britain. Having seen this with his own eyes, and brought back to this country a large series of the hybrids, it was only reasonable for him to suppose that other birds are also capable of hybridising, and we think that the author proves his case with regard to the two Blue Rock Thrushes (*Monticola cyanus* and *Monticola solitarius*), which in certain parts of China inter-breed; and it is most curious that the vast majority of the birds found in the winter quarters of the Eastern Blue Rock Thrush, from Burmah and Malaisia to the Mollucca Islands appear to be hybrids. According to the author, *Cettia cantans* and *Cettia minuta* also inter-breed, and produce an intermediate form which he calls *Cettia cantans minuta*, a re-introduction of trinomial nomenclature which we do not at all like. The intermediate form, too, appears to be principally found in the Island of Formosa, though also met with at Chefoo, on the mainland opposite Japan, while one of the other forms is an inhabitant of Japan, with the exception of one Formosan skin in the author's collection, and the other is said to breed in South China and Hainan. Of these three forms then we should suppose that the Formosan was the oldest bird from which the other two had developed themselves, but that they had not as yet become entirely separated as distinct species. We must wait for more evidence with regard to

the South African Chats, to some of which Mr. Seebohm has applied his principle of hybridisation, as we are not yet satisfied that the changes of plumage cannot be accounted for by the more natural process due to age or the season of the year. These few remarks will not, however, detract from the sterling merit of Mr. Seebohm's volume, which bears on every page the evidences of the careful and exhaustive work which the author bestows on every subject he handles. The eighteen coloured plates are beautiful examples of Mr. Keuleman's great talents as a natural history artist, and the colouring is much more satisfactory than in the last volume of this Catalogue, issued by the British Museum.

MAGNETIC AND AURORAL OBSERVATIONS IN HIGH LATITUDES[1]

LIEUTENANT WEYPRECHT, the noted leader of the Austrian Arctic Expedition of 1872-74, whose death is a great loss to science, recently published a little text-book embodying the results of his wide experience in Arctic observation of magnetic and auroral phenomena, which will be invaluable in pointing out to future observers the precautions and requirements which only actual experience of Arctic life can suggest, and the arrangements of apparatus and stores, which, once left behind, must be done without; frequently to the loss of opportunities for observation which do not recur. It would however be wearisome to the general reader to enter into details of Arctic work, and no one to whom the matter is of practical moment will omit reading the book itself. Some however of the precautions suggested give so vivid an idea of the difficulties and even the suffering which Arctic observers have to meet in the cause of science, that we cannot forbear a passing mention of them.

In magnetic observatories, where iron is rigidly tabooed, and uniformity of temperature is of the first importance, stoves are naturally out of the question. In winter, when the huts are thickly covered with snow, the temperature should never fall below $-20°$ C. ($-4°$ F.), which, as Weyprecht says, may be borne for some hours in suitable clothing without severe suffering. Good fur garments are naturally essential. The most difficult part is the sufficient protection of the feet, that found most effectual being very wide felt boots reaching to the knee, into which the feet, clad in thick woollen stockings, are packed with dry hay or straw. The hands are covered with thick woollen gloves, and whenever they are not in actual use are further protected by fur. In absolute determinations however, which have to be carried on in a separate hut, which is also used as an astronomical observatory, and hence more open to the weather, the cold is not only much more intense, but the fingers must be left bare, or at most covered with thin cotton gloves, on account of the delicacy of the instruments. On two occasions during the *Tegetthoff* Expedition such quantities of snow were driven into the observatory through the crevices of the shutters as, by loading one side of the telescope, actually to throw it off its pillar.

For absolute magnetic observations Weyprecht gives preference to Prof. Lamont's portable theodolite, which contains in itself everything necessary for the determination of declination and horizontal and vertical intensity. If however a fixed scale could be attached to the telescope for rapid readings the instrument would be still further improved.

With regard to northern light observations, Weyprecht repeats his important classification of auroral forms given in his *Nordlichtbeobachtungen*. For observations on the altitude of auroræ, with a view to calculation of height, he recommends a simple instrument consisting of a tube with an eye-piece, movable in the magnetic meridian, and with an altitude circle reading to $\frac{1}{2}°$. The tube must be attached to the end of the axis, so as to be capable of sweeping the entire meridian. The observations should be repeated at short and regular intervals, and both the upper and lower edges of the arches should be observed, thus giving at once the mean altitude and the breadth of the bands. If the "dark segment" is visible, its mean height and the azimuth of its summit must be observed, as it probably indicates the direction of the origin of the aurora. If a corona is formed the approximate position of its centre must be observed. Another method of determining the position of the corona is by measuring the direction of the rays of which the arches are formed. This is best done by measuring their inclination from the perpendicular in two azimuths 90° apart. If the tube we have mentioned be provided with an azimuth circle and cross-wires in the eye-piece with a position circle reading to $\frac{1}{2}°$ this is readily accomplished, the perpendicular being verified by observation of a plumb-line.

For spectral observations Weyprecht considers direct-vision instruments of good dispersion the most suitable. In low latitudes we have found a single bisulphide prism and simple slit and eye-hole without lenses to answer well, and if such an instrument were attached to the same axis as the measuring tube, which would act as a finder, we believe it would show fainter spectra than any direct vision arrangement. We do not know however how it would be affected by Arctic temperatures. Weyprecht does not mention any means of measuring the position of the lines—the simplest is Piazzi Smyth's comparison with the hydrocarbon spectrum of a spirit-lamp, and another very good scale is the band-spectrum of air yielded by a vacuum-tube fixed across the slit and made to flash as required.

Weyprecht insists on the importance of further comparisons between the movements of the aurora and magnetic disturbances, and points out the high interest that would also attach to observations of the earth-currents.

H. R. PROCTER

[1] "Praktische Anleitung zur Beobachtung der Polarlichter und der magnetischen Erscheinungen in hohen Breiten," von Carl Weyprecht, Schiffslieutenant. (Wien, 1881.)

NOTTINGHAM UNIVERSITY COLLEGE

THE fine building, auspiciously opened the other day as a college in the heart of Nottingham, represents the last development of that all too tardy interest in higher education which, in the more recent years, has originated the Victoria University in Manchester, the Yorkshire College in Leeds, the Science College in Newcastle (a flourishing offshoot of Durham University), the Mason Science College in Birmingham, and others.

It is gratifying to find in a new provincial centre, with its varied activities and the usual temptations associated with money-getting, an intelligent, if somewhat late, appreciation of the thirst after knowledge for its own sake, as well as for that to which it may be profitably applied, and a disposition to take generous means of satisfying it.

Of the inception and growth of the Institution we need not here speak at any length. The elements of a college were already in existence. For seven or eight years past lecturers from Cambridge have visited Nottingham and drawn large audiences. The Government Science Classes were also highly appreciated. It is thus estimated that no fewer than 1400 students will be ready to take advantage of the instruction soon to be provided. Nottingham, moreover, has possessed a public library since 1868, and this, along with the Natural History Museum, greatly needed larger accommodation. From the union and consolidation of these and other educational agencies under one roof where the conditions of progress are much more favourable, excellent results may be anticipated. A distinctive feature of the Nottingham College is that it has been built by the Municipal

Corporation of the place, is to be held as corporate property, and will be sustained mainly out of the corporate funds. With a total cost of 70,000*l*. (or, taking into account the value of the land, 100,000*l*.) the only endowments at present are the 10,000*l*. presented by an anonymous donor, and 300*l*. from Lady Ossington (for a scholarship). It is expected that the trustees of the late Mr. F. C. Cooper will, in accordance with his will, apply some part of his estate towards the endowment of classes in the College, but it is not at present known how much. Thus the expense of maintenance will, at least in the outset, mainly fall on the town itself. The experiment will doubtless be watched with interest.

The general internal arrangement of the new building may be here briefly noticed. The library-rooms are in the eastern wing, to the left of the principal front, and the natural history museum is housed in the other wing. The former include two reading-rooms on the ground and first floors. Behind the principal entrance are placed the three theatres for chemical, physical, and general lectures, the two former having laboratories, work-rooms, and professors' rooms attached. The largest theatre accommodates 600 persons, the chemical 220, and the physical 100. These rooms are well provided with modern appliances. In addition may be noted a balance-room, and an optical gallery 125 feet long for experiments in light.

It is stated in the report of the Organisation Committee that all persons will be admitted students who give evidence of their desire to improve their education and make advances in the acquisition of knowledge. More particularly the object of the founders of the institution seems to have been of a threefold character. First of all the College will absorb, as already indicated, the University Extension Lectures and Classes and the Government Science Classes, developing and systematising the courses of instruction in which these have been engaged. Next a technological school will be provided, and classes formed for teaching, in a more direct manner, the theoretical parts of certain trades. Once more, the preparation of students for residence at the older universities seems to have been contemplated; but this feature will probably, at least for some time, have little prominence.

For the purpose of systematic education the course of instruction has been arranged under four heads: (1) ancient and modern languages, literature, history, political philosophy and economy, logic, and philosophy; (2) mathematics, theoretical and applied mechanics, and physics; (3) inorganic and organic chemistry, pure and applied; (4) biological science, botany, zoology, and physiology; also geology and allied subjects. In the Government Science Classes (distinct from the College curriculum) instruction will be given in several of the subjects in which aid is given by the department at South Kensington. The Technological School will deal with the following among other subjects:—Cloth, cotton, silk, lace, and hosiery manufactures, weaving, metallurgy, gas manufacture, telegraphy, pottery and porcelain, bleaching, dyeing, and printing, tanning, mechanical engineering, oils, colours, and varnishes. It is to be distinctly understood, however, "that these classes are not so much for teaching trades as for teaching those subjects which underlie work and bear upon trade, and help to develop the intellect of the workmen."

The scheme of education provided will thus be seen to be of a comprehensive nature. All who are solicitous that England should take a good place among the nations in industrial competition will be glad to see a new technical school added to the few we already possess. The number of these schools will have to be greatly multiplied before we have anything like the advantages of Germany in this respect. In this connection we may direct attention to an interesting little volume recently written by Mr. Felkin (a native of Nottingham, by the way) who has carried on the manufacture of hosiery in Chemnitz, Saxony, since 1861, and describes what is being there done in the way of technical instruction, and its results. (Mr. Samuelson criticises the system in the *Fortnightly* this month.) The aspects of such technical education are various, and not the least in importance is that the workman, who is thereby enabled to feel an intelligent interest in his work, to comprehend the scientific principles on which it is based and the conditions of excelling in it, and to seek to do it as well as he possibly can, becomes conscious of mental growth and expansion. He even thus acquires new vistas (to use Prince Leopold's expression), and finds the drudgery of routine materially lightened. For those again who seek culture in different directions (scientific or literary) from that bearing on their daily work, a wide range of subjects is presented for choice. The cultivators of science for its own sake will doubtless not be wanting, and some excellent solid work, we trust, will be done. The youth seeking to be trained for a scientific career, and the working lad ardently pursuing some favourite study in his scanty hours of leisure, may alike resort to the College for stimulus and direction.

In the strong reaction which has become evident in recent years from that neglect of science which was so long prevalent among us, it has appeared to some that there is now an objectionable tendency to onesidedness in education. However this may be, the founders of the new College have determined, and we think wisely, that it should be more than merely a college of science and technical institute, and the purely literary elements of culture are included.

The requirements of the industrial population will be respected by the holding of classes in the evening, and the adoption of lighter fees than those for the day-students. The teaching will be conducted by resident professors, non-resident lecturers, and local teachers; and the student, after passing through the regular course of instruction and training, will, on passing an examination, receive a certificate.

The four professors required for the curriculum have been appointed as follows:—Professor of Language and Literature, Rev. J. E. Symes, M.A.; Professor of Mathematics and Mechanics, Dr. J. A. Fleming, B.A.; Professor of Chemistry, Dr. F. Clowes, F.C.S.; Professor of the Biological Sciences, Rev. J. F. Blake, M.A. One of the professors will act as Principal or Dean, with some extra emolument.

The University College of Nottingham, in fine, begins its career with good promise of usefulness, and it is to be hoped that wealthy and liberal friends of education will respond in a practical way to the appeal of the College Committee, who "desire it to be known that they are prepared to receive endowments in aid of the funds of the College." We hope soon to hear that the burden of maintenance for the townspeople has been thus happily diminished.

ANTHROPOLOGY[1]

TO those readers whose knowledge of ethnology or anthropology has been derived from a perusal of Prichard's "Natural History of Man," or the compilations of Wood, Brown, Peschel, or Brace, the present work will present a surprising amount of freshness and originality. They will in fact find themselves introduced to a new and very captivating science. Instead of the disconnected, and often confusing accounts of the numerous races, families, and tribes into which mankind have been divided, with separate details of the appearance, manners, customs, houses, implements, weapons, and ornaments of each, the reader of the present work will be shown how

[1] "Anthropology: an Introduction to the Study of Man and Civilisation, by Edward B. Tylor, D.C.L., F.R.S. With Illustrations. (London: Macmillan and Co., 1881.)

mankind may be studied in a logical, connected, and far more interesting manner, by the method of comparison, and by tracing the growth or development of those faculties which more especially distinguish him from the lower animals. Everywhere he will find proofs of the essential unity of man; whether in the close similarity of the forms of the stone implements and weapons found in the most remote parts of the earth, and among the most varied races; in the identity of signs and gestures, and the striking resemblances even among the most diverse languages; or in the wonderful similarity and often identity, of habits, customs, ideas, beliefs, and religions among all savages, and the curious way in which traces of these can often be found in the very midst of modern civilised society.

It is very difficult to give any adequate idea of a work of this kind, which, in a moderate compass, contains the essence and outcome of all modern research on the various branches of the study of man and civilisation; but we shall perhaps best exhibit its wide scope and systematic treatment by an enumeration of the subjects discussed in the several chapters, adding a few remarks or criticisms where called for.

The first chapter contains a brief sketch of what we learn from history, archæology, and geology, as to man's antiquity and early condition; and in the next we are shown man's relation to the lower animals both in bodily structure and mental characteristics. These two chapters might, with advantage, have been considerably enlarged, as they constitute the foundation, and, to many persons, the most interesting portions of the modern study of man. The results hitherto arrived at by these branches of study, are, besides, both suggestive and important, and might, we think, have been more expressly referred to. The numerous remains now discovered of prehistoric man, and of his works, dating back to an undoubtedly vast antiquity, show us in no case any important deviation from the existing human type, nor any indication that his mental status was lower than (if so low as) that of many living races. At the same time the increasing rudeness of his implements as we go back, undoubtedly indicates that we have made some approach towards the period when he first emerged from the purely brute state and became "a tool-using animal." We find him in the remote past surrounded by a number of huge mammalia, including many carnivora of greater size and destructive power than any that now exist, and we know that at a still earlier period these animals were even more abundant and more destructive; yet man must have held his own against them during the time when he had not yet begun to make tools or use fire. How did he do this without the possession of some additional natural weapons or faculties, of which nevertheless we find no trace in the earliest remains yet discovered? Again, the whole bearing of the evidence as to the development of man, indicates that the point of union or of common origin of man and the anthropoid apes, is enormously remote. Each of the existing types of these great apes possesses some specially human characteristic wanting in the others (for an enumeration of which see Mivart's "Man and Apes"), and this indicates that the common origin of these apes is of less remoteness than the common origin of them all and of man. How immensely remote, then, must be this point of common origin, and what a long and complex series of diverging forms must have existed, always in sufficient numbers to hold their own against their numerous competitors and enemies! The evolutionist *must* postulate the existence of this long series of divergent forms, yet notwithstanding the richness of the Tertiary deposits in many parts of the world no trace whatever of their actual existence has yet been discovered. The extreme remoteness of the origin of man is also shown by the facts, that neither the size nor the form of the cranium of the prehistoric races shows any inferiority to those of existing savages, while the approximate equality of their mental powers is shown by the ingenious construction of weapons and implements, and the artistic talent which we find developed at a period when the reindeer and the mammoth inhabited the south of France. It has been argued that the inferiority of the early implements shows mental inferiority, but this is palpably illogical. Did Stephenson's first rude locomotive—the *Rocket*—show less mind in its constructor than the highly-finished products of our modern workshops? Or were the Greeks mentally inferior to us because they had rude cars instead of locomotives, and had no clocks, water-mills, steam-engines, or spinning-jennies? It is forgotten that arts are a growth, and have little relation to the mental status of the artificer. A number of European infants brought up among savages would not, probably, in many generations, invent even the commonest implements and utensils of their ancestral homes; and it is difficult to say how slow may have been the development of the arts in their earliest and by far most difficult stages. It is therefore by no means impossible that the makers even of the palæolithic implements may have been fully equal, mentally, to existing savages of by no means the lowest type.

In the next chapter we have an excellent sketch of the chief races of man copiously illustrated by portraits, mostly from photographs and very characteristic. Among the best are those of the Andaman Islanders and the Dyaks, which we here reproduce. The Malays are less characteristic, this race being in fact better represented by the cut of the two Cochin Chinese at p. 98.

The four chapters on Language, whether manifested by gestures and signs, by articulate speech, by pictures, or by written characters, are exceedingly interesting and instructive, especially the account of the gesture language and the illustrations of how connected stories may be told to the deaf-and-dumb quite independently of any knowledge of alphabetical or even verbal signs. Picture-writing, as exhibited in the works of savages, in Egyptian hieroglyphics and in the modern Chinese characters, is also well explained, and is so interesting that one wishes the subject were more fully gone into. In treating of the origin of language Mr. Tylor doubts the sufficiency of the theory that emotional, imitative, and suggestive sounds were the basis on which all languages were founded, though he gives tolerably full illustrations of how roots thus obtained became modified in an infinite variety of ways to serve the growing needs of mankind in expressing their wants or their feelings. He impresses on his readers the important fact that language is always growing and that new words are continually made "by choosing fit and proper sounds." He shows how words once imitative or emotional have been often so changed and modified as to have their original character totally concealed; yet he concludes, that—"it would be unscientific to accept all this as a complete explanation of the origin of language" —because "other causes may have helped." It seems, however, to the present writer, that the imitative and emotional origin of language is demonstrated by a body of facts almost as extensive and complete as that which demonstrates the origin of species by natural selection; and that the "other causes" are in both cases exceptional and subordinate. As the examples of imitative words given by Mr. Tylor are comparatively trivial and altogether inadequate, it may be well to call attention to the wide and far-reaching character of such words, and to show how much of the force, expressiveness, and beauty of our language (as of most others) depends upon them.

Putting aside all mere representations of animal sounds —as the *whinny* of the colt, the *mew* of the cat, or the *bleat* of the sheep—let us consider what an immense number of natural sounds are named by words which we at once see to be appropriate representations or imitations of them. Such are—*crash, whizz, fizz, hiss, creak, whistle, rattle, bang, clang, flop, thud, clap, roar, snore,*

groan, *moan*, *wail*, *thunder*. In other cases sights, sounds, or feelings, are represented by their accompanying or appropriate sounds. We see a *splash*, or a *slop*; we feel a thing to be *smooth* or *rough*, or to *vibrate*; and we *shiver* with cold or *terror*. Again, how many actions and qualities are represented by words expressing the sounds which sometimes accompany them—as *knock*, *shock*, *crack*, *snap*, *ring*, *whisper*, *hush*, *sigh*, *sob*, *wash*, *squash*, *crush*, *crunch*, *rip*, *rend*, *grind*, *scratch*, *split*, *spit*, *cough*, *sneeze*, *wheeze*. How characteristic are such words as *sticky*, *flicker*, *flutter*, *hurry*, *flurry*, *stumble*, *hobble*, *wobble*. Here we have not only sound, but motion and quality, represented by the arrangement of letters and syllables. How clearly do such words as *slide*, *glide*, and *wave* imply slow and continuous motion, the movement of the lips while pronouncing the latter word being a perfect double undulation. How curiously do the tongue and palate seem to be pulled apart from each other while pronouncing the words *glue* or *sticky*. How marked is the contrast between the harsh consonants used to express *rough*, *rugged*, and *gritty*, as compared with the soft flow of sounds in *smooth*, *oily*, *even*, *polished*. Look again at

Andaman Islanders.

the sense of effort and feeling of grandeur in pronouncing the words *strong*, *strength*, *power*, *might*, as compared with the opposites, *weak*, *faint*; or the open-mouthed sounds of *grand*, *huge*, *monstrous*, *vast*, *immense*, *giant*, *gigantic*, as contrasted with the almost closed lips with which we say *small*, *little*, *tiny*, *minute*, *pigmy*, *midget*. So *crawl* and *drag* are pronounced slowly as compared with *run*, *fly*, or *swim*; while *difficult* and *easy* express their own meaning while we pronounce them. Many objects and substances have names curiously corresponding with their qualities. We have already noticed *glue* as indicating stickiness, but no less clearly is *oil* smooth; while *brass* and *glass* indicate resonance; *tin* a tinkling sound; *lead* and *wood* a dull sound or *thud*; in *bell* we imitate its sound, while the word *jelly* indicates the shaking of the substance. In *ice* we hear the interjectional *sh* of shivering with cold; in *fire* the flicker of the ascending flame. In other cases the motion of the breath gives an indication of meaning; *in* and *out*, *up* and *down*, *elevate* and *depress*, are pronounced with an inspiration and expiration respectively, the former being necessarily accompanied with a raising, the latter with a depression, of the head. When we name the *mouth* or *lips* we use labials; for *tooth* and *tongue*, dentals; for the *nose* and things relating to it, nasal sounds; and this peculiarity is remarkably constant in most languages, civilised and savage. Among the Malay races, for instance, we find such words as *mulut*, *bawa*, *mohou*, and *moda* for mouth; *gigit*, *nisiuen*, *nigni*, and *uiki* for teeth; and *idong*, *ugerun* and *usnut*, for nose. So in words for *large* we find a prevalence of broad sounds involving a wide opening of the mouth, as *busâr*, *bàkè*, *băgut*, *lâmu*, *elămo*, *ilăhé*, *eräămei*, *aiyuk*, *mäina*—and for *small*, words that are pronounced quickly and with slight opening of the lips, as *kichil*, *chili*, *kidi*, *köi*, *roit*, *keuni*, *anan*, *kiiti*, *fek*, *didiki*, all taken from languages of the Malay Archipelago.

Dyaks.

These few examples, which might be greatly increased, indicate the variety of ways in which, even now, after all the modifications and development which language has undergone, sound still corresponds to sense; and if the reader will turn to Dr. Farrar's suggestive little work on the "Origin of Language," he will find how wonderfully, by the help of analogy and metaphor, the uses and meanings of simple words and sounds have been indefinitely increased, so as to subserve the growing need of mankind to express more and more complex ideas. Mr. Tylor is rather unfortunate in his illustration of words for the form of which no cause can be assigned, when he says: "There is no apparent reason why the word *go* should not have signified the idea of coming, and the word *come* the idea of going." But, in accordance with the examples already

given, there *is* a very good and sufficient reason. We pronounce *come* with a closure and contraction of the lips and usually during inspiration, *go* with open and protruding lips and usually during expiration. Now many savages point with the lips as we do with the finger, signifying *there*, by protruding the lips in the direction to be indicated; and any one who has seen this curious gesture must be struck with its close similarity to the protrusion of the lips in pronouncing the word *go*. The same difference of the nearly closed or open lips characterises the words for these two ideas in many other languages. In French we have *viens* and *va*, in German *komm* and *geh*, in Italian *vieni* and *vai*, showing that words in distinct languages differing greatly in spelling and pronunciation may yet have a common character in the mode of speaking which indicates their common meaning.

The five following chapters treat of the Arts of Life, a subject which Mr. Tylor has to a great extent made his own, and which he discusses in a very interesting manner. The doctrine of development in the arts is however somewhat strained when it is implied that the modern gun is an outgrowth of the South American or Indian blowtube; while the origin of bank notes, and the account of the rise and progress of mathematics are hardly anthropology.

The next two chapters discuss the ideas of savage man as to the spirit-world, and the origin and development of myths; while the final chapter gives an admirable sketch of man as a social being, and of the development of that complex organism, Society. This thoughtful chapter cannot be epitomised, but the reader will find in it much curious information as to the sources of many of the customs, laws, and observances of civilised life, which are shown to be often traceable among the lowest savages. The following passage will serve to illustrate the author's style and treatment of his subject:—

"Much of the wrong-doing of the world comes from want of imagination. If the drunkard could see before him the misery of next year with something of the vividness of the present craving, it would overbalance it. Ofttimes in the hottest fury of anger, the sword has been sheathed by him across whose mind has flashed the prophetic picture of the women weeping round the blood-stained corpse. The lower races of men are so wanting in foresight to resist passion and temptation, that the moral balance of a tribe easily goes wrong, while they are rough and wantonly cruel, much as children are cruel to animals through not being able to imagine what the creatures feel. What we now know of savage life will prevent our falling into the fancies of the philosophers of the last century, who set up the 'noble savage' as an actual model of virtue to be imitated by civilised nations. But the reality is quite as instructive, that the laws of virtue and happiness may be found at work in simple forms among tribes who make hatchets of sharpened stones and rub sticks together to kindle fire. Their life, seen at its best, shows with unusual clearness the great principle of moral science, that morality and happiness belong together—in fact that morality is the method of happiness."

The reader who wishes to know what is the outcome of modern research into the nature, characteristics, and early history of man; and into his progress in the arts of life, in morality, and in social economy, will find a store of valuable information and much suggestive remark in this carefully-written but unpretending volume.

ALFRED R. WALLACE

NOTES

WITH regard to the forthcoming session of the American Association in Cincinnati, to begin August 17, we have to add to information already given (p. 146) that all the meetings, general and sectional, will be held under one roof, that of the Music Hall and Exposition Buildings. On the evening of the first day of meeting there will be a citizens' reception. An afternoon is to be devoted to visiting the Zoological Gardens. An exhibition of scientific apparatus, appliances and collections will be held during the Association meeting. The objects displayed will be kept over for the ninth Cincinnati Industrial Exposition in September. After the adjournment of the Association excursions will be organised on the Cincinnati Southern Railroad, and also, it is hoped, to the Mammoth Cave.

THE Prince of Wales is expected to lay the foundation-stone of the Central Institution of the City and Guilds of London Institute, on Monday next, at 3.30 p.m. His Royal Highness and the Princess of Wales were present at the Lord President's reception on Wednesday night at the South Kensington Museum. Prior to the reception the Prime Minister and several members of the Cabinet visited the Science Schools.

WE regret to announce the deaths of Dr. E. Zaddach, director of the Zoological Museum at Königsberg, who died on June 5 last; of Dr. Wilhelm Gottlob Rosenhauer, Professor of Philosophy at Erlangen University, who died on June 13, aged sixty-eight (on the same day on which Medical Science lost Josef Skoda at Vienna); of Dr. Matthias Jakob Schleiden, the well-known botanist, and author of many works on natural history (amongst which we may point out as standard works "Die Pflanze" and "Das Meer"), who died at Frankfort on June 23, aged seventy-seven; of Dr. Theodor Benfey, Professor of Philosophy at Göttingen University, a celebrated orientalist and linguist, whose death occurred on June 26 at the age of seventy-two; and of Dr. Rudolf Hermann Lotze, Professor of Philosophy at Berlin University, author of the "Mikrokosmus," who died on July 1, aged sixty-four years.

MESSRS. SIEMENS have received advice of the completion of the new Atlantic cable recently constructed by them. The reports of insulation and working speed are highly satisfactory. The cable connects Sennen Cove, Land's End, with Dover Bay, Nova Scotia, direct, a length of 2500 nautical miles.

M. PASTEUR has received the Grand Cross of the Legion of Honour.

M. WURTZ, the present president of the Academy of Science, has been appointed Life Senator by a very large majority. This highly-approved appointment raises to three the number of members of the Academy of Sciences who now belong to the Upper House of the French Republic; the two others are M. Robin and M. Dupuy de Lome. M. Berthelot, another member of the section of chemistry, has been proposed for a seat which is at present vacant, and his election is considered quite certain. A large number of the French senators belong to the other section of the Institute, which is now taking such a prominent part in French politics. This influence of the Institute was contemplated by M. Thiers, and his views are advocated principally by Mr. Barthélemy St. Hilaire, the Minister for Foreign Affairs and Member of the Academy.

SIXTY French members of the Congress of Electricians have been appointed by M. Cochéry, the Minister of Telegraphs, who has been made President. M. Ferry, Minister of Public Instruction, has been appointed Vice-President, and four other members of the Cabinet will be chosen by the Congress. The Academy of Sciences and other public scientific institutions will appoint their own representatives, as well as foreign nations.

WE have received the sixth report of Mr. Crookes and Professors Odling and Tidy, to the President of the Local Government Board, on the London Water Supply. It relates to the quality of water supplied from May 20 to June 30, and is highly favourable. "The results of our six months' work" (say the authors),

"and the examination during this period of 1127 samples, enable us to state that as an excellent drinking supply it [*i.e.* the water supplied to London] leaves nothing to be desired."

THE Royal Archæological Institute, of which Lord Talbot de Malahide is president, holds its annual congress at Bedford this year from Tuesday, July 26, to Monday, August 1. Elstow Church, Woburn Abbey, the Roman camp and amphitheatre near Horbury and Sandy, St. Alban's Abbey, and the ruins of Old Verulam, are among the places set out in the programme to be visited.

IN the Archives of the Observatory of Stockholm the assistant, M. Lindhagen, has made a highly interesting discovery. It consists of a copy of a treatise by Copernicus which is more complete than all those known hitherto, and which thus fills a gap in the works left by the great astronomer. The treatise bears the title, "Nicolai Copernici de hypothesibus motuum cœlestium a se constitutis commentariolus." It is bound with a copy of Copernicus' "De revolutionibus orbium cœlestium," which formerly belonged to Hevelius, the Danzig astronomer. The treatise, with an introduction by Lindhagen, will shortly be published in the *Transactions* of the Stockholm Academy of Sciences.

THE Annual Report of the Royal Society of New South Wales for 1880 states that thirty-six new members were elected during the year; the actual increase is twenty-two, and the present number of members 452. One honorary member, Sir J. D. Hooker, was elected; and Mr. Hyde Clarke, Major-General Sir E. Ward, and Mr. F. B. Miller were elected corresponding members. The Clarke Medal for 1881 has been awarded to Prof. McCoy of Melbourne University, for his distinguished researches in the Palæontology of Europe and Australia; (Prof. Owen, Mr. G. Bentham, and Prof. Huxley have been the three previous recipients). The finances of the Society are in a satisfactory condition. At the annual meeting on May 4, Prof. Smith, the retiring president, gave an address, in which he reviewed the twenty-five years of the Society's existence (eleven years of which it had the name of the Philosophical Society). Up to 1875 the Society had a somewhat chequered career. It entered the new building that year, and the tide of prosperity still enjoyed is largely due to the zeal and energy of Prof. Liversidge and Dr. Leibius, the secretaries then appointed. During the past year twenty-eight papers were read by thirteen members, many of them involving much laborious research. Mr. H. C. Russell was elected president for the coming year.

CONSIDERABLE progress has been made with the building for the zoological station at Watson's Bay, near Sydney, due to the initiative of Baron Maclay. The building is a neat cottage providing five workrooms and two bedrooms, besides storeroom and bathroom in the basement. It is not intended to make a charge for each table or workroom as at Naples; but every naturalist will be expected to contribute a small sum (say five shillings a week) towards paying a caretaker. Other current expenses will have to be met by subscriptions. The Government has agreed to double the subscriptions for establishing the station up to 300*l*. The Royal Society of New South Wales may be expected to carry forward the enterprise with spirit, and assistance has been promised by the Royal Society of Victoria and other bodies there.

THE French Goverument have sent a scientific expedition to Mesopotamia and Assyria. An architect and an engineer accompany the expedition.

AN Astronomical Congress will be held at Strassburg during September next, and will be visited by many eminent astronomers from all parts of the civilised world. Strassburg was chosen because in its new observatory the best and most modern astronomical apparatus are to be found.

IT is said that MM. Siemens have renounced the idea of constructing an electrical railway in Paris, as the Municipal Council has not granted to them the concession they required. We believe that the railway which is to be made will be worked with Faure accumulators.

WITH reference to Mr. Newberry's letter on American cretaceous flora, in our issue of 30th ult., we are requested to state that Mr. J. Starkie Gardner is at present travelling in Iceland, and his silence may be thus accounted for.

THE meeting of Austrian Anthropologists and Antiquaries will be held at Salzburg on August 12 and 13 next.

THE Committee of the Liebig Institution at Munich has made an award of two gold medals for 1881: one to Prof. G. Hanssen of Göttingen, the other to Prof. H. Settegast of Berlin, in recognition of their great merit in the furtherance of German agriculture.

THE Berlin Medical Society are now making preparations for celebrating the twenty-fifth anniversary of Dr. von Virchow's professorship. The celebration will probably take place on October 13 next, Virchow's sixty-first birthday.

AN International Alpine Congress will be held at Salzburg in the latter half of August, upon the occasion of the ninth annual meeting of the German and Austrian Alpine Club. The following subjects will probably be discussed:—(1) On the method of surveying Alpine territory and the multiplication of Alpine maps, with an exhibition of maps executed in Bavaria, England, France, Italy, Austria, and Switzerland. (2) On glaciers and the various methods of studying the same, with special reference to the observations at the Rhone glacier and to the glacier-book of the Swiss Alpine Club. (3) On the construction of shelter-huts and their interior arrangement, with an exhibition of models and plans.

AT Hermannstadt a Transsylvanian Carpathian Club has just been formed after the model of the German and Austrian Alpine Clubs. Its object is the scientific investigation of the Transsylvanian Alps with their glens and caves. The new club numbers a good many members already, and branches have been established at Broos, Kronstadt, Mühlbach, and Schässburg.

AT Leipzig a new Ornithological Society has been formed, which numbers amongst its objects the general protection of birds.

DURING the last days of May the whole neighbourhood of Kamenz was visited by strange guests. Enormous swarms of *Libellula quadrimaculata* passed over the valley, here and there in dense masses, and extending from five to ten miles in breadth. The first swarm arrived about noon on May 30, its passage occupied two hours; in the evening a second swarm came from the direction of Weisswasser. The third swarm arrived on the morning of the 31st. Swarms of this description have not been observed since June, 1825. At Dresden the strange phenomenon was also observed.

A BURIAL-PLACE has been discovered in the neighbourhood of Naumburg, which proves to be a rich store of antiquities. Up to the present some ninety clay vessels and bronze objects have been excavated, amongst which is a very large ash-urn.

IN a peat bog near Triebsee (Stralsund district) a discovery of flint implements has been recently made. Some four or four and a half feet below the surface eight lance-points and fourteen edge tools were found lying in a heap together. Although the whole neighbourhood and the Island of Rügen are rich in objects of the kind, the large size of the present specimens and

the perfection of the workmanship cause general surprise. The objects found have been forwarded to the Stralsund Museum.

THE Thuringian Fisheries Union had a meeting at Jena on June 7, when the Grand Duke was present. The proceedings proved the satisfactory progress of pisciculture in Thuringia.

A NEW commentary to Kant's "Kritik der reinen Vernunft" will be published by Spemann of Stuttgart, upon the occasion of the centenary of that great work. The author is Dr. H. H. Vaihinger of Strassburg, and the commentary will be in four volumes. It will give a detailed explanation of the text, a logical analysis of the contents, and an abstract of all the works published during the century with reference to Kant's masterwork.

SIMILAR devastations to those which we reported from the Caucasus some time ago are now caused in Turkey by grasshoppers. The Turkish Government is compelled to employ extraordinary measures to overcome the plague. A particularly voracious species has appeared in the Bodirum district (Smyrna), and the whole population is employed to combat the insects. At Angora all business was suspended for three days by order of the Governor-General, and all the inhabitants were ordered to march out into the fields to destroy the grasshoppers. Every inhabitant was compelled to deliver 20 oka (about ½ cwt.) of dead grasshoppers to the officials. The swarms are said to emanate principally from Persia.

THE Epping Forest and County of Essex Naturalists' Field Club has held three field-meetings this year. On April 2 the Club visited Waltham Abbey under the guidance of Mr. George H. Birch, who communicated a paper on this interesting building. On May 14th the Club united with the Geologists' Association for the purpose of visiting the chalk-pits at Gray's Thurrock. The conductor for the occasion was Mr. Henry Walker, F.G.S. Prof. Morris, who was also of the party, gave a series of most instructive addresses on the ground. The last field-meeting, held on June 25, was microscopical, the conductor being Mr. W. Saville Kent, F.L.S. The Club met at Chigwell, where they were hospitably entertained by the Rev. Linton Wilson, M.A., at Oakhurst. Mr. Kent read a paper entitled "Infusoria—what are they?—their Collection and Investigation." Field-meetings in conjunction with the Hertfordshire Naturalists' Field Club and with the Essex Archæological Society are under arrangement. The Essex Field Club has recently been making excavations in the ancient earthwork in Epping Forest known as Ambresbury Banks, under the superintendence of General Pitt-Rivers, F.R.S., who will shortly give an account of the results obtained.

IN the Poserna district, between Weissenfels and Lützen, saline springs have been discovered by the Mining Engineer, Herr C. Reyher of Halle. The spring near the village of Poserna comes from an old shaft which is said to be eighty yards deep, but is now filled with *débris*. The water is effervescent. Another spring was discovered near Stösswitz at a depth of 19 metres, and rose with such force that it could only be stopped with great difficulty. Some 100 yards from the latter a third spring was discovered. The water of the three springs is strongly saline, and as they contain principally potash salts, their discovery is valuable. It is now found that during the fifteenth century salt works existed in this neighbourhood.

M. THOREL, President of the General Council of the Seine, has received an official answer to his inquiries relating to the reasons why the Paris gas companies refused to sell their commodity to Parisian aëronauts. It is probable that an arrangement will soon be entered into, and that public ascents will be resumed shortly as in former years, under the supervision of the municipal authorities.

THE Kant Society at Königsberg inaugurated a chapel dedicated to the memory of Kant on June 19. The small Gothic building touches the cathedral on the north side and forms a fitting substitute for the "Stoa Kantiana," which has become useless since the opening of the new University building. The interior of the chapel is formed by a double cross vault. On the left the same tombstone is let into the floor which covered the grave in the "Stoa Kantiana," and which was originally presented by Herr Scheffner. Underneath this the remains of Kant are contained in a double zinc coffin.

AT Zamoly, in the Hungarian Comitat of Stuhlweissenburg, two tombs have been discovered which contained coins from the time of Diocletian. One was the grave of a boy, the other that of a very tall man. Interesting remains of bas-reliefs and portraits were found in the tombs. They were on pieces of wood; one shows the words "Bibite hoc."

THE *Journal Télégraphique* of Berne, the organ of international telegraphy, has expressed its approval of the proposal to establish an international code for the protection of submarine telegraph property, both during war and peace.

THE German Geometrical Society held its tenth annual meeting at Karlsruhe in the third week of June.

ON the 16th inst. an International Agricultural Exhibition will be opened at Hanover. The exhibitors number over 1600.

MR. MURRAY has in hand, and will shortly publish, the following works of interest to scientific men:—"The Land of the Midnight Sun," being an account of Summer and Winter Journeys through Sweden, Norway, Lapland, and Northern Finland; with Descriptions of the Inner Life of the People, their Manners and Customs, the Primitive Antiquities, by Paul B. du Chaillu. In two vols.; with map and numerous illustrations. "The Formation of Vegetable Mould through the Action of Worms," with Observations on their Habits. By Charles Darwin, F.R.S. "The White Sea Peninsula;" a Journey to the White Sea, and the Kola Peninsula. By Edward Rae. With illustrations. "The Life of Sir Charles Lyell;" with Selections from his Journals and Correspondence. Edited by his sister-in-law, Mrs. Lyell. With portrait, two vols.

DURING the demolition of some old buildings at 406 and 407, Oxford Street, last week, the workmen on reaching the foundations came on a quantity of old armour and weapons—helmets, breastplates, spears, swords, and daggers, some very curious in shape. On opening a stone vault they found also some plate, including church utensils, such as a monstrance and a chalice, the workmanship of which is thought to be of the fourteenth century. The monstrance had a Latin inscription.

AN Agricultural Exhibition will be held at Strassburg from September 11-18 next. Over 2000*l*. will be distributed in prizes.

THE additions to the Zoological Society's Gardens during the past week include a Weeper Capuchin (*Cebus capucinus*) from Brazil, presented by Mr. J. S. Chapperton; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. Arthur Tower; a Central American Agouti (*Dasyprocta isthmica*) from Central America, presented by Mr. A. Melhuish; two Pileated Jays (*Cyanocorax pileatus*) from La Plata, presented by Mr. A. A. Dawley; a Berg Adder (*Vipera atropos*) from South Africa, presented by Mr. Borrodaile Pillans; a Cullen's Eagle (*Aquila culleni*), South European, eight American Menobranchs (*Menobranchus lateralis*) from N. America, purchased; a Common Rhea (*Rhea americana*) from South America, on approval; a Reeves' Muntjac (*Cervulus reevesi*), born in the Gardens; two Scarlet Ibis (*Eudocimus ruber*), three Common Widgeons (*Mareca penelope*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE COMET.—By favour of Dr. W. L. Elkin of the Royal Observatory, Cape of Good Hope, we are able to subjoin observations of the great comet made at that establishment before the perihelion passage, which it is stated will admit of improvement when the places of the comparison-stars have been more accurately determined:

	Cape M.T. h. m. s.	R.A. ° ′ ″	Decl. ° ′ ″
May 31	6 19 24	75 46 25	−29 42 19
June 3	5 53 47	76 12 30	26 25 57
4	5 59 57	76 21 55	25 4 53
9	5 31 18	77 16 24	−15 44 53

From the positions on May 31, June 4 and 9, Dr. Elkin has calculated the following elements of the comet's orbit, by the side of which we place for comparison others deduced by Mr. Hind, from post-perihelion observations up to July 1:—

	ELKIN	HIND
Perihelion passage	June 16·29839 G.M.T.	June 16·41519 G.M.T.
Longitude, perihelion	264 55 13	265 9 4
,, node	270 54 27	270 58 0
Inclination	63 27 4	63 29 9
Log. perihelion dist.	9·866656	9·865516
	Direct.	Direct.

The longitudes are reckoned from the mean equinox of 1881·0. Mr. Hind's orbit gives the following expressions for the comet's heliocentric co-ordinates x, y, z to be used with the X, Y, Z of the *Nautical Almanac* in the calculation of geocentric right ascensions and declinations; they apply to apparent equinox 1881·5.

$$x = r[9{\cdot}65000] \sin. (v + 356^\circ\ 22'{\cdot}5)$$
$$y = r[9{\cdot}99187] \sin. (v + 243\ 20'{\cdot}7)$$
$$z = r[9{\cdot}96142] \sin. (v + 328\ 24'{\cdot}3).$$

Here, v is the true anomaly, r the radius vector, and the quantities within square brackets are logarithms.

We may take this opportunity of correcting a singular misstatement with which Admiral Mouchez, the director of the Observatory at Paris, is credited in the *Comptes rendus* of the Academy of Sciences. He is there made to say that the period of revolution of the comet of 1807, which had elements resembling those of the present comet, was found by Bessel to have been reduced to 174 years after he had taken account of "new perturbations." Any one who refers to Bessel's treatise will see that this is an error. Bessel fixed the period of revolution at 1713½ years, on September 22, 1807, and in continuing the computation of the perturbations by the planet Jupiter to March, 1815, when they had become very small, he found that the revolution had been diminished thereby, about 170 years; he considered that the period he had assigned for September 22, 1807, was not liable to a greater error than 100 years. In the *Comptes rendus* the effect of perturbation on the period appears to have been quoted, instead of the revolution itself, as perturbed to March, 1815. In the communication to which we refer, the identity of the comet of 1881 with that of 1807, is pretty distinctly assumed, but the weight of evidence is certainly in the other direction.

THE VARIABLE STAR U CEPHEI.—Prof. Julius Schmidt has published an ephemeris of the last variable star discovered by Ceraski in Cepheus, extending to the end of the present year. From his later observations he has found that the gradual increase of period which he had formerly suspected is not confirmed, and he now fixes the period at 2d. 11h. 49m. 33·35s. On August 18 commences a series of minima, which may be observed at intervals of something less than five days to the end of December: the following are the Greenwich mean times to the end of October:—

	h. m.		h. m.		h. m.
Aug. 18,	14 44·9	Sept. 12,	13 0·8	Oct. 7,	11 16·6
23,	14 24·0	17,	12 39·9	12,	10 55·8
28,	14 3·2	22,	12 19·1	17,	10 35·0
Sept. 2,	13 42·4	27,	11 58·3	22,	10 14·2
7,	13 21·6	Oct. 2,	11 37·5	27,	9 53·4

The letter T, at first used to designate this star, will be properly applied to another variable also discovered by Ceraski, the place of which for 1855·0 is in R.A. 21h. 7m. 33s., Decl. + 67° 54′·4, which seems to have a period of about 400 days, and was at a maximum on December 30, 1880.

PHYSICAL NOTES

IN the *Journal* of the Franklin Institute Prof. S. W. Robinson has recently described some experiments upon the effect produced upon sound-waves by repeated oblique reflections at membranes forming the boundary of two gaseous media. These experiments, as far as they go, would appear to show that after repeated reflection at oblique surfaces set in vertical planes a sound-wave acquires new properties by virtue of which it is reflected at another such oblique surface with an intensity which is a maximum if this surface be also in a vertical plane, and a minimum if the surface be equally inclined to the direction of propagation of the wave, but turned through a right angle about that direction as an axis. If this be indeed established as an experimental result it is equivalent to a proof that sound-waves can be *polarised* by reflection. The apparatus consisted of a series of L-shaped tubes of tin plate, one inch in diameter and three inches long, the parts joined at an obtuse angle, but having the outer angle cut away and covered by a thin membrane. This membrane was fixed so that a sound-wave coming in either direction should be incident on it at the angle of supposed maximum polarisation, the angle being calculated by Brewster's Law so that its tangent should represent the ratio between the velocities of wave-propagation in the two media, namely, coal-gas within, air without (14 : 11). The series of tubes was so set that at first the membranes were all parallel, and then a "pulse" was sent along the tube in the following manner:—The initial and final openings were closed by membranes stretched across the tubes orthogonally. Against each a small ball of ivory or glass was hung by a thread. The ball at one end was raised to a given height and dropped on to the membrane, and the impulse given to the ball at the other end was noted. Then the second half of the system was turned round so that the membranes of this portion were at right angles to their former position, and the ball was again dropped. The impulses received on the second ball were in general feebler when the second system, or "analyser," stood at right-angles to the first system, or "polariser," the diminution varying in different experiments from 16 to 38 per cent. No diminution was observable when the interior of the tube was filled with air instead of coal-gas. It remains to be seen whether the results are capable of being reproduced under other circumstances, or whether they are due to some mechanical peculiarity of Prof. Robinson's apparatus. Whether this be so or not we must absolutely reject the very unwarranted conclusion at which Prof. Robinson arrives, namely, that the vibrations of light as well as of sound are longitudinal until they are polarised, and that they become transversal only in the act of polarisation.

M. MERCADIER has examined the resistance of selenium at different temperatures, using for this purpose a photophonic receiver of selenium spread between the edges of platinum sheets, the resistance being measured in the dark by the ordinary bridge-method. The receiver, which had been well annealed, had at 15° C. a resistance of 54,000 ohms. This amount decreased as the temperature rose with great regularity to 36° C., when its resistance was less than 1500 ohms. From this point the diminution of resistance was less rapid; but at about 125° C. it had fallen to less than 500 ohms, rising slightly to 163° C., from which it again fell as the temperature was raised to 208°. These results accord with the earlier experiments of Werner Siemens.

M. LIPPMANN finds (*Jour. de Phys.*, May) that galvanic polarisation, which modifies so much the capillary properties of a metallic surface, causes no perceptible variation of its optical properties. He directed a beam of polarised light towards a platinum or silver mirror in acidulated water, or copper sulphate solution, and it was received, after reflection, in a Jamin quartz compensator, followed by an analyser-Nicol. The dark fringe in the compensator was not displaced when the mirror was included in the circuit of a Daniell or Bunsen element, and the current passed. Again, Newton's rings, formed by a glass plate on the platinum surface, and observed under various incidences, showed no displacement when the polarising current passed. (M. Lippmann adds some observations as to the mode of production of gas-bubbles by electrolysis.)

A FEW years ago M. Montigny called attention to the fact that the scintillation of stars is considerably increased during aurora borealis. Further data on this subject are afforded in a recent issue of the Belgian Academy's *Bulletin*, No. 3. *Inter alia*, he has observed that the phenomenon is more pronounced in winter than in summer, and that stars in the northern region

show the increased scintillation most. The author's general conclusion is that the effect is not one due to direct influence of the electro-magnetic light of aurora on the scintillation itself, but to disturbances (probably a cooling) which coincide with the appearance of aurora in the upper regions of the air, traversed by the star-rays.

AN acoustical apparatus for lecture purposes, devised by Dr. Maschke (*Wied. Ann.* No. 5), consists of a longish graduated piece of wood on adjustable supports, and having a longitudinal groove in in its upper surface, in which a glass tube is placed. At the end of the wooden piece is a screw arrangement supporting a thin steel rod which enters the tube, and has at its end a (vertical) ring with collodion membrane, against which hangs a little ball of shellac by a cocoon-fibre from the upper border. The tube may be made a closed one by means of a felt-covered piston. When the tube is sounded by means of a suitable tuning-fork, the shellac ball shows pendulum-motions, if not at a nodal point. The effects may be projected on a screen.

THE passivity of iron has been studied under new conditions by M. Bibart (*Jour. de Phys.*, May), and he considers it is not produced by a layer of insoluble sub-nitrate, as some physicists have thought, since the previous action of nitric acid is not necessary. Still less is it due to a formation of bioxide of nitrogen. It is produced by any cause which tends to oxidise iron, and destroyed by any cause which tends to deoxidise it. It is due then either to a layer of oxide or a layer of oxygen. The oxides formed on the surface may preserve it by their very presence, furnishing a sort of unattackable varnish, or they may preserve it like platinum, liberating, on the denuded parts, a protective layer of oxygen. The passivity of iron from contact of platinum seems to be produced at first by a simple layer of oxygen condensed on the platinum surface (a simple shock destroys it). But by degrees a layer of oxide is formed, and the iron then loses its passivity much less readily than before.

GEOGRAPHICAL NOTES

DR. SCHWEINFURTH, who left Suez for Socotra on February 23, returned on June 19. From the *Egyptian Gazette* we learn that his voyage to the island in a native vessel lasted one month (with calls). He was a month on the island. The rich flora yielded many new species. The mountains are well wooded, and covered with a more luxuriant vegetation than Mount Etna. The climate is exceptionally temperate, and the natives are inoffensive, greater security being experienced everywhere than in any part of uncivilised Africa On the approach of the south-west monsoon Dr. Schweinfurth had to leave, and in a miserable native vessel he tried to reach the Arabian coast opposite Socotra. It was found impossible to get to the east of Aden, and in H.M.S. *Dragon*, which was met, the explorer was conveyed to that place. Dr. Schweinfurth has brought with him a rich botanical collection, including some living plants which he will try to acclimatise in his garden at Cairo.

THE Polar relief ship *Rodgers*, Lieut. Berry, sailed from San Francisco on June 16 in search of the *Jeannette*. She will first cruise to St. Berry, then proceed to Petropaulovsky, then cross to St. Michael's in Alaska to coal, thence to St. Lawrence Bay and along the coast of Northern Siberia, making inquiries of the natives; then from Cape Serge to Kamon, where letters will be left with some natives; then north to Herald Island, hunting well over for cairns. It is intended next to go along the southern shore of Wrangel Land, and seek a harbour to winter in. The north-east and west coasts will be examined in sledge parties. Lieut. Berry means to return, if possible, by the second year.

INSTRUCTIONS have now been issued by Brigadier-General Hazen, of the U.S. Signal Office, to the commanding officers of the two expeditionary forces about to be sent out to establish permanent stations of observation in Polar regions. Lieut. Greely commands one of these forces, which goes to the neighbourhood of the coal-seam discovered near Lady Franklin Bay in 1875. The steamer will directly return (with a transcript of observations during the voyage), and the party landed will proceed to erect dwelling-houses and observatories, after which a sledge party will proceed to the high land near Cape Joseph Henry. It is intended that the permanent station shall be visited in 1882 and 1883 by a steam-sealer or other vessel with stores, &c., but directions in case of failure are supplied. Lieut. Ray commands the other expedition, which will sail from San Francisco for Point Barrow, Alaska Territory, and establish there a permanent station to be occupied till the summer of 1884 and visited annually. A great variety of observations will be made by both parties, and the instructions drawn out by the recent Hamburg Conference (with added notes from the U.S. Chief Signal Office) are furnished.

DR. GERARD ROHLFS delivered an interesting lecture at the meeting of the Berlin Gesellschaft für Erdkunde regarding his last journey in Abyssinia. He was received in a friendly manner by the inhabitants, and he is quite charmed with the hospitality of the Abyssinians. At Debratabor he was received kindly by Negus (Emperor) Johannes, and this was of great importance for his subsequent tour. Rohlfs travelled in a northerly direction to the ancient Portuguese capital Gondar, the residence of the late King Theodore. By means of a pass given to him by the ecclesiastical chief of the city he was able to inspect all the ancient historical buildings of the city. He continued his journey to the north, and after crossing the Takkaze River he reached Artum (Axum?), the ancient former royal residence, with its monuments dating from the period of Ptolemy. In April Rohlfs reached the Nassaur mountain chain, the boundary of Abyssinia, whence he proceeded to Cairo by boat.

THE expedition sent out by the French Secretary for Public Instruction to investigate the east coast of Finmarken has arrived at Vadso after a journey of twenty-one days. The expedition, which is under the command of M. Georges Pouchet, has for its special object to study the natural history of the Varanger Fjord, which is situated at the boundary of the Atlantic and Arctic Oceans. The keeper of the Christiania Royal Zoological Collections joined the expedition at Hammerfest.

THE Emperor of Germany has honoured Dr. Oscar Lenz, the well-known African traveller, with the Order of the Crown.

MR. SANDFORD FLEMING, C.M.G., Engineer-in-Chief of the Canadian Pacific Railroad, is to attend the International Geographical Congress at Venice in the interests of the question of standard time, coupled with which is that of a prime meridian. The former of these subjects is attracting much attention on the other side of the Atlantic, and a paper on it was lately read by Mr. Fleming before the American Society of Civil Engineers at a convention in Montreal, which brought out an interesting discussion, and a committee, consisting of leading railway officials in Canada and the United States, was appointed to examine and report at a future meeting. Dr. Barnard, the President of Columbia College, New York, is to represent the American Meteorological Society on the same subject at a meeting of the Association for the Reform and Codification of the Law of Nations, which is to be held at Cologne in August.

COL. PERRIER, a well-known French topographer, has lately been for some time engaged in survey work in the Regency of Tunis, and has prepared a map of the Kroumir country, which will shortly be published by the Dépôt de la Guerre.

MESSRS. S. CLARKE AND J. H. RILEY, of the China Inland Mission, whose station is at Chungking, formerly Mr. Colborne Baber's head-quarters, have each lately made important journeys in the Szechuen province. Mr. French, accompanied in one of his journeys by Mr. Broumton, has visited nearly every city in Eastern Yunnan, and has lately started again for the same region. In the course of their journey from Burmah Messrs. Soltan and Stevenson also made extensive journeys in the western part of the Yünnan province.

THE steamer *Nordenskjöld* is to start this week for the Gulf of Obi to the assistance of the *Oscar Dickson* and the *Nordland*. The crews of these vessels were all safe on April 23, when five men from the former left for Tobolsk, which they have lately reached.

A LETTER from Mussuca, on the Lower Congo, states that a Jesuit missionary expediti n has been set on foot to penetrate into the far interior. Père Argourd has been sent from the French mission station at Landana to organise a party of about thirty to proceed to Stanley Pool. He tried to procure carriers at Boma, Mussuca, and Noki, but not succeeding there, he went some thirteen miles inland to King Kangan Pecca, with a view to secure his aid. This was readily obtained by a truly missionary present of two gallons of rum, and Père Argourd returned with eight men and the promise that seventeen more should be sent shortly. The men however ran away the next day, and, yielding to circumstances, Père Argourd has resolved to proceed to Stanley Pool by M. de Brazza's route up the Ogowé.

MR. CRUDGINGTON, of the Baptist Congo Expedition, has just returned to England for a short time, and reports that by now Messrs. Comber and Bentley will have formed a first station at Isangila, Mr. Stanley's second post, and will at once push on to Mbu, some sixty or seventy miles further along the north bank of the Congo, where the the second station will be erected. It is for this navigable portion of the river that a steel boat is required, so as to avoid the Basundi. In his late journey up the Congo Mr. Crudgington found these people warlike and troublesome, as Mr. Stanley had done, and they were a source of perpetual anxiety to him. The practicability of utilising this part of the river is shown by the fact that Mr. Crudgington and his party went over it in heavy, clumsy, native canoes; but from Mbu the expedition will have to go to Stanley Pool by land, as the rapids and falls render the river quite unnavigable. The plans of the expedition are now on such an extended scale that six additional missionaries will be required—two for San Salvador, one for the depot at Mussuca on the Lower Congo, one each for Isangila and Mbu, and four for Stanley Pool, so that occasional journeys may be made higher up the Congo. The steel boat required by the expedition has been presented by an anonymous donor at a cost of about 400*l*. It has been built in London from drawings furnished by Mr. H. M. Stanley.

THE *Times* last week published an exceedingly interesting letter on trade and exploration on the Congo from a private correspondent at the mouth of the Ogowé. Speaking of M. Savorgnan de Brazza, the writer says that he has done much to open up the country between the Ogowé and the Congo, that he purchased a large tract of country near the sources of the former river at a very cheap rate, erected a station, and left a white man in charge. He is said to have purchased villages as they stood, freed a great many slaves, and engaged them at monthly wages to cultivate the plantations and keep the ground in order. He seems to have been regarded as the apostle of freedom in the country; troops of slaves came flocking to him to be freed, and his visit is regarded as having struck a blow at slavery in West Africa. The writer gives a very different picture of the state of affairs on the Belgian road along the north bank of the Congo. It may be interesting to mention, the observations respecting the light in which M. de Brazza is viewed by the natives are fully confirmed by a letter from a Roman Catholic missionary who accompanied him up the Ogowé last December.

WHIRLED ANEMOMETERS [1]

IN the course of the year 1872 Mr. R. H. Scott, F.R.S., suggested to the Meteorological Committee the desirability of carrying out a series of experiments on anemometers of different patterns. This suggestion was approved by the Committee, and in the course of the same year a grant was obtained by Mr. Scott from the Government grant administered by the Royal Society for the purpose of defraying the expenses of the investigation. The experiments were not however carried out by Mr. Scott himself, but were intrusted to Mr. Samuel Jeffery, then Superintendent of the Kew Observatory, and Mr. G. M. Whipple, then First Assistant, the present Superintendent.

The results have never hitherto been published, and I was not aware of their nature till on making a suggestion that an anemometer of the Kew Standard pattern should be whirled in the open air, with a view of trying that mode of determining its proper factor, Mr. Scott informed me of what had already been done, and wrote to Mr. Whipple, requesting him to place in my hands the results of the most complete of the experiments, namely, those carried on at the Crystal Palace, which I accordingly obtained from him. The progress of the inquiry may be gathered from the following extract from Mr. Scott's report in returning the unexpended balance of the grant:—

"The comparisons of the instruments tested were first instituted in the garden of the Kew Observatory. This locality was found to afford an insufficient exposure.

"A piece of ground was then rented and inclosed within the Old Deer Park. The experiments here showed that there was a considerable difference in the indications of anemometers of different sizes, but it was not possible to obtain a sufficient range of velocities to furnish a satisfactory comparison of the instruments. Experiments were finally made with a rotating apparatus, a steam merry-go-round, at the Crystal Palace, which led to some results similar to those obtained by exposure in the Deer Park.

"The subject has however been taken up so much more thoroughly by Doctors Dohrandt and Thiesen (*vide* "Repertorium für Meteorologie," vols. iv. and v.), and by Dr. Robinson in Dublin, that it seems unlikely that the balance would ever be expended by me. I therefore return it with many thanks to the Government Grant Committee.

"The results obtained by me were hardly of sufficient value to be communicated to the Society."

On examining the records it seemed to me that they were well deserving of publication, more especially as no other experiments of the same kind have, so far as I know, been executed on an anemometer of the Kew standard pattern. In 1860 Mr. Glaisher made experiments with an anemometer whirled round in the open air at the end of a long horizontal pole,[2] but the anemometer was of the pattern employed at the Royal Observatory, with hemispheres of 3·75 inches diameter and arms of 6·725 inches, measured from the axis to the centre of a cup, and so was considerably smaller than the Kew pattern. The experiments of Dr. Dohrandt and Dr. Robinson were made in a building, which has the advantage of sheltering the anemometer from wind, which is always more or less fitful, but the disadvantage of creating an eddying vorticose movement in the whole mass of air operated on; whereas in the ordinary employment of the anemometer the eddies it forms are carried away by the wind, and the same is the case to a very great extent when an anemometer is whirled in the open air in a gentle breeze. Thus, though Dr. Robinson employed among others an anemometer of the Kew pattern, his experiments and those of Mr. Jeffery are not duplicates of each other, even independently of the fact that the axis of the anemometer was vertical in Mr. Jeffery's and horizontal in Dr. Robinson's experiments; so that the greater completeness of the latter does not cause them to supersede the former.

In Mr. Jeffery's experiments the anemometers operated on were mounted a little beyond and above the outer edge of one of the steam merry-go-rounds in the grounds of the Crystal Palace, so as to be as far as practicable out of the way of any vortex which it might create. The distance of the axis of the anemometer from the axis of the "merry" being known, and the number of revolutions (n) of the latter during an experiment counted, the total space traversed by the anemometer was known. The number (N) of *apparent* revolutions of the anemometer, that is, the number of revolutions *relatively to the merry*, was recorded on a dial attached to the anemometer, which was read at the beginning and end of each experiment. As the machine would only go round one way the cups had to be taken off and replaced in a reverse position, in order to reverse the direction of revolution of the anemometer. The *true* number of revolutions of the anemometer was, of course, $N + n$, or $N - n$, according as the rotations of the anemometer and the machine were in the same or opposite directions.

The horizontal motion of the air over the whirling machine during any experiment was determined from observations of a dial anemometer with 3-inch cups on 8-inch arms, which was fixed on a wooden stand in the same horizontal plane as that in which the cups of the experimental instrument revolved, at a distance estimated at about 30 feet from the outside of the whirling frame. The motion of the centres of the cups was deduced from the readings of the dial of the fixed anemometer at the beginning and end of each experiment, the motion of the air being assumed as usual to be three times that of the cups.

The experiments were naturally made on fairly calm days, still the effect of the wind, though small, is not insensible. In default of further information, we must take its velocity as equal to the mean velocity during the experiment.

Let V be the velocity of the anemometer, W that of the wind, θ the angle between the direction of motion of the anemometer and that of the wind. Then the velocity of the anemometer relatively to the wind will be—

$$\sqrt{V^2 - 2VW \cos\theta + W^2} \quad . \quad . \quad . \quad . \quad . \quad (a)$$

The mean effect of the wind in a revolution of the merry will be different according as we suppose the moment of inertia of the anemometer very small or very great.

If, as is practically the case, W be small as compared with V, the correction to be added to V on account of the wind may be

[1] "Discussion of the Results of some Experiments with Whirled Anemometers." Paper read at the Royal Society, May 12, by Prof. G. G. Stokes, Sec. R.S.

[2] "Greenwich Magnetical and Meteorological Observations," 1862, Introduction, p. li.

shown to be $W^2/4V$ on the first supposition, and $3W^2/4V$ on the second.

Three anemometers were tried, namely, one of the old Kew standard pattern, one by Adie, and Kraft's portable anemometer Their dimensions, &c., were as follows:—

(α) *The Old Kew Standard.*—Diameter of arms between centres of cups 48 inches; diameter of cups 9 inches. Fixed to machine at 22 3 feet from the axis of revolution.

(β) *Adie's Anemometer.*—Diameter of arms between centres of cups 13·4 inches; diameter of cups 2·5 inches. Fixed to machine at 20·7 feet from the axis of revolution.

(γ) *Kraft's Portable Anemometer.*—Diameter of arms between centres of cups 8·3 inches; diameter of cups 3·3 inches. Fixed to machine at 19·10 feet from the axis of revolution.

With each anemometer the experiments were made in three groups, with high, moderate, and low velocities respectively, averaging about 28 miles an hour for the high, 14 for the moderate, and 7 for the low. Each group again was divided into two subordinate groups, according as the cups were direct, in which case the directions of rotation of the merry and of the anemometer were opposite, or reversed, in which case the directions of the two rotations were the same.

The data furnished by each experiment were: the time occupied by the experiment, the number of revolutions of the merry, the number of *apparent* revolutions of the anemometer, given by the difference of readings of the dial at the beginning and end of the experiment, and the space S passed over by the wind, deduced from the difference of readings of the fixed anemometer at the beginning and end of the experiment.

The object of the experiment was of course to compare the mean velocity of the centres of the cups with the mean velocity of the air relatively to the anemometer. It would have saved some numerical calculation to have compared merely the spaces passed through during the experiment; but it seemed better to exhibit the velocities in miles per hour, so as to make the experiments more readily comparable with one another, and with those of other experimentalists. In the reductions I employed 4-figure logarithms, so that the last decimal in V in the tables cannot quite be trusted, but it is retained to match the correction for W., which it seemed desirable to exhibit to 0·01 mile.

On reducing the experiments with the low velocities I found the results extremely irregular. I was subsequently informed by Mr. Whipple that the machine could not be regulated at these low velocities, for which it was never intended, and that it sometimes went round fast, sometimes very slowly. He considered that the experiments in this group were of little, if any, value, and that they ought to be rejected. They were besides barely half as numerous as those of the moderate group. I have accordingly thought it best to omit them altogether.

In the complete paper tables are then given containing the reduced results of the individual experiments, and from them the mean results for the high and moderate velocities are collected in the following table, in which are also inserted the mean errors:—

Anemometer.	Directions of rotation.	High velocities. Mom. inert. small. p. c.	m. e.	High velocities. Mom. inert. large. p. c.	m. e.	Moderate velocities. Mom. inert. small. p. c.	m. e.	Moderate velocities. Mom. inert. large. p. c.	m. e.
Kew	Opposite ...	122·6	2·4	121·9	2·3	115·1	4·9	113·2	5·2
	Alike	118·4	2·9	117·5	2·8	109·7	4·5	108·5	5·1
	Mean ...	120·5	...	119·7	...	112·4	...	110·8	
Adie.	Opposite ...	95·1	2·3	94·2	2·3	88·5	4·5	86·8	5·0
	Alike	98·0	6·5	97·3	6·5	82·6	7·3	81·0	7·3
	Mean ...	96·5	...	95·7	...	85·5	...	83·9	
Kraft.	Opposite ...	101·5	2·6	100·8	2·5	89·1	4·8	86·9	5·1
	Alike	100·8	1·2	99·4	1·3	87·8	5·0	86·0	6·0
	Mean ...	101·1	...	100·1	...	88·4	...	86·4	

The mean errors exhibited in the above table show no great difference according as we suppose the moment of inertia of the anemometer small or large in correcting for the wind. From the mean errors we may calculate nearly enough, by the usual formulæ, the probable errors of the various mean percentages for rotations opposite and alike. The probable errors of these mean percentages come out as follows:—

Kew,	1·0 for high velocities;	2·7 for moderate velocities.	
Adie,	1·5 ,, ,,	2·0 ,, ,,	
Kraft,	0·9 ,, ,,	1·8 ,, ,,	

These probable errors are so small that it appears that for the high and even for the moderate velocities the experiments are extremely trustworthy, except in so far as they may be affected by *systematic* sources of error.

It may be noticed that the difference of the percentages according as the directions of rotation of the anemometer and of the merry are opposite or alike is greatest for the Kew, in which the ratio of r to R is greatest, r denoting the radius of the arm of the anemometer, and R the distance of its axis from the axis of revolution of the machine, and appears to be least (when allowance is made for the two anomalous experiments in the group "Adie H +") for the Kraft, for which r/R is least. In the Kraft indeed the differences are roughly equal to the probable errors of the means. In these whirling experiments r/R is always taken small, and we might expect the correction to be made on account of the finiteness of R to be expressible in a rapidly converging series according to powers of r/R, say—

$$A' \frac{r}{R} + B'\left(\frac{r}{R}\right)^2 + C'\left(\frac{r}{R}\right)^3 + \cdots$$

We may in imagination pass from the case of rotations opposite to that of rotations alike, by supposing R taken larger and larger in successive experiments, altering the angular velocity of revolution so as to preserve the same linear velocity for the anemometer, and supposing the increase continued until R changes sign in passing through infinity, and is ultimately reduced in magnitude to what it was at first. The ideal case of $R = \infty$ is what we aim at, in order to represent the motion of a fixed anemometer acted on by perfectly uniform wind by that of an anemometer uniformly impelled in a rectilinear direction in perfectly still air. We may judge of the magnitude of the leading term in the above correction, provided it be of an odd order, by that of the difference of the results for the two directions of rotation. Unless therefore we had reason to believe that A' were 0, or at least very small compared with B', we should infer that the whole correction for the finiteness of R is very small, and that it is practically eliminated by taking the mean of the results for rotations opposite and rotations alike.

We may accept, therefore, the mean results as not only pretty well freed from casual irregularities which would disappear in the mean of an infinite number of experiments, but also, most probably, from the imperfection of the representation of a rectilinear motion of the anemometer by motion in a circle of the magnitude actually employed in the experiments.

Before discussing further the conclusions to be drawn from the results obtained, it will be well to consider the possible influence of systematic sources of error.

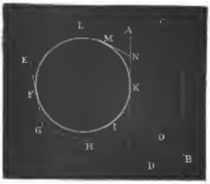
1. *Friction.*—No measure was taken of the amount of friction, nor were any special appliances used to reduce it; the anemometers were mounted in the merry just as they are used in actual registration. Friction arising from the weight is guarded against as far as may be in the ordinary mounting, and what remains of it would act alike in the ordinary use of the instrument and in the experiments, and as far as this goes, therefore, the experiments would faithfully represent the instrument as it is in actual use. But the bearings of an anemometer have also to sustain the lateral pressure of the wind, which in a high wind is very considerable; and the construction of the bearing has to be attended to in order that this may not produce too much friction. So far the whirled instrument is in the same condition as the fixed. But besides the friction arising from the pressure of the artificial wind, a pressure which acts in a direction tangential to the circular path of the whirled anemometer, there is the pressure arising from the centrifugal force. The highest velocity in the experiments was about thirty miles an hour, and at this rate the centrifugal force would be about three times the weight of the anemometer. This pressure would considerably exceed the former, at right angles to which it acts, and the two would compound into one equal to the square root of the sum of their squares. The resulting friction would exceed a good deal that arising from the pressure of the wind in a fixed anemometer with the same velocity of wind, natural or artificial, and would

sensibly reduce the velocity registered, and accordingly raise the coefficient which Dr. Robinson denotes by *m*, the ratio, namely, of the velocity of the wind to the velocity of the centres of the cups. It may be noticed that the percentages collected in the above table are very distinctly lower for the moderate velocities than for the high velocities. Such an effect would be produced by friction; but how far the result would be modified if the extra friction due to the centrifugal force were got rid of, and the whirled anemometer thus assimilated to a fixed anemometer, I have not the means of judging, nor again how far the percentages would be still further raised if friction were got rid of altogether.

Perhaps the best way of diminishing friction in the suppport of an anemometer is that devised and employed by Dr. Robinson, in which the anemomeser is supported near the top on a set of spheres of gun-metal contained in a box with a horizontal bottom and vertical side which supports and confines them. For vertical support this seems to leave nothing to be desired, but when a strong lateral pressure has to be supported as well as the weight of the instrument, it seems to me that a slight modification of the mode of support of the balls might be adopted with advantage. When a ball presses on the bottom and vertical side of its box, and is at the same time pressed down by the horizontal disk attached to the shaft of the anemometer which rests on the balls, it revolves so that the instantaneous axis is the line joining the points of contact with the fixed box. But if the lateral force of the wind presses the shaft against the ball the ball cannot simply roll as the anemometer turns round, but there is a slight amount of rubbing.

This however may be obviated by giving the surfaces where the ball is in contact other than vertical or horizontal direction.

Let A B be a portion of the cylindrical shaft of an anemometer; C D the axis of the shaft; E F G H I a section of the fixed box or cup containing the balls; L M N a section of a conical surface fixed to the shaft by which the anemometer rests on its balls; F I K M a section of one of the balls; F, I, the points of contact



of the ball with the box; M the point of contact with the supporting cone; K the point of contact, or all but contact, of the ball with the shaft. The ball is supposed to be of such size that when the anemometer simply rests on the balls by its own weight, being turned perhaps by a gentle wind, there are contacts at the points M, F, I, while at K the ball and shaft are separated by a space which may be deemed infinitesimal. Lateral pressure from a stronger wind will now bring the shaft into contact with the ball at the point K also, so that the box on the one hand and the shaft with its appendage on the other will bear on the ball at four points. The surface of the box, as well as that on the cone L N, being supposed to be one of revolution round C D, those four points will be situated in a plane through C D, which will pass of course through the centre of the ball.

If the ball rolls without rubbing at any one of the four points F, I, K, M as the anemometer turns round, its instantaneous axis must be the line joining the points of contact F, I, with the fixed box. But as at M and K likewise there is nothing but rolling, the instantaneous motion of the ball may be thought of as one in which it moves as if it were rigidly connected with the shaft and its appendage, combined with a rotation over L N A B supposed fixed. For the two latter motions the instantaneous axes are C D, M K respectively. Let M K produced cut C D in O. Then since the instantaneous motion is compounded of rotations round two axes passing through O, the instantaneous axis must pass through O. But this axis is F I. Therefore F I must pass through O. Hence the two lines F I, M K must intersect the axis of the shaft in the same point, which is the condition to be satisfied in order that the ball may roll without rubbing, even though impelled laterally by a force sufficient to cause the side of the shaft to bear on it. The size of the balls and the inclinations of the surfaces admit of considerable latitude subject to the above condition. The arrangement might suitably be chosen something like that in the figure. It seems to me that a ring of balls constructed on the above principle would form a very effective upper support for an anemometer whirled with its axis vertical. Possibly the balls might get crowded together on the outer side by the effect of centrifugal force. This objection, should it be practically found to be an objection, would not of course apply to the proposed system of mounting in the case of a fixed anemometer. Below, the shaft would only require to be protected from lateral motion, which could be done either by friction wheels or by a ring of balls constructed in the usual manner, as there would be only three points of contact.

2. *Influence on the Anemometer of its own Wake.*—By this I do not mean the influence which one cup experiences from the wake of its predecessor, for this occurs in the whirling in almost exactly the same way as in the normal use of the instrument, but the motion of the air which remains at any point of the course of the anemometer in consequence of the disturbance of the air by the anemometer when it was in that neighbourhood in the next preceding and the still earlier revolutions of the whirling instrument.

It seems to me that in the open air, where the air impelled by the cups is free to move into the expanse of the atmosphere, instead of being confined by the walls of a building, this must be but small, more especially as the wake would tend to be carried away by what little wind there might be at the time. On making some inquiries from Mr. Whipple as to a possible verticose movement created in the air through which the anemometer passed, he wrote as follows:—"I feel confident that under the circumstances the tangential motion of the air at the level of the cups was so small as not to need consideration in the discussion of the results. As in one or two points of its revolution the anemometer passed close by some small trees in full leaf, we should have observed any eddies or artificial wind had it existed, but I am sure we did not."

3. *Influence of the Variation of the Wind; first, as regards Variations which are not Rapid.*—During the twenty or thirty minutes that an experiment lasted there would of course be numerous fluctuations in the velocity of the wind, the mean result of which is alone recorded. The period of the changes (by which expression it is not intended to assert that they were in any sense regularly periodic), might be a good deal greater than that of the merry, or might be comparatively short. In the high velocities, at any rate, in which one revolution took only three or four seconds, the supposition that the period of the changes was large compared with one revolution is probably a good deal nearer the truth than the supposition that it is small.

On the former supposition the correction for the wind during two or three revolutions of the merry would be given by the formulæ already employed, taking for W its value at the time. Consequently the total correction will be given by the formulæ already used if we substitute the mean of W^2 for the square of mean W. The former is necessarily greater than the latter, but how much we cannot tell without knowing the actual variations. We should probably make an outside estimate of the effect of the variations if we supposed the velocity of the wind twice the mean velocity during half the duration of the experiment, and nothing at all during the remainder. On this supposition the mean of W^2 would be twice the square of mean W, and the correction for the wind would be doubled. At the high velocities of revolution, the whole correction for the wind is so very small that the uncertainty arising from variation as above explained is of little importance, and even for the moderate velocities it is not serious.

4. *Influence of Rapid Variations of the Wind.*—Variations of which the period is a good deal less than that of the revolutions of the whirling instrument act in a very different manner. The smallness of the corrections for the wind hitherto employed depends on the circumstance that with uniform wind, or even with variable wind, when the period of variation is a good deal greater than that of revolution of the merry, the terms depending on the first power of W, which letter is here used to denote the momentary velocity of the wind, disappear in the mean of a revolution. This is not the case when a particular velocity of wind belongs only to a particular part of the circle described by

the anemometer in one revolution. In this case there will in general be an outstanding effect depending on the first power of W, which will be considerably larger than that depending on W^2. Thus suppose the velocity of whirling to be thirty miles an hour, and the average velocity of the wind three miles an hour, the correction for the wind supposed uniform, or if variable, then with not very rapid variations, will be comparable with 1 per cent. of the whole; whereas, with rapid variations, the effect in any one revolution may be comparable with 10 per cent. There is, however, this important difference between the two: that whereas the correction depending on the square leaves a positive residue, however many experiments be made, the correction depending on the first power tends ultimately to disappear, unless there be some cause tending to make the average velocity of the wind different for one azimuth of the whirling instrument from what it is for another. This leads to the consideration of the following conceivable source of error.

5. *Influence of Partial Shelter of the Whirling Instrument.*— On visiting the merry-go-round at the Crystal Palace, I found it mostly surrounded by trees coming pretty near it, but in one direction it was approached by a broad open walk. The consequence is that the anemometer may have been unequally sheltered in different parts of its circular course, and the circumstances of partial shelter may have varied according to the direction of the wind. This would be liable to leave an uncompensated effect depending on the first power of W. I do not think it probable that any large error was thus introduced, but it seemed necessary to point out that an error of the kind may have existed.

The effect in question would be eliminated in the long run if the whirling instrument were capable of reversion, and the experiments were made alternately with the revolution in one direction, and the reverse. For then, at any particular point of the course at which the anemometer was more exposed to wind than on the average, the wind would tend to increase the velocity of rotation of the anemometer for one direction of revolution of the whirling instrument just as much, ultimately, as to diminish it for the other. Mere reversion of the cups has no tendency to eliminate the error arising from unequal exposure in different parts of the course. And even when the whirling instrument is capable of reversion it is only very slowly that the error arising from partial shelter is eliminated compared with that of irregularities in the wind; of those irregularities, that is to say, which depend on the first power of W. For these irregularities go through their changes a very great number of times in the course of an experiment lasting perhaps half an hour, whereas the effect of partial shelter acts the same way all through one experiment. It is very desirable therefore that in any whirling experiments carried on in the open air, the condition of the whirling instrument as to exposure or shelter should be the same all round.

The trees, though taller than the merry when I visited the place last year, were but young, and must have been a good deal lower at the time that the experiments were made. Mr. Whipple does not think that any serious error is to be apprehended from exposure of the anemometer during one part of its course and shelter during another.

From a discussion of the foregoing experiments it seems to me that the following conclusions may be drawn:—

1. That, at least for high winds, the method of obtaining the factor for an anemometer, which consists in whirling the instrument in the open air, is capable, with proper precautions, of yielding very good results.

2. That the factor varies materially with the pattern of the anemometer. Among those tried, the anemometers with the larger cups registered the most wind, or in other words required the lowest factors to give a correct result.

3. That with the large Kew pattern, which is the one adopted by the Meteorological Office, the register gives about 120 per cent. of the truth, requiring a factor of about 2·5, instead of 3. Even 2·5 is probably a little too high, as friction would be introduced by the centrifugal force, beyond what occurs in the normal use of the instrument.

4. That the factor is probably higher for moderate than for high velocities; but whether this is solely due to friction the experiments do not allow us to decide.

Qualitatively considered, these results agree well with those of other experimentalists. As the factor depends so much on the pattern of the anemometer it is not easy to find other results with which to compare the actual numbers obtained, except in the case the Kew standard. The results obtained by Dr. Robinson, by rotating an anemometer of this pattern without friction purposely applied are given at pp. 797 and 799 of the *Phil. Trans.* for 1878. The mean of a few taken with velocities of about 27 miles an hour in still air gave a factor 2·36, instead of 2·50, as got from Mr. Jeffery's experiments. As special anti-friction appliances were used by Dr. Robinson, the friction in Mr. Jeffery's experiments was probably a little higher. If such were the case the factor ought to come out a little higher than in Dr. Robinson's experiments, which is just what it does. As the circumstances of the experiments were widely different with respect to the vorticose motion of the air produced by the action of the anemometer in it, we may, I think, conclude that no very serious error is to be apprehended on this account.

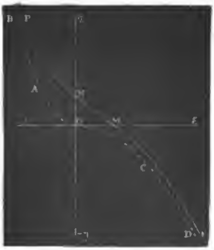
In a later paper (*Phil. Trans.* for 1880, p. 1055), Dr. Robinson has determined the factor for an anemometer (among others) of the Kew pattern by a totally different method, and has obtained values considerably larger than those given by the former method. Thus the limiting value of the factor m corresponding to very high velocities, is given at p. 1063 as 2·826, whereas the limiting value obtained by the former method was only 2·286. Dr. Robinson has expressed a preference for the later results. I confess I have always been disposed to place greater reliance on the results of the Dublin experiments, which were carried out by a far more direct method, in which I cannot see any flaw likely to account for so great a difference. It would be interesting to try the second method in a more favourable locality.

I take this opportunity of putting out some considerations respecting the general formula of the anemometer, which may perhaps not be devoid of interest.

The problem of the anemometer may be stated to be as follows:—Let a uniform wind with velocity V act on a cup anemometer of given pattern, causing the cups to revolve with a velocity v, referred to the centre of the cups, the motion of the cups being retarded by a force of friction F; it is required to determine v as a function of V and F, F having any value from 0, corresponding to the ideal case of a frictionless anemometer, to some limit F_1, which is just sufficient to keep the cups from turning. I will refer to my appendix to the former of Dr. Robinson's papers (*Phil. Trans.* for 1878, p. 818), for the reasons for concluding that F is equal to V^2, multiplied by a function of V/v. Let

$$V/v = \xi, \qquad F/V^2 = \eta,$$

then if we regard ξ and η as rectangular co-ordinates we have to determine the form of the curve, lying within the positive quadrant $\xi O \eta$, which is defined by those co-ordinates.



We may regard the problem as included in the more general problem of determining v as a function of V and F, where V is positive, but F may be of any magnitude and sign, and therefore v also.[1] Negative values of F mean, of course, that the cups, instead of being retarded by friction, are acted on by an impelling force making them go faster than in a frictionless anemometer, and values greater than F_1 imply a force sufficient to send them round with the concave sides foremost.

[1] Of course v must be supposed not to be so large as to be comparable with the velocity of sound, since then the resistance to a body impelled through air, or having air impinging on it, no longer varies as the square of the velocity.

Suppose now F to be so large, positive or negative, as to make v so great that V may be neglected in comparison with it, then we may think of the cups as whirled round in quiescent air in the positive or usual direction when F is negative, in the negative direction when F is greater than F_1. When F is sufficiently large the resistance may be taken to vary as v^2. For equal velocities v it is much greater when the concave side goes foremost than when the rotation is the other way. For air impinging perpendicularly on a hemispherical cup Dr. Robinson found that the resistance was as nearly as possible four times as great when the concave side was directed to the wind as when the convex side was turned in that direction (*Transactions* of the Royal Irish Academy, vol. xxii. p. 163). When the air is at rest and the cups are whirled round, some little difference may be made by the wake of each cup affecting the one that follows. Still we cannot be very far wrong by supposing the same proportion, 4 to 1, to hold good in this case. When F is large enough and negative, F may be taken to vary as v^2, say to be equal to $-Lv^2$. Similarly, when F is large enough and positive, F may be taken equal to $L'v^2$, where in accordance with the experiment referred to, L' must be about equal to 4 L. Hence we must have nearly—

$\eta = -L\xi^2$, when ξ is positive and very large;
$\eta = 4L\xi^2$, ,, negative ,, ,,

Hence if we draw the semi-parabola OAB corresponding to the equation $\eta = 4L\xi^2$ in the quadrant $\eta\,O - \xi$, and the semi-parabola OCD with a latus rectum four times as great in the quadrant $\xi\,O - \eta$, our curve at a great distance from the origin must nearly follow the parabola OAB in the quadrant $\eta\,O - \xi$, and the parabola OCD in the quadrant $\xi\,O - \eta$, and between the two it will have some flowing form such as PNMK. There must be a point of inflection somewhere between P and K, not improbably within the positive quadrant $\xi\,O\,\eta$. In the neighbourhood of this point the curve NM would hardly differ from a straight line. Perhaps this may be the reason why Dr. Robinson's experiments in the paper published in the *Phil. Trans.* for 1878 were so nearly represented by a straight line.

FELLOWSHIPS AT OWENS COLLEGE, MANCHESTER

A SCHEME of Science and Literature Fellowships, modelled very closely after the pattern of the Fellowship Scheme of the Johns Hopkins University, Baltimore, has been organised in Owens College, Manchester. The Council propose, early in October next, to appoint to five Fellowships on the terms and conditions following:—1. The appointment will be made by the Council, after receiving a report from the Senate, not on the results of examination, but after consideration of documentary or other evidence furnished to them. 2. Application by persons desiring to hold these fellowships must be made, in writing, on or before October 1. In his application the candidate should indicate the course of his previous reading and study, and his general purposes with reference to future work. 3. The candidate must give evidence of having received a sound and systematic education either in literature or in science, such as the possession of a degree of an English University, or a certificate from the authorities of an English School of Medicine or Science, of good repute, showing that he has passed through his curriculum with distinction, or, in default thereof, such other evidence as shall be satisfactory to the Council that he is qualified to prosecute some special study or investigation in the manner indicated in § 6. Finally, he should produce a satisfactory testimonial of character and conduct, and should give the names of not more than three persons from whom further information may be sought. 4. In the award of the Fellowships regard will be had to the pecuniary circumstances of the candidates. 5. The value of each Fellowship will be 100*l.* for the academical year 1881-82. In case of resignation or other withdrawal from the Fellowship, payment will be made for the time during which the Fellowship may have been actually held. 6. Every holder of a Fellowship will be expected to devote his time to the prosecution of some special study, with the approval of the Council after receiving a report from the Senate; and before the close of the year to give evidence of progress by the preparation of a thesis, the delivery of a lecture, the completion of some research, or in some other method. He will study under the direction of the Professor of the subject in which he is appointed, and will be required to pay such fees as the Council shall in each case determine. 7. He may be called on by the Council, after report from the Senate, to render some service to the College, either as an occasional examiner or by giving instruction in lectures or otherwise, to students in the College—provided always that he shall not, during his tenure of the Fellowship, hold any regular or salaried post as Assistant Lecturer or Demonstrator in the College—but he may not engage in teaching elsewhere. 8. He must reside in Manchester during the academical year. 9. He may be re-appointed at the end of the Session for a second and, in like manner, for a third year. 10. Candidates are invited to apply for appointment in any one of the following nine departments:—(1) Classics; (2) English Language and Literature; (3) History; (4) Philosophy; (5) Pure Mathematics; (6) Applied Mathematics (including Engineering); (7) Physics; (8) Chemistry; (9) Biology (including Physiology)

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 16.—"On Stratified Discharges. VI. Shadows of Striæ," by William Spottiswoode, P.R.S., and J. Fletcher Moulton, F.R.S.

One of the most interesting questions connected with the subject of stratified discharges is this: What is the physical, as distinguished from the electrical, nature of the striæ themselves? Are they, in fact, to be regarded as aggregations of matter possessing greater density than the gas present in the dark spaces, or are they to be considered as indicating merely special local electrical conditions? The fact of their having a definite configuration, especially on the side which is turned towards the negative terminal of the tube, that of their temperature being higher than that of the dark spaces, the manner in which they are affected by solid bodies, and other considerations, all tend to give support to the view that the striæ are loci of greater density than the dark spaces. Still it can hardly be said that as yet any experimental proof of this has been given sufficiently decisive to decide the question conclusively. And it is in the hope of contributing something towards the solution of this question that the following experiments are submitted to the notice of the Royal Society.

The two terminals of a Holtz machine were connected in the usual way with the two terminals of the tube, so as to produce a stratified discharge. A narrow strip of tin-foil, or a wire, was stretched along the tube opposite the column of striæ. The positive terminal of a second Holtz machine (in practice we used for this purpose a Töppler machine) was connected with the tin-foil, and the negative terminal with one (either) terminal of the tube. An air-spark, or interval across which sparks could pass, was interposed in the part of the circuit between the machine and the tin-foil. The effect of this arrangement was this: In the interval between two sparks the tin-foil and tube became charged like a Leyden jar; the tin-foil being the outer coating, charged positively, and the gas inside serving as the inner coating, charged negatively. When the spark passed across the interval mentioned above, the jar (*i.e.* the tube) became discharged, and the electricity previously held bound on the two coatings was set free.

When the first (say the "internal") machine was not working, or when it was disconnected, *i.e.*, when no regular discharge was passing through the tube, then, whenever a spark passed at the second (or "external") machine, a negative discharge with its accompanying Crookes' radiation took place from the inside of the tube next the tin-foil, and the opposite side of the tube became covered with a sheet of green phosphorescence (the tube being of German glass).

When, however, other things remaining as before, a discharge from the internal machine was sent through the tube, and a good stratified column was produced, it was found that the green phosphorescence was entirely cut off from the parts of the tube opposite to the striæ, while on the parts opposite to the dark spaces it remained, in the form of phosphorescent rings, as brilliant as before. The experiment was repeated with various tubes with various degrees of strength of current, and with various densities of gas (produced by heating a chamber of potash in connection with the tube). It may be added that when, as is sometimes the case, through greater exhaustion, the striæ became feebler in illumination and less compact in appearance, the shadows cast by them lost proportionally in sharpness of definition and in completeness of extinction of the phosphorescent light.

The brilliancy and definition of the phosphorescent rings may be increased by inserting a small Leyden jar in the circuit, care being taken that the jar shall discharge itself completely each time. If this is not the case the main discharge is followed by

subsidiary discharges, which tend to blur the effect. The angle of dispersion may be increased, or rather supplemented, by placing more than one strip on the tube, distant from one another by an angle of 90° or 120°. By this means the rings may be made to comprise the entire circumference of the tube.

It thus appears that the striæ are competent to cast shadows in the radiant showers issuing from the inside of the tube adjacent to the tin-foil, which part acts as a negative terminal. Many experiments have contributed to show that these radiant showers, although accompaniments of the discharge, are not carriers of the discharge; and that, having once issued from their source, they continue their own course irrespective of that of the discharge proper. They are in fact material showers, and, although not improbably charged with electricity, yet their ulterior course does not appear to depend on their electrical condition. Under these circumstances the simplest explanation appears to be that they have been arrested by a material obstacle, and consequently the phenomena above described may be considered as furnishing an experimental proof that the striæ represent local aggregations of matter, and not merely special electrical conditions of the gas.

June 16.—"On Stratified Discharges. VII. Multiple Radiations from the Negative Terminal." By William Spottiswoode, P.R.S., and J. Fletcher Moulton, F.R.S.

On examining the image of a negative terminal as traced out in tubes of great exhaustion, by the phosphorescence due to Crookes' radiations, we have often noticed that the image was not a simple figure, but that more than one outline of the contour of the terminal might be traced. From the fact of the double contour having been first remarked when the terminal was of a conical form, it was at first supposed that the second image might be due to internal reflection, or to some property appertaining to the edge of the cone. But this supposition led to no satisfactory explanation of the phenomenon.

It was however thought that, inasmuch as the two images implied different systems of radiation, a magnet suitably disposed might affect them in different degrees, and thereby throw some light on their origin. For this purpose we used a large electromagnet with its coils so coupled up as to give the two poles similar polarity. By bringing the two poles together, inclined at a moderate angle, a single pole and a field of great magnetic strength was produced.

The tube was then placed in the plane containing the axis of the two poles, and in the direction of a line bisecting their directions. The tubes first used were of great exhaustion, and were placed sometimes with the positive, sometimes with the negative terminal towards the magnet. When the tube was placed in a comparatively weak part of the field the two images of the cone were seen in their usual positions relatively to each other, except that they were slightly more separated. But as the tube was brought gradually into a stronger part of the field the two images became further separated, and by degrees a third, a fourth, and even more images were brought out on the side of the tube. In one tube of very high exhaustion, for which we are indebted to Mr. Crookes, as many as eight images became visible.

We have then, as an experimental fact, a series of images, each formed by a system of rays issuing from the surface of the negative terminal. The images being distinct, the system of rays must be distinct also. Now, as it seems hardly possible to imagine that, from every point of a surface, there can issue at one and the same instant of time a variety of systems of radiations, each system ranging over a finite angular distance, and each differently directed in space, we are driven to the conclusion that these radiations must have issued successively and not simultaneously from the terminal. In other words, the various images are formed in succession. Now, the entire series of images are present whenever a discharge passes through the tube; and when a "continuous" discharge (such as that from a Holtz machine) is passing, they are all as steady and as persistent as are any other features of the discharge. From this it follows that the radiations are not a continuous phenomenon, but that they are composed of a recurrent series of discharges, each having its own angular range, and its own direction in space; and as the electricity, which is the motive power, and the metallic terminal, which is the directing machinery, are the same in kind for each image, we are led to the conclusion that the positions of the images are determined by the force with which the radiations are projected. In fact, we understand that the various images are due to a succession of discrete discharges of successively diminishing strength.

The phenomenon of multiple images of the negative terminal as explained above has an important bearing on the nature of electrical discharges in vacuum tubes. For, if the phosphorising radiation consists of a recurring series of discrete discharges, the radiation in each series, and *à fortiori* the radiation as a whole, is discontinuous; and consequently the electrical discharge, to which it is due, must itself be discontinuous or "disruptive." We appear, therefore, in these phenomena to have an experimental proof, independent of and in addition to those adduced by Mr. De La Rue and others, of a fundamental point in the theory of these discharges, namely, their disruptive character.

Geological Society, June 8.—R. Etheridge, F.R.S., president, in the chair.—The meeting was made a special general meeting for the election of a Member of the Council in the room of the late Sir P. de Malpas Grey-Egerton, Bart., M.P., F.R.S., F.G.S.—The President announced that the late Sir Philip Egerton had bequeathed to the Society all the original drawings made from specimens in his collection for the illustration of Prof. Agassiz's works on Fossil Fishes. The Society had long possessed the drawings made for the same purpose from the Earl of Ellesmere's collection, and some years ago the Earl of Enniskillen presented those which had been prepared from specimens in his possession. Sir Philip Egerton's kind bequest would complete this interesting series.—Sir John Lubbock, Bart., M.P., F.R.S., was elected a new Member of Council. Messrs. Grenville A. J. Cole and J. L. Roberti were elected Fellows, and Il Commendatore Quintino Sella of Rome a Foreign Member of the Society.—The following communications were read:—The reptile-fauna of the Gosau formation, preserved in the Geological Museum of the University of Vienna, by Prof. H. G. Seeley, F.R.S., with a note on the geological horizon of the fossils, by Edward Suess, F.M.G.S. The collection of reptiles described in this paper was obtained at Neue Welt, near Wiener Neustadt, by tunnelling into the freshwater deposits which there yield coal. A part of the collection was described by Dr. Bunzel in 1871; but the author's interpretation of the fossils rendered a re-examination of the whole collection necessary. All the species hitherto discovered are new, and, with the exception of those referred to *Crocodilus*, *Megalosaurus*, *Ornithochirus*, and *Emys*, are placed in new genera. Nearly all the bones are more or less imperfect. The *Iguanodon Suessii*, of Bunzel, was referred to a new genus, *Mochlodon*, characterised by the straight anterior end of the ramus of the lower jaw and by the vertical bar in the middle of the teeth of the lower jaw. There appear to be two teeth in the ramus. The tooth referred to the upper jaw has several uniform parallel vertical bars. A small parietal bone, referred by Bunzel to a lizard, is considered by the author to belong probably to the same species, and, with some doubt, he associated with it the articular end of a small scapula. Bunzel's *Struthiosaurus Austriacus* was re-described by the author, who indicated that the bones of the base of the brain-case, regarded by Bunzel as the quadrate bones, really belong to the occipital region, which necessitates a different interpretation. The foramina along the base of the skull were also described as presenting one of the characteristics of the Dinosaurian order. The base of the skull of *Acanthopholis horridus* was described to show its relation to the above type, with the view of demonstrating its Scelidosaurian affinities. The greater part of the remains were referred by the author to a new genus, *Crataeomus*; some of these had been figured by Bunzel as "Crocodili ambigui," and others as belonging to *Scelidosaurus*, and to a new Lacertilian genus, *Danubiosaurus*. To *Crataeomus* he referred mandibles, teeth, vertebræ from all parts of the column except the sacrum, dermal armour, and the chief bones of the limbs. Two species were distinguished, *C. Paulowitschii* and *C. lepidophorus*. The former, which is much the larger, was named in honour of M. Paulowitsch, who voluntarily superintended the work at the Neue Welt. The author stated that he regarded these animals as carnivorous, and that, unlike the typical Wealden Dinosaurs, they were not kangaroo-like in habit, but had strongly developed fore limbs, as indicated in the proposed generic name. Two teeth belonging to *Megalosaurus* were described as representing a new species, *M. Pannoniensis*, characterised by the crown being shorter and broader than in previously described forms. A fragment, regarded by Bunzel as the thoracic rib of a lizard, was interpreted as the distal end of the femur of a Dinosaur, and named *Ornithomerus gracilis*. The lower jaw, described by Bunzel as *Crocodilus carcharidens*, of which a maxillary bone also occurs, was made the basis of a new genus, *Doratodon*,

probably Dinosaurian, judging from the lateral position of the apertures of the skull and the characters of the teeth. The genus *Rhadinosaurus* was founded upon the humerus and femur, the latter having been regarded by Bunzel as the dorsal rib of a crocodile; the species was named *R. alcimus*. *Oligosaurus adelus* was described as presenting Lacertilian characters in combination with some Dinosaurian peculiarities. The remains include the humerus, femur and scapula, and two vertebræ, which were regarded by Bunzel as fœtal vertebræ of a Dinosaur. The genus *Hoplosaurus* was founded on some vertebræ, fragments of limb-bones, and dermal armour; it shows, with distinctive peculiarities, a certain resemblance to *Hylæosaurus*. A procœlian crocodile was represented by many parts of the skeleton; some figured by Bunzel as Lacertilian, others as Crocodilian. It is remarkable for having a buttress supporting the transverse process in the lumbar region. The author calls it *Crocodilus proavus*. The specimen figured by Bunzel as the ilium of his *Danubiosaurus anceps*, was stated by the author to be a costal plate of a large Chelonian, in which, apparently, the margins of these plates remained separate through life. Skull bones, believed to belong to the same animal, are strongly sculptured; the author named the species *Pleuropeltus lissus*. Three or four species of Emydians were said to be indicated by isolated plates, the largest of which was named *Emys Neumayri*. The only specimen referable with certainty to a lizard is a small vertebra of elongated form, regarded as indicating a new genus and species, named *Spondylosaurus gracilis*. Of Pterodactyls there are but few remains; but these certainly represent two genera. The author only describes one species, to which he gives the name of *Ornithochirus Bunzeli*. There are, in all, probably ten genera of Dinosaurs, and five genera of other groups, making fifteen in all. The paper was supplemented by a note by Prof. Suess on the geological relations of the beds at Wiener Neustadt to those of the Gosau Valley, in which he comes to the conclusion that they are older than the true Turonian deposits, and especially older than the zone of *Hippurites cornu vaccinum*.—On the basement-beds of the Cambrian in Anglesey, by Prof. T. McKenny Hughes, M.A., F.G.S. In this paper the author first pointed out that there was in Anglesey:—(1) An upper slaty group, in which he had fixed two live zones, which showed that the series belonged to the Silurian (Sedgwick's classification), and (2) a lower group of slates and sandstones in which Arenig fossils had been found in several localities, and Tremadoc had been less clearly recognised, while by the correction of the determination of a species of *Orthis*, there was now a suspicion of even Menevian forms. These all rested upon the basement-beds of the Cambrian, of which the paper chiefly treated. They were made up of conglomerates, grits, and sandstones, with Annelids and Fucoids. The basement-beds varied in thickness and character according to the drift of currents along the pre-Cambrian shore and the material of the underlying rocks. Near Penlon, where they rested on a quartz-felspar rock, they consisted chiefly of a quartz-grit and conglomerate, almost exactly like that of Twt Hill. Near Llanerchymedd, where there was a mass of greenish schistose rock succeeding the Dimetian, the Cambrian basement-bed contained a large number of fragments of that rock, certain bands being chiefly composed of it. Near Bryngwallen, where the underlying Archæan consisted of gneissic rocks, the Cambrian basement-beds were made up of quartz conglomerate. Tracing it still further to the south-west he found bosses of conglomerate among the sand dunes of Cymmeran Bay, full of fragments of green schistose rock like that of Bangor, and telling of the further development of Pebidian at the south-west end of the Anglesey axis. In several localities these conglomerates were associated with and passed into fossiliferous grits and sandstones. He exhibited slices of the more important rocks, which he showed confirmed the results arrived at from other evidence. He pointed out that the observations now made confirmed the views he had expressed on a former occasion with regard to the basement-beds of the Cambrian between Caernarvon and Bangor, where the deposits which rested upon the granitoid rocks of Twt Hill were either a kind of arkose or chiefly composed of quartz with a few pieces of mica-schist and jasper; but as he followed them a few miles to the north-east he found that the quartz had got pounded into smaller grains, and the larger pebbles were chiefly of felsite, which here formed the shore, while further towards Bangor fragments of the still higher Bangor volcanic series helped to make up the Cambrian shingle beach.—Description and correlation of the Bournemouth beds. Part II. Lower or freshwater series, by J. S. Gardner, F.G.S. This was in continuation of a former paper by the author (*Q.J.G.S.* vol. xxxv. p. 209). The beds described are exposed east and west of Bournemouth and near Poole harbour, over a distance of about four miles. The author referred them to the Middle Bagshot, and stated that they are distinguished from the Lower Bagshot by the absence of the extensive pipe-clay deposits and the presence of brick-earths, and from the overlying beds by the absence of flints. They reach their extreme limit in the western area of the London basin, and are represented by the lignitic beds 19-24 of Prof. Prestwich's section. Lignites can be traced partly across the bay. The cliffs present an oblique section across a delta divisible roughly into four masses, one of which, from its confused bedding and want of fossils, is supposed to have been formed by the silting up of the main channel. The total thickness of the series was estimated at 600 to 700 feet. The inferences drawn by the author were as follows:—(1) From the beds cut through showing a steep side to the west, that the river flowed from that direction; (2) from the absence of boulders or coarse sediment, that the area was flat; (3) from the absence of lignite, that there were catchment basins; (4) from the absence of flint, and the quartzose nature of the beds, that no chalk escarpments were cut through, and that the deposits came from a granitic area; and (5) from the presence of wood bored by *Teredo* that the beds belong to the lower part of the river in proximity to tidal water. The flora was stated to be confined to local patches of clay. Those at the western end of the section are very rich, and distinguished from the rest by absence of palms and rarity of ferns. The beds near Bournemouth are still richer and very distinct; those east of Bournemouth are characterised by *Eucalypti*, Aroids, and *Araucariæ*; and those at the western end of the section by abundant Polypodiaceæ. It is remarkable that nearly every patch contains a flora almost peculiar to it; but the flora as a whole seems to pass upward to the Oligocene, but not down to the Lower Bagshot.

Sanitary Institute of Great Britain, June 21.—Dr. A. Carpenter in the chair.—A paper was read by Prof. W. H. Corfield, M.A., M.D., on the present state of the sewage question. In the discussion which followed Mr. W. C. Sillar, Mr. E. F. Bailey Denton, Mr. Douglas Onslow, Mr. R. W. P. Birch, Mr. G. B. Jerram, and Mr. Wilson Grindle took part. The Chairman made a few remarks relative to the successful working of the sewage farm at Croydon, and Prof. Corfield replied briefly to some of the points raised in the discussion.

CONTENTS

THURSDAY, JULY 21, 1881

INHERITANCE

THE tendency in any new character or modification to reappear in the offspring at the same age at which it first appeared in the parents or in one of the parents, is of so much importance in reference to the diversified characters proper to the larvæ of many animals at successive ages, that almost any fresh instance is worth putting on record. I have given many such instances under the term of "inheritance at corresponding ages." No doubt the fact of variations being sometimes inherited at an earlier age than that at which they first appeared—a form of inheritance which has been called by some naturalists "accelerated inheritance"—is almost equally important, for, as was shown in the first edition of the "Origin of Species," all the leading facts of embryology can be explained by these two forms of inheritance, combined with the fact of many variations arising at a somewhat late stage of life. A good instance of inheritance at a corresponding age has lately been communicated to me by Mr. J. P. Bishop of Perry, Wyoming, N.Y., United States:—The hair of a gentleman of American birth (whose name I suppress) began to turn grey when he was twenty years old, and in the course of four or five years became perfectly white. He is now seventy-five years old, and retains plenty of hair on his head. His wife had dark hair, which, at the age of seventy, was only sprinkled with grey. They had four children, all daughters, now grown to womanhood. The eldest daughter began to turn grey at about twenty, and her hair at thirty was perfectly white. A second daughter began to be grey at the same age, and her hair is now almost white. The two remaining daughters have not inherited the peculiarity. Two of the maternal aunts of the father of these children "began to turn grey at an early age, so that by middle life their hair was white." Hence the gentleman in question spoke of the change of colour of his own hair as "a family peculiarity."

Mr. Bishop has also given me a case of inheritance of another kind, namely, of a peculiarity which arose, as it appears, from an injury, accompanied by a diseased state of the part. This latter fact seems to be an important element in all such cases, as I have elsewhere endeavoured to show. A gentleman, when a boy, had the skin of both thumbs badly cracked from exposure to cold, combined with some skin disease. His thumbs swelled greatly, and remained in this state for a long time. When they healed they were misshapen, and the nails ever afterwards were singularly narrow, short, and thick. This gentleman had four children, of whom the eldest, Sarah, had both her thumbs and nails like her father's; the third child, also a daughter, had one thumb similarly deformed. The two other children, a boy and girl, were normal. The daughter, Sarah, had four children, of whom the eldest and the third, both daughters, had their two thumbs deformed; the other two children, a boy and girl, were normal. The great-grandchildren of this gentleman were all normal. Mr. Bishop believes that the old gentleman was correct in attributing the state of his thumbs to cold aided by skin disease, as he positively asserted that his thumbs were not originally misshapen, and there was no record of any previous inherited tendency of the kind in his family. He had six brothers and sisters, who lived to have families, some of them very large families, and in none was there any trace of deformity in their thumbs.

Several more or less closely analogous cases have been recorded; but until within a recent period every one naturally felt much doubt whether the effects of a mutilation or injury were ever really inherited, as accidental coincidences would almost certainly occasionally occur. The subject, however, now wears a totally different aspect, since Dr. Brown-Séquard's famous experiments proving that guinea-pigs of the next generation were affected by operations on certain nerves. Mr. Eugène Dupuy of San Francisco, California, has likewise found, as he informs me, that with these animals "lesions of nerve-trunks are almost invariably transmitted." For instance, "the effects of sections of the cervical sympathetic on the eyes are reproduced in the young, also epilepsy (as described by my eminent friend and master, Dr. Brown-Séquard) when induced by lesions of the sciatic nerve." Mr. Dupuy has communicated to me a still more remarkable case of the transmitted effects on the brain from an injury to a nerve; but I do not feel at liberty to give this case, as Mr. Dupuy intends to pursue his researches, and will, as I hope, publish the results.

July 13 CHARLES DARWIN

VOLCANOES

Volcanoes: what they are, and what they Teach. By John W. Judd, F.R.S., Professor of Geology in the Royal School of Mines. (London: C. Kegan Paul and Co., 1881.)

ONE of the fathers of vulcanology in this country was the late Mr. Poulett Scrope, in whose well-known treatise on Volcanoes, the subject of their cause and effect was for the first time discussed from a thoroughly philosophical standpoint. A great traveller and investigator himself, he strove to imbue younger geologists with his spirit, and when he became too old and infirm to undertake travel and research in distant countries, he directed some chosen disciples to prosecute his favourite lines of thought. Prof. Judd was one of these, and upon him has assuredly fallen the mantle, and a portion of the spirit of his master. His able papers on the study of volcanoes, contributed to the *Geological Magazine*, are well known to every vulcanologist. He has travelled much; he makes good use of both pen and pencil, and he is an accurate observer. We are glad that he has condensed his reading and research into a work, which becomes so widely distributed, both at home and abroad, as the volumes of the International Scientific Series invariably do.

Before entering more minutely into a discussion of the work, we would venture to say that among its few defects, that which strikes us most prominently is an insufficiency of logical sequence and method. The facts are multitudinous; carefully selected, but not carefully arranged. They require to be grouped; to be classified, and each set of facts to be set in apposition to the generalisation which they tend to prove. It is indeed a useful mental discipline for the reader to do this for himself, but unless he starts with some knowledge of the subject, and as the

possessor of a thoroughly methodical habit of mind, he can scarcely hope to arrange all the facts as they should be.

After an introductory chapter on the general nature of the inquiry instituted in the succeeding chapters, the author discusses "the nature of volcanic action." To illustrate this he takes the case of the ever-active volcano Stromboli, first examined scientifically by Spallanzani in 1788, and by Mr. Judd in 1874. It is a conical mountain rising 3090 feet above the sea, but the shore slopes to a depth of nearly 600 fathoms, hence the real height of the mountain from the bottom of the ocean exceeds 6000 feet. On the upper side of the crater a spot exists from which it is possible to look down upon the floor of the crater, and here may be seen apertures in which three classes of action take place. From some high-pressure steam is emitted in loud puffs; from others masses of molten lava well out; and in the third kind, a semi-liquid substance may be seen heaving up and down. Sometimes it rises as a kind of scum, swollen by the steam beneath it, and at last a gigantic bubble of molten lava filled with steam appears, and bursts; the imprisoned steam then escapes, carrying with it masses of the bubble high into the air. The author considers that all volcanic phenomena depend on these same conditions: (*a*) cracks or apertures forming communication between the surface and the interior of the earth; (*b*) highly heated matter beneath the surface; and (*c*) imprisoned water.

Animadverting on the common delusion that a volcano is a burning mountain, and that sulphur is the combustible, it is shown that sulphur is the result and not the cause of volcanic action. Common constituents of volcanic action are sulphurous acid and sulphuretted hydrogen, and when these come into contact, according to the author, "water and sulphuric acid are formed and a certain quantity of sulphur is set free." It should be understood however that if the gases are at all dry, as they sometimes are in the Solfataras of Krisuvik and elsewhere, water and sulphur are the sole products of the decomposition, while if moist, sulphur, water, and pentathionic acid are the first result.

If we examine the history of Vesuvius and other volcanic centres which have been known from a remote antiquity, we are led to the following conclusions, as regards the frequency of outbursts:—"(1) A long period of quiescence is generally followed by an eruption which is either of long duration, or of great violence. (2) A long-continued, or very violent eruption is usually followed by a prolonged period of repose. (3) Feeble and short eruptions usually succeed one another at brief intervals. (4) As a general rule, the violence of a great eruption is inversely proportional to its duration."

In the third chapter the author describes the products of volcanic action. In the account of Vulcano he has omitted to mention the very remarkable substance lately analysed by Prof. Cossa of Turin, which contains no fewer than seven non-metals and eight metals, combined in the following forms:—Arsenious sulphide, selenium sulphide, boric acid, ammonium chloride, lithium sulphate, thallium alum, cæsium alum, rubidium alum, and potassium alum. Bunsen's important division of all lavas into "acid lavas" and "basic lavas" is accepted, and the author admits an intermediate lava which contains from 55 to 66 per cent. of silica. He divides lavas further into five great groups: the Rhyolites, Trachytes, Andesites, Phonolites, and Basalts; the first being *acid*, the last *basic*, and the three others intermediate. An interesting account (illustrated by the frontispiece) is given of the microscopic examination of thin sections of rock, and the practicability of tracing by this means the passage from a glassy to a crystalline lava. It is shown that volcanic rocks having precisely the same chemical composition differ considerably in texture according as they are cooled slowly or rapidly. Thus gabbro, basalt, and trachyte are respectively the crystalline, lava, and glassy forms of the same substance. Some interesting details are given of the liquid cavities found in certain crystals, and of their contents.

In the fourth chapter the distribution of materials ejected from volcanic vents is discussed. In the account of "Pele's Hair"—the long threads of lava blown out by high-pressure steam in Hawaii—the reader may be misled. The author speaks of it as "filamentous volcanic glass," and in the passage preceding it (p. 71) he is evidently discussing "glassy lavas" and "pumice," which have been ranged among the acid lavas. But on p. 94 the same lava of Kilauea is spoken of as a "basic lava," although before described as a "molten glass," and presumably acid in character, that is, containing from 66 to 80 per cent. of silica. But basalt is a basic lava, and by rapid cooling it may become a perfect glass, hence we can understand how Pele's Hair may be described as "filamentous glass," without belonging to the class of acid lavas.

In the following chapter an extremely interesting account of the dissection of volcanoes by denudation is given, and the subject is illustrated by some striking examples, among which we may specially mention the plan of the volcano of Mull in the Inner Hebrides. In a past geological period this volcano was probably as large as Etna. The Island of Skye is the basal wreck of another volcano of Tertiary times. In the account of the formation of mineral veins the author has not alluded to Bunsen's surmise that the metallic copper found in the palagonite tuff of the Faroe Islands was reduced by volcanic hydrogen from the chloride.

The sixth chapter treats of the parasitic cones which appear upon the flanks of great volcanoes, and herein we notice one or two errors. Thus on p. 162 we read, "Among the hundreds of parasitic cones which stud the flanks of Etna, there are some which are nearly 800 feet in height." There are however less than a hundred cones worthy of the name, the rest are mere monticules, and of these we believe there are over six hundred. Among the larger cones Monte Minardo is the largest, and it is 750 feet in height, but has undoubtedly been much higher. In Fig. 63, p. 163, the outline of Etna, *as seen from the Val del Bove*, is wrongly described. The picture represents Etna as seen from Bronte, the opposite side of the mountain to the Val del Bove. It is taken from von Waltershausen's "Atlas des Ætna," and appears in Mr. Scrope's book on Volcanoes, in which it is also wrongly described. Occasionally we meet with hasty writing, particularly when the author is firmly convinced of his statement. Such small defects are easily remedied in the second edition. The following is an example of what we mean:

"That volcanoes are thus built up along lines of fissure in the earth's crust, we have the most convincing proofs. Not only have such fissures been seen in actual course of formation at Vesuvius, Etna, and other active volcanoes, but a study of the volcanoes dissected by denudation affords the most convincing evidence of the same fact. The remarkable linear arrangement of volcanoes, which is conspicuous to the most superficial observer, is a very striking evidence of the same fact." A slight looseness of expression is also apparent when the author speaks of carbonic acid as a *poisonous gas*, the fact being that the gas produces suffocation by spasmodically closing the glottis and without entering the lungs at all. On the other hand, carbonic oxide, which has a direct and baneful action on the organism, may truly be described as a poisonous gas.

In the account of Geysers the author, after stating that many attempts have been made to explain the mechanism by which the intermittent action of geysers is produced, remarks that probably no "such explanation will cover all the varied phenomena exhibited by them." Herein he does not even allude to Bunsen's classical experiments on the action of geysers, which are generally accepted as furnishing conclusive proofs of the mechanics of these intermittent springs.

A highly-instructive chapter discusses the number and distribution of volcanoes. In the second edition the map inserted at the beginning of Mr. Scrope's book might with advantage be introduced.

Concluding chapters discuss the information furnished by volcanoes concerning the interior of the earth, and the attempts made to explain the causes of volcanic action. In regard to this latter matter we are left as much in the dark as ever, and authors usually content themselves with stating the various hypotheses which have been proposed, leaving the reader to select that which he considers the most rational.

Prof. Judd's work is very instructive, and it will excite intense interest in the minds of many readers. Laid down upon the lines of Mr. Scrope's book, it is less methodical, less philosophical, and to most people more readable. A few things seem to us to be wanting, but probably the author has good reasons for their omission. Particularly we notice the absence of references to the labours of such men as Bunsen, von Waltershausen, Johnstrup, von Lasaulx, Steenstrup, Elie de Beaumont, and Tacchini. Iceland, the most marvellous country in the world from the volcanic point of view, is scarcely alluded to. And, as we said above, it seems to us that we require some *νοῦς* to arrange and put in order the countless *ὁμοιομέρειαι* that are scattered throughout the pages. But even without this we cannot read the book attentively without feeling that we have acquired a great mass of information concerning phenomena which have occupied the attention of wise men from the earliest times.

G. F. RODWELL

THE FIGURE OF THE EARTH

The Figure of the Earth: an Introduction to Geodesy. By Mansfield Merriman. (New York, 1881.)

THE author of this volume has already made his name known to us as the writer of an excellent treatise on the Method of Least Squares. The book before us presents to the reader, who is supposed to have some little knowledge of Algebra and Geometry, an explanatory and historical sketch of the labours of geodesists from the earliest days. We read in Chapter I. that Anaximander—a speculator in Geometry, Astronomy, and other sciences—concluded, from some reasons best known to himself, that the earth was a cylinder whose height is three times its diameter. There must have been some good reason for this idea, for we are told that Anaxagoras held the same. And it is scarcely to be wondered at that Plato originated some views of his own in the matter.

Passing to comparatively modern times we have a detailed account of the measurement of a degree (in 1766–68) by Mason and Dixon along the boundary line between the states of Maryland and Delaware. This measure gave 3947 miles as the radius of our supposed sphere.

Then the Franco-Peruvian expedition—*circa* 1736—of the Academicians MM. Bouguer and Lacondamine is briefly referred to (and here there is a misprint in the length of the base-line at Cotchesqui, which was 6274 toises in length), their labours giving 3936 miles as the radius. Henceforth the earth, abandoning its claims to sphericity, and not escaping a temporary imputation of being egg-shaped, settles down into an oblate spheroid—the figure generated by the revolution of an ellipse round its lesser axis.

Chapter II. treats of the method of determining the excentricity of this spheroid. As one measured arc will determine the radius of the spherical earth, so two measured arcs determine the radii of the spheroidal earth, that is, if the two arcs differ considerably in latitude. The actual excentricity is then calculated from the arc measured in Peru and that measured in Lapland. Then, further, taking the arc measured in France and combining these three in pairs, three quite different values of the excentricity are obtained. Here enters a discord not yet resolved; and in fact all modern measurements show that the earth is not a true spheroid, for, combining the arcs in pairs, all kinds of values of the excentricity present themselves. Then we fall back on the method of least squares, and grouping all the measurements into a unique calculation, we get a unique value of the excentricity, which may, with some show of reason, be called the most probable value. A specimen of this mode of calculation applied to pendulum observations is given at page 52; and it may be noted in passing that the calculation would have been made both neater and simpler by writing $S + 39$ instead of S. By inadvertence it is stated at page 54 that pendulum observations give 1–288·5 as the earth's ellipticity, and again at page 64, 1–289 is given as the result of the same observations. But these are the ellipticities that were obtained previous to the very extensive pendulum work recently completed by General Walker in India. When these modern observations are taken into account the 1–289 is changed to 1–292 or 1–293.

The earth then being no true spheroid, an attempt, described in Chapter III., is made to ascertain whether it is an ellipsoid with three unequal axes. Here but little better success is met with, and failing to establish for itself any fair name, the earth, like other pretenders, takes shelter under hard words, and in the concluding chapter of the book calls itself a geoid. Here we are safe and

beyond controversy, for your geoid makes no pretensions except to irregularity.

The surface of the geoid is in fact at every point perpendicular to the direction of gravity there. Thus the surface of the (unagitated) sea is a geoid, the surface of all lakes are portions of geoidal surfaces, nearly but not exactly parallel to that of the sea. That particular geoidal surface which represents the figure of the earth is the sea surface, which indeed is an old enough idea with a new name.

The work may be characterised as a fairly successful attempt to combine the advantages of a scientific and a popular treatment of its subject. It does not claim originality, and the mechanical theory of the earth's figure is not touched on.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Special Solar Heat-Radiations and their Earth-felt Effects

I REGRET if, by the words "lagging behind" in my paper to NATURE, vol. xxiv. p. 150, I have inadvertently misrepresented the views of Prof. Piazzi Smyth. Had his paper been only recently published, I might have been able to plead ignorance of its contents, but it is one which was published in 1869, and which I have read several times in the belief that it contains not only the first, but likewise the most complete contribution to this branch of knowledge.

Having made this confession, let me now in a very words endeavour to render clear that which I intended to say.

The hypothesis advocated in the lectures to which Prof. Smyth alludes was that which represents the sun as most powerful when it has most spots on its surface. Nevertheless if we take the observations of Prof. Smyth, of Mr. Stone, and of Dr. Köppen, and bring them together, we are led to think that perhaps on the whole we have highest temperatures about those times when there are fewest spots on the sun's surface. I then endeavoured to show that such an experience was nevertheless not inconsistent with the hypothesis of increased solar heat during times of most sun-spots.

Again, if we take rainfall, while we find that perhaps on the whole there is most rain during times of maximum sun-spots, yet there are certain stations which form an exception to this rule. Nor is this to be wondered at if we reflect that the direction, as well as the intensity, of the earth's convection currents must be affected by solar variability, and bear in mind that local causes have a very powerful influence upon rainfall. Now this last remark applies to temperature as well. I should therefore be prepared to hold, *simply as a working hypothesis*—

1. That, on the whole, the temperature on land may be less at times of maximum than at times of minimum sun-spots.

2. That, on the whole, the rainfall on land may be greater at times of maximum than at times of minimum sun-spots.

3. That while a period of temperature and one of rainfall coinciding with the sun-spot period will probably be found at most stations, nevertheless in individual localities the turning-points of these periods may vary considerably from the rule laid down in 1 and 2.

4. That the above order of phenomena is not inconsistent with the hypothesis that the sun is most powerful when there are most spots on its surface.

In conclusion permit me cordially to assent to the remarks of Prof. Smyth about the possibility of rapid outbreaks of solar heat being responded to by the earth; it may be only a few hours afterwards. His Madeira observations are of great interest to those who, like ourselves, believe that the bond between the sun and the earth is more intimate and sympathetic and less formal in its nature than that which the older generations of astronomers have been accustomed to imagine.

14, Queen's Road, Bayswater BALFOUR STEWART

How to Prevent Drowning

I OBSERVE several letters in NATURE, vol. xxiv. pp. 101 and 126, on floating as a means of preventing drowning, but I do not think the last word is yet said on this subject. I fully agree with Dr. Dudgeon that no rules for preventing drowning are of any practical value, and also with Mr. Hill and Dr. Dudgeon that those who can float are the rare exceptions. According to my observation not one in ten, in fact I might say hardly one in 100 even, of good swimmers can float in fresh water in any useful fashion, *i.e.*, lying motionless on the water and breathing easily. The obvious reason is that the human body in the natural condition, *i.e.*, with the lungs half inflated, is specifically heavier than water. Many persons say they can float, but in most cases they either inflate the lungs and hold the breath, or else they make slight movements of the hands.

But Dr. Dudgeon is wrong in supposing that the exceptions are all fat men. I am myself a case in point. I am slender almost to meagreness, and yet I float easily. From boyhood I have been fond of all athletic sports, and especially I am a practised and expert swimmer. I swim almost as easily as I walk. I float even in fresh water with the utmost ease, and for any length of time, breathing meanwhile naturally. While floating, the whole face, a large area on the chest, a small spot on the knees, and the tips of the toes are above the surface. Breathing causes the body to rise and fall gently, so that the exposed areas of the face and chest increase and diminish alternately.

It is evident therefore that the cases of persons who can really float are of two kinds, viz., (1) Those who are very fat, and (2) slender persons with very small bones and proportionately large lungs. This latter is my case. I never knew a heavy, muscular, large-boned man without excess of fat who could float. Such men make powerful swimmers, but are less easy and graceful in the water than those who are slenderer.

Berkeley, California, June 27 JOSEPH LE CONTE

Optical Phenomenon

I INCLOSE copies of photographs from two negatives (as you may see by looking at the points stereoscopically) of the Cyclopean gallery at Tiryns, for the sake of calling attention to the optical phenomenon shown in it. The gallery is very dark, the only light entering it by the narrow entrance and crevices between the rocks. At the extreme end of the gallery is an opening to the sky large enough to put one's hand through. In the photograph this is shown as a nucleus by a black speck surrounded by bright light, around which appears a dark circle, which again is encircled by a halo as perfectly rendered as one can see that around the moon at times. The dark nucleus is larger in the negative which had the longest exposure (the irregular lights around are only the light falling on the stones from side openings not visible). There was no such phenomenon recognisable to the naked eye.

The exposures were 25 and 15 min. with the full opening of the Ross "portable" lens and a gelatine plate.

Athens, July 3 W. J. STILLMAN

[The photographs quite bear out Mr. Stillman's statements.—ED.]

Implements at Acton

IN reply to the letter of Mr. Worthington G. Smith (NATURE, vol. xxiv. p. 141), the Palæolithic implements at Acton I found in a gravel pit on the hill west of the North London Railway, and from spread gravel which was local. Those I obtained at Hammersmith occurred in gravel raised on a piece of ground (Mr. Butt's) south of Great Church Lane, where building is going on. They consisted of a hollow scraper, drill, &c., which I readily found; but the implements here seem ruder than at Acton, and less easy of detection. I also found implements in gravel raised from the foundations of the neighbouring houses, which had been spread on some newly-made roads south of Shepherd's Bush, between the Uxbridge Road and Addison Road Stations. I see no reason for supposing that the implements here or at Hammersmith occur under different conditions from those at Acton.

I examined carefully, some weeks ago, some extensive heaps of sand and gravel raised on the premises of the Water-works Company on the right across the Hammersmith Suspension-bridge, but found no worked flints. The gravel here may be of more recent deposition.

With regard to the Neolithic implements of Acton, I am interested to hear that Mr. Worthington Smith is familiar with them, and that there are specimens in the Pitt-Rivers Collection. My letter nevertheless will have done good in making their occurrence more generally known.

As regards the quartzite pebble, if more are found on the fields about Acton it will tend to show that they have served the same purpose as those in South-East Devon, and that they have been brought from a region where specimens adapted for missiles would be found in abundance, viz. the south-west coast, as gravel sections and gravel pits were not accessible in Neolithic times, nor would they have proved adequate arsenals. But if the pebble I found at Acton were accidentally derived from the Middlesex gravel (which contains a considerable quantity of Midland Bunter material),* it is remarkable that a selection should have been made so well calculated to deceive a Devonshire neolithologist.

July 19 SPENCER GEO. PERCEVAL

Lightning

ABOUT 10 a.m. of the 6th instant two of the labourers on this farm were sitting on the ground (with their backs against a clover haystack and their faces towards the north, having in front of them and on their left a wood) engaged in eating their lunch. It had been raining and thundering for about half an hour, but not heavily, until suddenly—in the words of one of the men—"a flash of lightning came right at us as if it were shot out of a gun." This man had his knife up to his mouth at the time in the act of eating, and he describes his sensation as a feeling of nausea in his throat and chest, and also that both he and his companion felt an actual push against their shoulders, which swayed and shook them to a considerable extent from the direction of the flash. The other man was blinded for about five minutes, and they were both much dazed for some time. Also they both describe having heard a sharp whiz somewhat resembling the quick escape of steam from an escape valve on an engine. For two days after they both suffered from severe headache. A. HALL, JUN.

Filstone Hall, Shoreham, Kent, July 11

THE COMET

IN a paper read to the Paris Academy on the 11th inst., giving further observations on comet *b* 1881, M. Wolf says:—

"The analysis of the light of the comet furnishes data as to the constitution of that body, which it is important to consider before starting any hypothesis as to its nature and mode of evolution.

"I have examined the spectrum of the comet, both with a highly dispersive spectroscope mounted on the Foucault telescope of 0·40 m. aperture, and with a smaller instrument, mounted on the telescope of 1·20m., giving therefore a very large quantity of light. This spectrum is triple: one sees (1) a continuous spectrum, broad, but very pale, visible in all the regions of the comet; (2) a continuous spectrum nearly linear, and very bright, given by the nucleus; (3) the spectrum of three bands, yellow, green, and blue, characteristic of the light of all comets examined hitherto. I have never been able to see the violet band.

"The continuous spectrum of the nucleus indicates the existence of solid or liquid matter, luminous of itself or by reflection. I have suspected in the strip some dark interruptions, especially in the region near D, without being able to determine their position. The presence of these dark lines, demonstrated by Dr. Huggins' photographs, denotes a reflected light, which can be no other than that of the sun.

"The nebulosity which forms the head of the comet gives, besides the continuous pale spectrum, the bright bands of an incandescent compound gas. The researches of M. Hasselberg tend to assimilate these bands to those of a carburet of hydrogen, probably acetylene. Besides these bands one sees throughout the strip formed by the light of the nucleus other protuberances very short, and paler, which seem to indicate, in the hotter and more luminous parts of the comet, an incandescent atmosphere of more complex constitution.

"When the slit of the spectroscope is passed over the comet, starting from the head, one finds the three bands all round the nucleus, at nearly the same distance from all the sides. They disappear in the tail properly so-called, the very pale spectrum of which seems to be continuous. Thus only the nebulosity surrounding the nucleus contains incandescent gases. The light of the tail comes to us from a pulverulent matter, luminous, or simply illuminated. Such are the data of spectroscopy.

"The polariscopic examination of the comet's light completes these first results. I used, as polariscope, a quartz plate perpendicular to the axis, giving the sensible tint, and a double-refracting prism, placed between a collimator and an observing telescope, in place of the prism of a direct-vision spectroscope. The two images of the nucleus and the nebulosity surrounding it are projected, well separate, on the common part of the field formed by the background of the sky; this is the process indicated long ago by M. Prazmowski for eliminating atmospheric polarisation. Under these conditions the nucleus and the nebulosity appear both distinctly polarised in the median plane of the tail, consequently in the plane passing through the sun. Here then, at least in all parts of the nebulosity round the nucleus, we have reflected light coming from the sun, and a non-gaseous matter possessed of reflecting power. I have had this important result verified by my assistant, M. Guénaire, and by several students in the Observatory.

"This process, so sensitive, evidently cannot serve for the tail, which occupies the whole field of vision, and does not moreover present very distinct limits. I have vainly tried other polariscopes—Savart's, for example. It would be very difficult besides to separate here the real polarisation of the tail from that of the atmosphere.

"In proportion as the light of the comet is diminished, the spectrum of the nucleus becomes paler; its colours, well pronounced on the earlier days, are no longer seen except on the side of the red; the bright bands retain their brightness. The green band is always distinctly limited in the less refrangible part. It will be interesting to know whether the comet, reduced to telescopic brightness, will at the same time have its light reduced to that of an atmosphere purely gaseous.

"On June 29, at 5h. 49m. sidereal time, during my polariscopic observations, a small star was found in the nebulosity, at a very short distance from the nucleus. Its image had not undergone any change, either of brightness or of form."

At the same *séance* M. Thollon communicated a note of spectroscopic observations of the comet as follows:—

"These observations were made with a direct-vision spectroscope which MM. Henry of the Observatory were good enough to lend me. The dispersion is that of an ordinary prism. A micrometer eye-piece, with point, giving 1-200th of a millimetre, enables one to make measurements of very high precision.

"In the night of June 24 I made my first observations and measurements. The nucleus presented then a very brilliant continuous spectrum, on which no trace of bands could be distinguished. On the violet side it extended beyond the line G. The parts next the nucleus likewise gave a continuous spectrum, on which the bands were still invisible; they only appeared a little further on and faintly. In the continuous spectrum I have thought I perceived several times a very complicated system of dark lines, and occasionally I believed I saw in the spectrum

bright parts having the aspect of short lines, not occupying the whole width of the spectrum. This was perhaps merely a result of fatigue of the eyes; these phenomena were only produced during the first two nights.

"It appeared to me important to follow the modifications the spectrum might undergo as the comet went away from the sun. These modifications were produced with perfect distinctness. In the spectrum of the nucleus the violet radiations were extinguished first. About June 30 the most refrangible part, commencing with the green band ($\lambda = 516$), had sensibly lost its brightness and became invisible in the region G, while the yellow and red appeared to me as bright as on the first day. The bands, masked at first by the brightness of the continuous spectrum, became each day more visible in the neighbourhood of the nucleus, and during the night of July 1 they were perfectly distinguished on the nucleus itself.

"The measurements successively made of the bands of the comet and of those of the alcohol flame led me to conclude the identity of the two spectra. The green band however, the most brilliant, seemed a little more refracted in the comet than in the flame. To submit this matter to a decisive test, a total reflection-prism was adjusted on the slit so as to cover half of it. On placing the two spectra together I observed that they were strikingly similar when they had the same brightness, but that the green band appeared indeed more refracted in the comet when the spectrum of the flame was more brilliant. The comparison made directly between the two spectra, and the perfect coincidence of the bands, dispense with the necessity of giving numbers furnished by my micrometric measurements. They would not add anything to the certainty of the result.

"As to the violet band, it has not been possible for me to see it in a certain manner, even using a very small dispersion and a very small ocular enlargement. There is not in this fact anything surprising, if we take account of atmospheric absorption and of variations of brightness undergone by the violet band, when the experimental conditions are varied. We know that in the ordinary flame of alcohol it is very brilliant; but if this flame be cooled by means of several folds of metallic sheeting, it becomes very weak and tends to disappear, while the other bands sensibly retain their habitual aspect.

"Continuing my observations till the present, I have found the continuous spectrum of the nucleus diminish progressively in brightness and extent, especially on the violet side. At present it has the aspect of a thin luminous thread, hardly passing beyond the line F. The bands, on the other hand, seem to have retained their intensity in the head of the comet. In the tail, and to a distance from the nucleus equal to twice or thrice the diameter of the head, they are still seen, but very faintly. Further on one sees only a continuous spectrum due perhaps to the light of the moon diffused by the haze, pretty thick during the last nights of observation.

"It seems to result from this that the cometary mass is formed in part of an incandescent gas, characterised by the spectrum of bands, and in part of solid or liquid matter, likewise incandescent, but in a state of extreme division, emitting a white light which belongs to it, and capable of reflecting in a certain proportion the light it receives from the sun. All the spectroscopic observations hitherto made on comets indicate the existence of carbon in the gases producing the band-spectrum. Dr. Huggins has given this conclusion a striking demonstration by showing, with photography, the existence of two bands of carbon in the ultra-violet spectrum of the comet.

"I have the honour to submit to the Academy three drawings representing (1) the spectrum of the alcohol flame, (2) the spectrum of the comet during the night of June 24, and (3) the same spectrum on July 1."

WIDTH OF MR. RUTHERFURD'S RULINGS

BY the direction of C. P. Patterson, the Superintendent of the U.S. Coast and Geodetic Survey, I have long been engaged in the precise measurement of a wave-length of light, in order to obtain a check upon the secular molecular changes of metallic bars used as standards of length. In advance of the publication of this work it may be useful to say I have found that the closest-ruled diffraction-plates by Mr. Lewis Rutherfurd have a mean width of ruling which varies in different specimens from 68078 to 68082 lines to the decimetre, at 70° F. There is a solar spectral line, well suited for precise observation, whose minimum deviation with one of Mr. Rutherfurd's plates in the spectrum of the second order with the closest ruled plates is 45° 01′ 56″ at 70° F. I would propose that this line be adopted as a standard of reference by such observers of wave-lengths as desire to escape the arduous operation of measuring the mean width of their rulings; for by means of the measures which are shortly to be published it will be possible to deduce from the minimum deviation of this line produced by any given gitter, the mean width of that gitter, and consequently the wave-length of any other line whose deviation has been observed with it. The accuracy of this method will greatly exceed that of assuming Ångström's measures to be correct. The wave-length of the line in question (still subject to some corrections which may be considerable) is 5624825. Ångström gives 562336.

C. S. PEIRCE

CITY AND GUILDS OF LONDON INSTITUTE

IT would seem as if at last, after long years of waiting, there were some hope that the views which for the last quarter of a century have been so persistently advocated touching technical education, were about to bring forth more fruit in London.

In season and out of season, since the note was first sounded by the late Prince Consort, one far-seeing advocate after another, and among these we must specially name Mr. Samuelson, Mr. Mundella, and Sir Henry Cole, have cried in the wilderness touching the need of more scientific instruction. At last it does seem as if there is an awakening, as if a part of the idea was realised in the Institute, the foundation-stone of which was laid at South Kensington on Monday by the Prince of Wales. No doubt in the building which has been begun a national school of science, theoretical and applied, worthy of a country like ours, may grow up. Mr. Mundella will rejoice that at last he has an opportunity of carrying out with something like adequacy the views on education of which he has been so long a strenuous advocate. We hope next week to give a detailed description and illustration of the new building; and meanwhile will content ourselves with briefly referring to what took place on Monday.

The company present to receive the Prince of Wales was large and distinguished, including many eminent men of science. The Lord Chancellor, as Chairman of the Institute, addressed the Prince, expressing the gratification of the Council that His Royal Highness had consented to become president. The Lord Chancellor then traced the growth of the Institute and the efforts of the City Guilds to improve the technical education of the country.

"Since July of last year," the Lord Chancellor said, "the date of the incorporation of the Institute, its work has satisfactorily increased, and the Council have a lively and grateful recollection of the assistance and encouragement afforded to them by His Royal Highness, Prince Leopold, Duke of Albany, who in May last laid the foundation-stone of the Finsbury Technical College, a college that has been established by this Institute, and

which, when erected, will be the first building in the metropolis exclusively devoted to technical teaching. Pending the completion of the Finsbury College, instruction is being given to a large and increasing number of artisan students in some of the applications of chemical science to manufactures and industrial operations, and also in that new and widely opening field of labour and invention—the application of electrical science to the transmission and conservation of energy. Instruction will also be provided in that college, when finished, for those who are engaged in various handicraft trades, and it is hoped that this kind of teaching, which is gradually taking the place of apprenticeship in France, Germany, and Sweden, will help in this country to supplement, without supplanting, workshop training. The Institute is also endeavouring to advance technical education in a large number of towns in the United Kingdom by holding annual examinations in technology, and by encouraging, in connection with these examinations, the formation of evening classes for artisans, by assisting in the payment of teachers of technical subjects. During the early part of the present year more than eighty such classes were in operation, and it is satisfactory to know that the number of candidates recently examined by the Institute in different branches of technology was 1563 as compared with 816 in the previous year. But it is to the Central Institution," the Lord Chancellor went on to say, "the first supporting pillar of which your Royal Highness has graciously consented to set this day, that the Council look to crown their endeavours and give unanimity to all their efforts. In this college, from which the entire work of the Institute will be directed, instruction of a higher and more advanced character will be given, adapted to the wants of those who will be engaged in professional or commercial pursuits, in which a knowledge of some branch of mechanics, physics, or chemistry in its practical application will be found not only serviceable but almost indispensable. The building when completed will be supplied with laboratories, in which the most delicate operations can be carried on, with workshops in which the various branches of mechanical and electrical engineering will be taught, with studios in which applied art may be practised, and with a lecture-hall, theatres, and class-rooms in which the principles of science will be explained. Here, it is anticipated, will receive their professional training the sons of manufacturers, many of whom have hitherto been compelled to pursue their studies abroad—in Germany, in Switzerland, in France, or in America, in all which countries, for some time past, technical colleges, such as this Central Institution is intended to be, have already flourished. Here it is expected that artisans who have shown merit and have won distinction at the branch or provincial colleges will complete the training which may qualify them to act as managers and superintendents of works. Some of these, it is hoped, will obtain their education in this college by means of scholarships to be established by the Institute itself, possibly by provincial colleges, trade societies, or other public bodies, or by private individuals who may be interested in the promotion of technical education. And here it is anticipated will be trained that body of technical teachers, of whom there is in England at the present moment so great a need, who will carry with them from this college into the manufacturing centres, to be there imparted to other students, a knowledge of the theory and the practice of various crafts and industries. This institution will not be established as a rival to any other existing seat of learning; least of all to the excellent schools situated in this neighbourhood, which for some years past have been the means of offering to hundreds of young men and women a knowledge of the principles of science and art. The aim of this institution will be to supplement the teaching of those schools by giving instruction in the practical application of science and art to the trades and industries of the country, and by cultivating and endeavouring to stimulate inventive genius. It is therefore hoped and anticipated that the sister institutions, representing pure and applied science, will work in harmony with each other, forming an alliance, the effect of which will be to raise the intellectual *status* and to improve the technical knowledge and practical skill of the working classes of this country, and so to increase its industrial prosperity. It gives me great pleasure to be enabled to add that it has seemed fit to Her Majesty to recognise on this occasion the eminent services of Mr. Bramwell, the indefatigable chairman of the executive committee of the institution, by signifying Her Majesty's gracious intention of conferring upon that gentleman the honour of knighthood. It is anticipated that the cost of this building, when fully equipped with the apparatus and appliances needful for technical instruction, will not fall far short of 75,000*l*. Of this sum 31,000*l*. has been alread subscribed by the worshipful companies of Fishmongers, Goldsmiths, Clothworkers, and Cordwainers; the grant of the Drapers' Company having been appropriated to the Finsbury College; and it is expected that about 24,000*l*. will be saved from the annual income of the Institute during the building of this college. The Council therefore, after paying the amount which is due, will have at their disposal only an estimated sum of about 55,000*l*., and they look to the liberality of the Livery Companies, both of those who have and of those who have not as yet subscribed to the funds of the Institute, to make good the balance of 20,000*l*., so that the building of this college may be completed at once and as a whole, in strict accordance with the plans."

The Prince of Wales in reply made some forcible and sensible remarks on the necessity to this country of improved technical education—education in things as contrasted with words—if we are to keep our place among the other industrial nations. "Other nations," the Prince said, "which did not possess in such abundance as Great Britain coal, the source of power, and iron, the essence of strength, compensated for the want of raw material by the technical education of their industrial classes, and this country has therefore seen manufactures springing up everywhere guided by the trained intelligence thus created. Both in Europe and in America technical colleges for teaching, not the practice, but the principles of science and art involved in particular industries, had been organised in all the leading centres of industry. England is now thoroughly aware of the necessity for supplementing her educational institutions by colleges of a like nature." The new building, the Prince remarked, will be of considerable benefit to the whole kingdom, not only as an example of the Institute devoting itself to technical training, but as a focus likewise for uniting the different technical schools in the metropolis already in existence, and as a central establishment also to which promising students from the provinces may, by the aid of scholarships, be brought to benefit by the superior instruction which London can command. The Prince reminded his audience that the realisation of the idea of such a college was one of the most cherished objects which his father had in view. "It is to me," the Prince stated, "a peculiar pleasure that the Commissioners of the Exhibition, of which I am the president, have been able to contribute to your present important undertaking, by giving to you the ground upon which the present college is to be erected with a sufficient reserve of land to insure its future development. By consenting at your request to become the president of this institute I hope it may be in my power to benefit the good work, and that our joint exertions, aided, I trust, by the continued liberality of the City and Guilds of London, may prove to be an example to the rest of the country to train the intelligence of industrial communities, so that, with the increasing competition of the world, England may

retain her proud pre-eminence as a manufacturing nation." Among the articles deposited in the stone were copies of the *Times*, *Nature*, and the *City Press*.

NOTES

THE Graham medal, instituted in connection with the Philosophical Society of Glasgow (Chemical Section), for the encouragement of chemical research, and open to competition to all chemists, has, on the recommendation of Prof. Williamson, F.R.S., the adjudicator in the competition, been awarded to Mr. James Mactear, F.C.S., F.I.C., for a paper entitled "Some Researches on the Reactions involved in the Leblanc Process of Alkali Manufacture."

THE fifty-fourth meeting of the German Association of Naturalists and Physicians will be held at Salzburg on September 18-24 next. From the list of addresses we note the following:—Dr. von Pettenkofer (Munich), on the soil and its connection with the health of man; Herr Meynert (Vienna), on the laws which govern human thoughts and actions; Dr. von Oppolzer (Vienna), on the question: Is Newton's law of gravitation sufficient for the explanation of the motion of heavenly bodies, and are there reasons to designate it only as approximately true? Herr Mach (Prague), on natural history teaching. All these addresses (besides one by Herr Weismann (Freiburg-im-Bresgau), the subject of which is not yet fixed) will be delivered at the general meetings. For the entertainment of visitors sufficient preparations will be made; the programme enumerates social gatherings, concerts, and excursions into the charming neighbourhood of Salzburg.

THE German Society for Anthropology, Ethnology, and Prehistoric Research will meet this year at Ratisbon on August 8-10 next. The programme of the meeting is a very varied one. In the first place the members will visit the curiosities and collections of the ancient city itself and the numerous Roman antiquities in the neighbourhood. At the Roman necropolis near Kumpfmühl some excavations will be made. Addresses will be delivered on the Roman period in Germany, on the period of serial tombs, on the pre-Roman metal age, on the stone period, and on anthropological questions generally.

ON Saturday the Prince of Wales opened, at South Kensington, the International Medical and Sanitary Exhibition which is being held in connection with the forthcoming Medical Congress. Up to the present nearly 2000 members of the medical profession have signified their intention of attending the Congress.

AT the Annual General Meeting of the Society of Arts medals were awarded as follows for papers read at the meetings of the Society:—Prof. A. Graham Bell, E. P. Edwards (of the Trinity House), Mr. Alex. Siemens, Sir Bartle Frere, Mr. J. Y. Buchanan, Prof. Perry, Sir Richard Temple, and Mr. J. M. Maclean.

AMONG recent valuable additions of models of ships to the collection now being exhibited in the galleries south of the Royal Horticultural Gardens is a whole model of the *Livadia*, showing in miniature all the details of that noted yacht. It is lent by the builders, Messrs. John Elder and Co. The London and Glasgow Shipbuilding and Engineering Company have lent half-block models of three of their steamships, and by an ingenious use of mirrors in mounting these the whole of each vessel is represented, and fore and aft views can be conveniently studied. There are many other admirable models.

THE geological distribution of endemic goître in England has been made the subject of a recent paper by Prof. Lebour of Newcastle. He shows that there is on the whole a striking sameness in the distribution in this country and in France, where Dr. de St. Lager of Lyons has fully investigated the facts. One important point only he considers to be established as common to those rocks on which goître does not occur—the absence of limestone together with that of metallic impurities. In both countries the rocks which support most goître are such as are both calcareous and metalliferous. But there are plenty of facts to show that metalliferous impurities alone cannot be credited with the origin of the disease, else the Devonian and the granite would surely not be free from it. Neither will the absence of limestone alone be sufficient to check the growth of the disease, else the lignitiferous beds of France and the ferruginous sands of the Weald would not support it. (Dr. de St. Lager's conclusion is that endemic goître coincides with metalliferous deposits, iron pyrites being in the first rank.)

THE Handbook of the Vertebrate Fauna of the County of York, by W. E. Clarke and W. D. Roebuck, the secretaries of the Yorkshire Naturalists' Union, is expected to appear about the beginning of August. The work will show what species are, or have been, within historical periods, found in Yorkshire. The authors are enabled to enumerate, as such, 508 species out of a total British list of 756, a fauna superior in numerical extent to that of any other county in the British Isles. The list includes 46 mammals, more than 300 birds (doubtful species being excluded), 12 reptiles and amphibians, and upwards of 150 fishes. For comparison, the British species *not* found in Yorkshire are also enumerated. Application should be made to the above-named gentlemen, 9, Commercial Buildings, Park Row, Leeds.

THE Marine excursion of the Birmingham Natural History and Microscopical Society to Oban this year, which extended from July 1 to July 12, proved a great success, and fully answered the expectations of its promoters. Thirty-two Members joined the excursion, including Dr. Thomas Wright, F.R.S., the President of the Midland Union of Natural History Societies, and Mr. E. D. Hamel, Ex-President of the Tamworth Natural History Society. There were also several ladies. A little steamer—the *Curlew*—of about twenty-five tons burthen, was chartered for a week. Dredging operations were carried on daily in the Bay of Oban and the neighbourhood in depths varying from fifteen to fifty fathoms, under the superintendence of Mr. Edmund Tonks, B.C.S., and Mr. W. R. Hughes, F.L.S. A most interesting and beautiful collection of animals was taken. The specimens included fine examples of the Alcyonarian zoophytes. The Echinoderms embraced many genera from *Antedon* (*Comatula*) through the group to *Holothuria*. The Mollusca were not very numerous, but they included several rare forms. A few interesting fishes were taken, including the Lump-Sucker. The specimens will be examined by specialists and reported to the Society in due course. Those Members who did not engage in the dredgings had good opportunities of botanising and geologising, the indefatigable honorary secretary, Mr. Morley, having arranged a series of excursions to the principal places of interest in the district. On Sunday evenings July 3 and July 10, Dr. Wright also gave by request addresses "On the Basaltic Formations of Staffa and Iona," and "On Glaciation," which afforded great gratification to the Members. In the evenings demonstrations were given by the microscope and otherwise on the more interesting forms of life taken, by Prof. Bridge, Mr. W. P. Marshall, Mr. W. R. Hughes, and Mr. G. W. Tait. By the courtesy of Mr. R. H. Scott, of the Meteorological Office, telegrams were received daily, giving the weather forecasts for the morrow, which enabled the members to make their arrangements. At the termination of the excursion votes of thanks were accorded to the leaders of the party, who rendered assistance in various ways, and a resolution was passed selecting the Channel Islands as the place for the next marine excursion.

THE German Government has been requested by many eminent hydrologists to establish a hydrological "Reichs-Centralstelle." They consider hydrological researches extending over the whole Empire necessary for the general welfare with regard to the utilisation of water and for the general protection of arable lands against floods and inundations. As these researches would necessarily often be combined with meteorological observations, it is proposed to connect the Hydrological Office with the Meteorological Central Office. The work would have to be done principally by hydrologists and meteorologists, but the staff would have to comprise geologists, agriculturists, and forest-rangers.

THE news that in the Pastorat Moor of Dejbjerg (district of Rinkjöbing, Jütland) a carriage of the fourth or fifth century has been discovered, causes great sensation in archæological circles. At the beginning of this year the Museum of Northern Antiquities of Copenhagen received several bronzes which had been found in the moor in question, which unquestionably had originally been carriage ornaments. Perfectly similar bronzes had been found a few years ago at Broholm (Fünen) in a tomb, and had been explained as ornaments of a wooden carriage which had been burned with the dead. The discovery in the Dejbjerg Moor now confirms this view. Dr. H. Petersen, who also conducts the excavations at Broholm, was intrusted with the investigation of the Dejbjerg Moor, and his researches show that the fragments now found belonged to a state carriage with neatly turned spars and fine bronze ornaments on the wheels and sides. Apart from the carriage fragments only a few clay vessels were found. They all date from the migration period.

THE Archæological Society at Athens has purchased the land at Eleusis necessary in order to excavate the temple of Ceres. News from the director of the excavations at Epidauros state that the theatre excavated in the forest of Asklepios is the second largest of Ancient Greece and a masterpiece of the architect Polykleitos. Even the headless statue found there, which is supposed to represent Hygieia, is believed to be a work of Polykleitos.

THE Museum of Antiquities at Sparta is reported to have been broken open and robbed of many objects.

AS we anticipated in our last issue, M. Berthelot has been nominated a life-member of the French Senate almost unanimously. It may be noted that it is just twenty years since M. Berthelot received the great prize of the Academy of Sciences for his method of producing artificially substances which have been found only in living bodies.

A SAD accident has happened in the vicinity of Lyons, where two balloons were sent up on the occasion of the *fête* of July 14. A match having been ignited close to the place where the largest landed, the balloon exploded instantaneously with a fearful crash. Three people were severely wounded.

M. DE MÉRITENS, the well-known electrician, tried a new system of electrical illumination on the occasion of the festivities of July 14. He suspended his regulators between two poles placed on each side of the Boulevard des Italiens and fifty feet high. A series of four of these regulators were placed at a distance of about 200 feet from each other. The effect was much approved by a large number of people.

A SPECIAL competition has been opened for erecting a statue to Carnot, the celebrated mathematician and politician of the First Republic. The number of competitors exceeds fifty, and some of the works sent are highly creditable to their authors. The statue is to be erected by public subscription at the birthplace of Carnot, Nolay, in Côte d'Or.

OUR Paris correspondent informs us that Philippart and Sons are preparing to work tramways at Roubaix with improved Faure batteries, and that experiments will also shortly be made in London. Our correspondent witnessed some preliminary experiments which he thinks give room for high expectations. One of the most important changes is the substitution of flat for round sheets, which produced numerous cracks in the minium coating, and had been resorted to in imitation of the old Planté batteries.

AT the anniversary meeting of the Sanitary Institute of Great Britain held at the Royal Institution, Albemarle Street, on Thursday, July 14, the Right Hon. Earl Fortescue in the chair, an address was delivered by Prof. F. S. B. F. de Chaumont, M.D., F.R.S., chairman of the Council, entitled "Modern Sanitary Science," and the medals and certificates were awarded to the successful exhibitors at the exhibition held at Exeter in October, 1880.

THE forty seventh anniversary meeting of the Statistical Society was held in the Society's rooms, King's College, Strand, on the 28th ult., Dr. W. A. Guy, a past president, in the chair. The report was highly satisfactory, showing that in the last decade the number of Fellows, the income, and the amount invested have been more than doubled, while the expenditure had increased in a less ratio A new edition of the Library catalogue is being prepared. Ten papers had been read during the year. The president for 1881–82 is James Caird, C.B., F.R.S.

A SECOND earthquake is reported from Metkovich (Dalmatia). It was observed on June 14 at 5.27 a.m. During the night of May 17 a violent shock occurred in Haiti, causing several landslips, through which a large number of cattle perished. The volcano in the Gulf of Santorin, which has been inactive since 1870, again began to eject vapour on May 30 last. This activity increased considerably on June 2. The sea between Pala and Aeo Kaymene has again become heated. Earthquakes are reported from the east coast of Tunis. It is stated that since June 10 last Gabes and neighbourhood was visited by a great many violent shocks, some recurring at very short intervals. The last shock was felt during the night of June 22-23. The mountains in the neighbourhood of Gabes are of volcanic nature; smoke rises during the night from the Ay-Buin Mountain (about 30 kilometres to the north-west of Gabes), and at Hamma, 18 kilometres from Gabes, there are hot springs. Shocks of earthquake are reported from different places in Dalmatia: Ragusa on July 4, at 10.28 a.m.; Budua, Castelastua, Sutomore, on July 4, at 10.19 a.m. and 1.53 p.m.; duration, two to four seconds; direction, north to south.

THE growth of American journalism is shown by recent census results to have been much more rapid than that of English. In 1824 there were eleven daily newspapers in Philadelphia and twelve in New York, with a circulation varying from 1000 to 4000 copies. To-day the State of New York has 115 daily newspapers and 84 weeklies, with a combined annual circulation of 384,328,454; and Pennsylvania 98 daily newspapers and 57 weekly papers, with a combined circulation of 202,539,482. There are 962 daily newspapers in the United States, and 803 weekly, semi weekly, tri-weekly, and Sunday newspapers. The total circulation of all newspapers is estimated to be 1,344,101,235, the bulk of which is in ten great States.

FROM a recent U.S. Census Bulletin relating to the Fishery Industries of the Pacific States and Territories (California, Oregon, Washington, and Alaska) we gather that the total number of persons engaged in these fisheries is 16,745, of whom 7910 are Esquimaux, Aleuts, and Indians, and about 4000 Chinese. A capital of over 2½ million dollars is invested in vessels, boats, apparatus, building, &c. There are 53 vessels and 5547 boats. Among other items in this Bulletin we note that the number of salmon caught in 1880 (to which all these

numbers refer) was 2,755,000, with a total weight of 51,862,000 lbs. The number of sealskins obtained was 155,718, valued at 1,540,912 dollars.

M. FERRY has ordered the teachers of elementary classes of the colleges to conduct their pupils into the galleries of the Museum of Natural History at Paris, to explain to them the differences of the several kinds of animals, plants, and minerals, and to incite young pupils to collect specimens during their walks in the country round Paris.

AN attempt at silk cultivation is to be made at Akaron, New Zealand, the valleys and bays of Banks' Peninsula being considered well suited for that purpose. The Colonial Government are sending to California and Japan for silkworms' eggs and mulberry trees of the best kinds, with the view of encouraging the industry.

THE *Colonies and India* reprints from a New Zealand paper some notes on a discussion at the Otago Institute, when Prof. Parker exhibited the skin and body of the extremely rare and remarkable bird, *Notornis Mantelli*. The specimen in question is only the third which has ever been captured, was caught low down on the ranges, and it is probable that an expedition will be fitted out to search for more of the species.

A POPULAR explanation of Kant's "Kritik der reinen Vernunft," by Albrecht Krause, has just been published by Moritz Schauenburg of Lahr (Germany), "in celebration of the centenary of the publication of the great work."

AN important invention relating to railway signals has recently been made in Germany, and the model apparatus has just been completed at the central works of the Bergisch-Märkische Railway Company at Witten. The model will be exhibited at the Electro-Technical Exhibition at Paris.

THE additions to the Zoological Society's Gardens during the past week include a Red-handed Tamarin (*Midas rufimanus*) from Surinam, presented by Mr. Keiser; an American Black Bear (*Ursus americanus*) from Nova Scotia, presented by the Earl of Caledon, F.Z.S., and the Hon. Charles Alexander; two Grey Ichneumons (*Herpestes griseus*) from India, presented respectively by Mr. C. R. Smith and Mrs. C. Hassell; a Common Raven (*Corvus corax*), British, presented by Major Botts; a Carrion Crow (*Corvus corone*), European, presented by Miss Mortimer; a —— Monitor (*Monitor*, sp. inc.) from Ceylon, presented by Mr. E. Lindstedt; a Sykes' Monkey (*Cercopithecus albogularis*), three Vulturine Guinea Fowl (*Numida vulturina*) from East Africa, deposited; three Common Peafowl (*Pavo cristatus*), two Cheer Pheasants (*Phasianus wallichii*), two Horned Tragopans (*Ceriornis satyra*), a Siamese Pheasant (*Euplocamus pralatus*), bred in the Gardens.

METEOROLOGICAL NOTES

FROM a discussion by Dr. Hann of a series of hourly summer observations of air-pressure, temperature, moisture, cloudiness, and force of wind made by the U.S. Engineer Corps on the plateaux of the Rocky Mountains (the stations lying between 3500 and 8500 feet above the sea), it appears that in valleys and wide basins, even at the greatest height, the influence of the daily barometer oscillation in summer is still very great, and no decrease with the height is noticed. The course of the curve is of the continental type, a comparatively large afternoon minimum, a slightly marked morning minimum, and an earlier occurrence (7 to 8h.) of the morning maximum. In the temperature-curve the most notable point is that the maximum is very near midday, or little behind the culmination of the sun. The maximum of absolute moisture occurs about 8 a.m., and a second smaller maximum in the afternoon or evening. The maximum of cloudiness and wind-force occurs between 3 and 4 p.m., the minimum between 3 and 4 a.m.

IN a letter dated April 14, Mr. Russell of the Sydney Observatory remarks that the rain return for 1880 shows it to have been a dry year in New South Wales, as in many other parts of the world; but the want of rain was not severely felt because it came at favourable times for grass. Perhaps the most curious consequence of the short supply of rain was the stoppage of the river navigation for a considerable part of the year, thus preventing the wool from going by steamer to market, and increasing the cost of all stores consumed: the river curves show, for instance, that at Bourke the water was at summer level from June to October, thus preventing navigation. Mr. Russell hopes, by the combination of the rain and river observations, to find an answer to a local question of very great importance, viz. the *amount* and *source* of the water found in wells which are being sunk by the hundred in the inland parts of the colony. There can be no doubt that all, or nearly all, the water brought down in such abundance from Tropical Queensland by the Culgoa, Warrego, and Paroo Rivers sinks into the ground before it reaches New South Wales, and there is good reason for thinking that much of the water brought down by the heads of the Darling sinks into the ground before it reaches Bourke. If this can be proved, which he thinks can be done in the course of a few years, there will be no fear for the abundance and permanence of the well-water. And when it is remembered that in most cases the water rises to within thirty or forty feet of the surface, in many instances to the surface, and in one case twenty-six feet above the surface, the local importance of the question will be obvious.

IN studying the conditions of temperature of the Russian Empire some time ago, M. Wild found that the irregular distribution of temperature revealed by the isotherms might be elucidated by means of "isanomals" (or lines of equal temperature-anomalies). Among the causes of the isanomals special regard must be had to the wind, which again immediately depends on the distribution of air-pressure, as shown by the isobars. A comparison of the lines of equal pressure with the lines of temperature-anomalies thus suggested, led M. Wild to recognise an intimate relation between the two systems. Reasoning from the results arrived at, he has attempted with some success to rectify the isobars over certain regions, where from want of observations their course was somewhat uncertain; and further has even suggested the probable existence of a pressure-maximum in Northern Siberia, of which region however little if anything is positively known, owing to the want of barometric observations. M. Wild's paper, which is of a provisional nature, appears in the *Bulletin* of the St. Petersburg Academy. (It is noted that M. Teisserenc de Bort, in the Paris Academy, has to a certain extent been prosecuting the same subject.)

AS an evidence of the great cold of last winter Mr. Angus M'Intosh, Schoolhouse, Laggan, states in the *Scotsman*, that on June 20 the Balgown peat moss in that parish was still frozen at the depth of 2½ feet beneath the surface.

THE *aurora* has been remarkably frequent at Stykkisholm, Iceland, last winter. From September 5, when the first aurora of the season was observed, to February 28, to which date the observations have been received, auroras were seen on forty-five nights, viz., five in September, eleven in October, four in November, eight in December, twelve in January, and five in February, the phenomena being very brilliant on September 29, December 23, January 31, and February 5.

FOR some time the Registrar-General has been printing in his weekly returns the deaths from small-pox in London under three heads, viz., the vaccinated, the unvaccinated, and those regarding whom no statement is returned. The results show for the whole mortality from small-pox substantially the small-pox curve as given in NATURE (vol. xxiv. p. 144), with its characteristic saddle-shaped maximum, the dip between the two heights of the curve being towards the end of March. On projecting curves of the death-rates for the vaccinated and the unvaccinated, it is seen that the dip in the curve for the whole mortality is due to a diminution of the deaths of the unvaccinated during March as compared with what occurred before and after. In other words, those climatic influences which raise the mortality from small-pox to the annual maxima, first in January-February, when the weather is coldest, and again in May when driest, bear with more fatal effect on the unvaccinated than the vaccinated. As fatal terminations in small-pox cases arise chiefly from complications with other diseases, and as the times of maxima of the curve point to diseases of the nervous system and the respiratory organs as those mostly concerned, even one year's results, particularly a year with cold and dryness so unusually pronounced, may be pointed to as warranting an inquiry of some importance into the relations of the vaccinated and unvaccinated to attacks of small-pox.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

I.

WHEN we have familiarised ourselves with the general phenomena presented to our notice by the analysis of the light proceeding from different sources, and wish to apply this know-

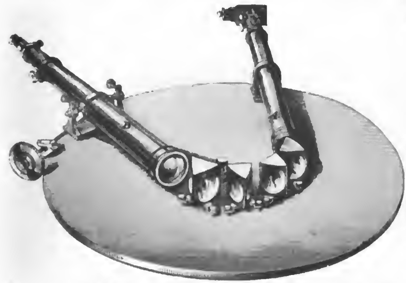


FIG. 1.—Steinheil's form of four-prism spectroscope. A, collimator; B, observing telescope.

ledge to the study of the sun, the first work to which attention must be given is a very admirable memoir of Kirchhoff (1861).[2]

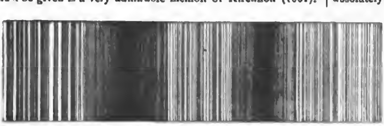


FIG. 2.—Copy of a photograph of the solar spectrum in the region of the thick calcium lines, by Lockyer.

In this, after referring to the prior work of Fraunhofer and others, he goes on to show that the same principles which had then been employed to suggest the extreme probability of the existence of sodium in the atmosphere of the sun, and the probability, therefore, that the dark line D, which we see in the spectrum, was caused by the absorption, by the cooler sodium vapour, of light proceeding from the solar nucleus which was hotter than the vapour; might be applied to other substances, such as iron, cobalt, nickel, and so on; and that if these were experimented on in the same manner, other of the dark lines in the solar spectrum might be explained.

Now I propose, in the first instance, to show what Kirchhoff saw, and what he did—his manner of work. Kirchhoff, and after him Ångström and Thalèn, to whom further reference will be made presently, used spectroscopes placed close or nearly close to the source of light. Kirchhoff's work was done by a spectroscope of this model. We have a slit and collimating lens, a train of prisms, which, of course, during the observations are carefully covered up, and the observing telescope. This instrument may be turned to the sun, or to a cloud illuminated by the sun in case the quantity of light which enters the instrument when turned directly towards the sun is too great to allow of easy observation; or light from the sun or a cloud may be reflected into the instrument by a mirror. Kirchhoff was enabled by means of properly contrived measuring apparatus to map down the positions of the lines observed.

Let us see, first of all, what kind of thing Kirchhoff saw. To give an idea of this I propose to throw on the screen photographs of that portion of the spectrum which is not so readily observable as that upon which Kirchhoff began his work. Here then is an absolutely untouched photograph of a part of the solar spectrum in the blue and violet (Fig. 2). We get in great prominence in the spectrum two very thick lines, which are called H and K, the precise position of which in the solar spectrum are shown by means of the diagram of the spectrum (Fig. 3). By moving his observing telescope along the spectrum, as it were, the telescope being furnished with a delicate micrometer, or some properly-contrived means for defining the exact position of each line, Kirchhoff was in that way able to prepare a map of the whole spectrum. Indeed he did prepare this map with the object of providing himself with a scale of extreme value for the future work which he then laid out for himself. The future work being this:—he wished to determine the positions of the bright lines given by the different chemical elements; having got this information, he

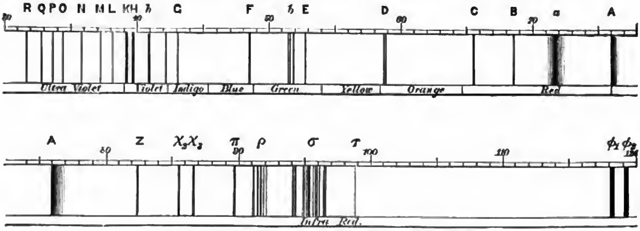


FIG. 3.—Wave-length map of the solar spectrum, including the infra-red.

wished to put the same question to the solar spectrum with regard to each of those elements as already had been done in the case of sodium. How then did he propose to do this? He made an addition to the slit of the spectroscope, such as was then employed. He put a prism in front of it, by means of which he illuminated one half of the slit with the direct light of the sun, and the other half with the light from the vapour employed

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. The first lecture is omitted, as it dealt with the general principles of spectrum analysis.

[2] "Researches on the Solar Spectrum and the Spectra of the Chemical Elements." *Transactions* of the Berlin Academy for 1861. Translation by Prof. Roscoe (Macmillan, 1862).

reflected on to that other half by means of the prism. You will see in a moment, therefore, that it was quite easy by this

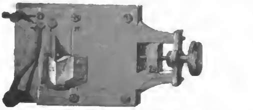


FIG. 4.—Steinheil's slit, showing reflecting prism.

method to see in his observing telescope no longer the spectrum of the sun alone, but the spectrum of the sun together with the

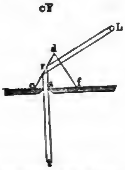


FIG. 5.—Path of light through comparison prism. *d f e*, prism, *l*, light source; *r*, point of reflection; *s*, slit; *r*, light source in front of slit.

spectrum produced by each of the chemical substances which he chose to experiment upon.

Again, anticipating matters somewhat, I can show you something like what Kirchhoff then saw—only again I give you a photograph, and therefore we have a part of the spectrum with which he did not begin his work. In the lower half of this slide we have the solar spectrum. There are the two lines H and K, and in the upper portions we have the bright lines given us by a metallic element—in this case cerium. You see when some of the metallic elements are treated in this way they have a trick of giving us very complicated spectra.

(The photograph was projected on the screen.)

I will show you the iron spectrum which Kirchhoff worked upon. The point was to determine which of the bright lines corresponded with the dark Fraunhofer lines. Over the whole visible reach of the spectrum Kirchhoff mapped the results, for iron; I will give one or two extracts from his paper. He says,[1] "It is specially remarkable that coincident with the positions of the bright lines which I have observed [that is the bright lines from the vapour of iron, using two iron poles with an induction coil] definite dark lines occur in the solar spectrum. By the help of a very delicate method of observation which I have employed, I believe that each coincidence observed by me between the iron lines and the lines of the solar spectrum, may be considered to be at least as well established as the coincidence of the sodium lines." Then he shows, limiting his attention to sixty of the most defined iron lines in the region included in his map, that the betting that there was iron in the sun was about three trillions to one, dealing alone with the absolute matching of the positions of the lines recorded in the solar spectrum. Then he goes on to show that this probability of three trillions to one was rendered still greater by the fact that the brighter a given iron line is seen to be the darker as a rule—and I beg you to mark those words "*as a rule*"—does the corresponding solar line appear. Hence this coincidence must be produced by some cause, and a cause can be assigned which affords a very perfect explanation of the phenomenon. He then gives the cause, which has already been stated by Prof. Stokes.

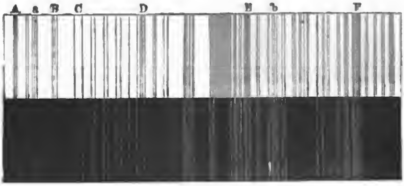


FIG. 6.—Coincidence of some of the bright lines of iron with some of the Fraunhofer lines.

Now before I go further I must point out that there is a considerable assumption here. It is quite easy in an electric lamp to produce the vapour of a meteorite or of any of our terrestrial rocks, to throw their spectra on the screen, and to map them with considerable minuteness; and we say we have the spectrum of such and such a meteorite, or of such and such a rock. Similarly we can get the spectrum of iron terminals, and serve that in the same way, and we are considerably astonished at the wonderful similarity of the results thus obtained. Now chemistry has advanced to a certain stage, and low temperature chemistry comes in and shows us that this meteorite or rock may be an excessively complicated substance. The same chemistry applied to iron shows that nothing can be done with it. But to say that iron cannot be broken up because low temperature chemistry fails to break it up is, you will see, an assumption, for as we undoubtedly get the lines of the constituents of the rock, or of the meteorite, recorded in the spectrum, we may also be registering the lines of the constituents of iron; and it is fair to say this, because we know that in the electric arc we have a stage of heat at which at present no experiment whatever has been made.

Passing on from that point, however, I will ask you to consider somewhat more in detail that part of Kirchhoff's work which deals with the connection between the solar spectrum and the spectra of the chemical elements.[2]

Confining his observations to the region of the solar spectrum between F and D, Kirchhoff found the following coincidences between lines in the spectra of certain elements and the Fraunhofer lines:—

	Lines.		Lines.
Sodium	2	Iron	42
Calcium	13	Chromium	4
Barium	7	Nickel	28
Strontium	2	Cobalt	10
Magnesium ...	3	Zinc	2
Copper	3	Gold	1

Hofmann[3] continued these researches on both sides of the region observed by Kirchhoff as far as A on one side and G on the other, and in addition investigated the spectra of the following metals:—Potassium, rubidium, lithium, cerium, lanthanum, didymium, platinum, palladium, and an alloy of iridium and ruthenium. Hofmann added the following coincidences between

[1] "Researches on the Solar Spectrum.", Roscoe's translation, Part I., p. 18.
[2] Kirchhoff's "Researches," translated by Roscoe, Part I., Supplement.
[3] *Ib.*, Part II., Appendix.

lines of the spectra of the different chemical elements and the dark solar lines:—

	Lines.		Lines.
Calcium	16	Chromium	0
Barium	5	Nickel	4
Strontium	2	Cobalt	4
Magnesium ...	0	Zinc	3
Copper	1	Cadmium	2
Iron	31	Gold	1

The spectra of the additional metals examined gave the following coincidences:—

	Lines.		Lines.
Cerium	2	Platinum	1
Didymium	2	Rubidium and Iridium	1
Lanthanum ...	1		
Palladium	2		

The potassium spectrum could not be obtained by moistening the electrodes with salts of this metal, and when poles of the metal were employed the spectrum was so very feeble that only two prisms could be employed, and hence the position of the lines with regard to the solar lines was not easily determined. He noted that the line Kα was better seen if the Bunsen flame was used instead of the electric spark.

In conclusion Kirchhoff and Hofmann state that, although the additional observations have added nothing to what the previous work had taught, they have confirmed the results of the previous examination. A large number of lines of iron and of calcium occur in the yellow and the blue, and all these were found coincident with well-defined Fraunhofer lines. The probability that nickel is present in the solar atmosphere is greatly increased by the number of new coincidences observed. Cobalt remains doubtful, the solar lines coincident with a considerable number of its bright lines not having been observed. New coincidences in the spectra of barium, copper, and zinc with dark solar lines confirm the presence of those elements in the sun's atmosphere. In the cases of strontium and cadmium the number of coincidences seemed to be too small to warrant the conclusion that those metals are in the sun. The other chemical elements examined, including potassium, did not appear to be visible in the solar atmosphere. The case of potassium however they consider as doubtful, since faint solar lines are very near the red potassium lines.

Note that the passage from the spectrum of the spark to the spectrum of the sun lands us in doubt in many instances.

Kirchhoff next discusses the bearing of this work on the physical and the chemical condition of the atmosphere of the sun. Of course this at once destroyed, at a blow, the idea of Sir William Herschel that the sun was a cool habitable globe with trees, and flowers, and vales, and everything such as we know of here. If the atmosphere were in a state of sufficient incandescence to give these phenomena it was absolutely impossible that anything below that atmosphere should not be at the same time at a higher temperature. He says, "Judging of the height of the solar atmosphere from the phenomena observed in a total eclipse of the sun, it cannot be small in comparison with the radius of the body, and hence the distances which two rays have to pass, one of which proceeds from the centre, and the other from the edge of the disk, do not greatly differ." That was a reply to an objection which had been urged to the effect that if a dark line had been produced by anything absorbing in the atmosphere of the sun, there would be a very considerable difference between the spectrum of the sun's limb and the spectrum of the sun's centre, for the same reason, *ceteris paribus*, that the sun is white at noon-day and reddish at sunset; for since our atmosphere is thin, the light passes through a greater stratum in the one case than in the other. At the sun the light would have to do the same thing, and we should get, therefore, a greater darkening of the limb than is actually observed. He says:—"In addition to this we must remember that the lowest layers of our terrestrial atmosphere are those in which the distance traversed by the light increases most rapidly when approaching most nearly the horizon; for the solar atmosphere, on the contrary, it is those layers which are elevated to a certain position above the solid crust of the sun which are more energetic in producing dark lines than the lower layers which possess a temperature slightly different, and effect but little alteration on the light." He therefore places the region where this absorption takes place at a considerable elevation in the atmosphere of the sun. His notion is that the sun we see is what gives us the continuous spectrum the light of which is absorbed; that above that there is a haze different in structure from it, and yet not competent to give us the absorption lines; that practically none of the absorption phenomena arise from that stratum, but that above this very luminous region of haze the absorption phenomena take place. Such was Kirchhoff's view.

We now pass on for some years to the next step, the work of another eminent man no longer amongst us, Ångström.[1] He took up very nearly the same work as Kirchhoff did, and extended it in certain directions; but he did the work in a different way instrumentally. He was not content with the kind of scale which Kirchhoff had employed, a scale dependent on the construction of his instrument. He wished to have a natural scale. He therefore rejected the use of prisms, and used a diffraction grating. By means of this he obtained what was called, and what is still called, a normal spectrum; and having obtained this he, as Kirchhoff had done before him, endeavoured to determine the coincidence, or want of coincidence, of metallic lines.

By the use of these diffraction gratings measured with great care and expressed in terms of the standard metre, along with a collimator and reading-telescope, the latter fitted with a micrometer screw which enabled the operator to determine with great accuracy the angle through which it moved, Ångström was able to determine with great exactness the wave-lengths of the more prominent line of the solar spectrum from A to H. Using these lines as starting-points he was able, by means of the micrometer, to measure the angle between any of these points and any line which lay between them, and then writing these determinations in interpolation formulæ he was able to compute the wave-length of any observed solar line.

The wave-lengths are given to the second decimal place, the unit being $\frac{1}{10000000}$th of a millimetre.

In the atlas which accompanies this memoir of Ångström the scale is divided, so that one division corresponds to $\frac{1}{10000000}$th of a millimetre of wave length. In addition to marking the wave-lengths of the solar lines, their relative intensities are shown. The map also shows the origin of each line and its correspondence with the lines of metallic spectra so far as these have been determined by Ångström and Thalèn.

The following is a summary of the coincidences observed[2]:—

	Lines.		Lines.
Hydrogen	4	Manganese... ...	57
Sodium	9	Chromium	18
Barium	11	Cobalt	19
Calcium	75	Nickel	33
Magnesium ...	4 (3?)	Zinc	(2?)
Aluminium ...	2 (?)	Copper	17
Iron	450	Titanium	118

Ångström remarks that the number of these lines, about 800,[3] might easily be increased by raising the metals to a higher stage of incandescence. Still, he observes, the number already found is quite sufficient to enable him to refer the origin of almost all the stronger lines of the solar spectrum to known elements, thus confirming the opinion he had expressed in a previous memoir, that the substances which constitute the mass of the sun are doubtless the same as those forming that of the earth. But, he says, the fact must not be lost sight of that there exists, nearly midway between F and G, strong solar lines of which the origin is entirely unknown; still it would be premature to assert that the substances to which these are due are not constituents of our globe.

Of aluminium he says[4] that although it gives brilliant lines in different parts of the spectrum, yet the two lines situated between Fraunhofer's two H-lines are the only ones which appear to coincide with solar lines. By way of explanation of this phenomenon he points out that the violet rays are much the strongest in the spectrum of this metal. He observes that these two lines often present the same phenomenon of absorption as is shown by the yellow sodium lines, which is a proof of their great intensity. He states finally that the point will be cleared up by ascertaining whether the ultra-violet lines of aluminium coincide or not with faint solar lines in that region.

Of zinc he remarks[5] that the two lines he has given of that metal as coincident with solar lines do not correspond with the latter in character, being wide, very strong and nebulous, so that

[1] "Recherches sur le Spectre Solaire" (Upsal, 1869).
[2] *Ib.*, p. 35.
[3] *Ib.*, p. 36.
[4] *Ib.*
[5] *Ib.*

the presence of zinc in the sun remains doubtful. It is noteworthy, however, that there are three lines in the magnesium spectrum which present the same nebulous appearance, and to which there are no corresponding solar lines, and yet magnesium is undoubtedly present in the sun.

Kirchhoff's and Ångström's maps are in all our laboratories, and there is a very considerable difference between them. This difference arises from the fact that whereas Kirchhoff used an induction coil and spark, Ångström varied his experimental method by placing no longer a spark, but the electric arc in front of the slit of his instrument. In this case, therefore, he was determining the spectrum which was produced at the temperature of the electric arc instead of the spectrum which was produced at the temperature of the induction coil. The result of their combined attack is shown in the accompanying table :—

Elements present in the Sun

Kirchhoff.	Ångström and Thalèn.
Sodium.	Sodium.
Iron.	Iron.
Calcium.	Calcium.
Magnesium.	Magnesium.
Nickel.	Nickel.
Barium.	—
Copper.	—
Zinc.	—
	Chromium.
	Cobalt.
	Hydrogen.
	Manganese.
	Titanium.

So far then for that mode of observing the sun which consists in comparing the total light of the light-source with the total light of the sun.

This introduces an important consideration. When we have a light source placed in front of the slit of the spectroscope it is perfectly clear that light from all portions of the light source must illuminate the slit. Similarly, if we content ourselves by pointing the spectroscope to the sun, or to a cloud illuminated by the sun, it is perfectly obvious that the light from all parts of the sun must enter all parts of the slit.

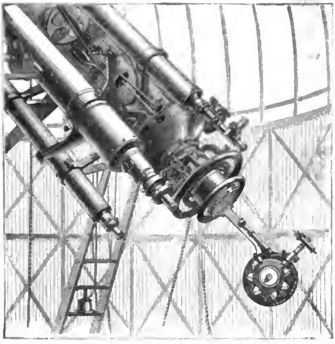


Fig. 7.—The eyepiece end of the Newall refractor (of 25 inches aperture) with spectroscope attached.

Is there any other way of observing the sun along with the light source? You will see in a moment that there is. *We can throw an image of the sun on the slit of the spectroscope.* This work was begun in 1866. If an image of the sun contains, let us say, a spot or a facula, we can see it when we throw it on to the slit. If we can manage to do so we shall get the spectrum of the sun-spot as distinguished from the spectrum of the other portions of the sun, or we shall get the spectrum of the facula as opposed to the spectrum of the other portions of the sun. The manner in which this kind of work is carried on is easily grasped. It simply consists in the use of a spectroscope of large dispersion attached at the focal point of a telescope of considerable power. Here is the eye-piece end of Mr. Newall's refractor, with a spectroscope, with a considerable number of prisms, fixed to the telescope by means of an iron bar, with the slit of it in the position of the focus, so that when the instrument is pointed towards the sun we see an image, in the case of this telescope something like four inches in diameter, with the spots and brighter portions wonderfully and beautifully clear, and by means of the different adjustments of the telescope we can bring now a spot, and now one of the brighter portions of the sun on to the slit, and see if there be any difference between the spectrum of the spot and the spectrum of the general surface of the sun.

If we wish to observe two adjacent spots and compare their spectra, we can rotate the spectroscope and look at both. Again, anticipating matters, I can show what we see to a certain extent, for latterly we have been fortunate enough to obtain some photographs of the spectra of sun-spots.

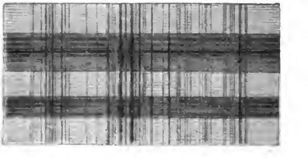


Fig. 8.—Spectrum of Sun-spot, showing the widening of the D lines.

The dark portion gives us the spectrum of the spot throughout the whole length of the spectrum. That is a case of continuous absorption. The continuous radiation of the sun is cut off, but independently of this continuous absorption some of the lines are considerably thickened in the nucleus of the spot (Fig. 8). Now the lines observed in the first instance were the lines of sodium, and the point of the observation was this. Two rival theories had been suggested to explain how it was that the sun-spot was dark. One school said it was due to absorption, and another that it was due to the defect of radiation from the interior gases of the sun. If we had been dealing with defective radiation, we should still have been dealing with radiation, and should have expected to see bright lines; but no obvious bright lines were seen in the spectrum of the spot; what we did see was the thickening and darkening of the lines and the continuous absorption. In the case of the lines of sodium it was very marked; so that we were perfectly justified in saying that the sun-spot was really not produced by any defect of radiation, but was truly and really produced by an increased amount of absorption.

I hope to show you that we can vary the thickness of this line in precisely the same way that it is varied in the different sun-spots, and if then we examine the conditions under which we can experimentally make the line thicker, we shall in that way get some explanation of the thickening of the line in the solar spot. This experiment is rather a difficult one. We will volatilise some sodium in the electric arc and throw its spectrum on the screen. I hope to show that the absorption line is very thick to start with, and then it becomes very thin; if I give it time it will thin down gradually. What is the cause of the thickening and the thinning? It is perfectly obvious. The temperature is practically the same all the time, but we have a very considerable quantity of sodium vapour surrounding the incandescent poles in the first instance. On the further application of the heat this sodium vapour goes away by degrees, and we gradually deal with a smaller quantity, and as we deal with a smaller quantity the line thins. We therefore are justified in saying that when in a sun-spot we get the line of sodium considerably thickened, that is due to the fact that in a sun-spot there is a greater quantity of sodium vapour present.

That was the first experiment with which I am acquainted which enabled us to locate chemical phenomena in any particular part of the sun.

Now although in the year 1866 a great many people were familiar with the spots on the sun, those who had been favoured by a sight of a total eclipse, and many more who had read the accounts of total eclipses, knew that there was a great deal more of the sun than one generally sees. From the time of Stannyan, who observed the prominences at Berne, down to the year 1842, let us say, several eclipses had been observed, and very beautiful coloured phenomena had been recorded by different observers. Red things had been seen projecting round the dark moon during the time of eclipse, and although many held them to be beautful effects produced by the passage of the moon over the sun, or even clouds in the atmosphere of the moon coloured by the strange way in which the solar light then fell upon them, a larger number of people, on the other hand, insisted that these things must really belong to the sun. Now if that were so, it was perfectly clear that we should not be contented with merely observing the chemical nature of the spots.

Having the spectroscope, the things which showed thus, and which up to that time had only been observed during eclipses, would be more or less *felt*, if they were not absolutely rendered visible, by this new instrument; and for this reason: the things seen round the sun during an eclipse were not there for the instant of the eclipse only: they were always there: why did we not see them? The illumination of our own air prevented this. What was our own air illuminated by? By the sun-light. Now whereas increasing dispersion does considerably dim a continuous spectrum for the reason that it makes it extend over a larger area on the screen, it does not dim to any great extent the brightness of a line, so that by employing a considerable number of prisms we ought to be able to abolish the illumination or our air altogether, and in that way we should no longer be limited to determining merely the chemical nature of the spots, we should be equally able to determine the nature of the surrounding solar atmosphere, supposing the phenomena observed during eclipses were really solar, and not lunar or terrestrial.

I will make an experiment with the electric light. I begin with a bright continuous spectrum. We will charge the cup in the lower pole with some vapour which will give us a bright line, in addition to the continuous spectrum due to the poles, and these two things must fight it out between them. If everything goes well what should happen will be this: by first mounting one prism, then two, and then three, the continuous spectrum will be gradually enfeebled, the line keeping the same luminosity

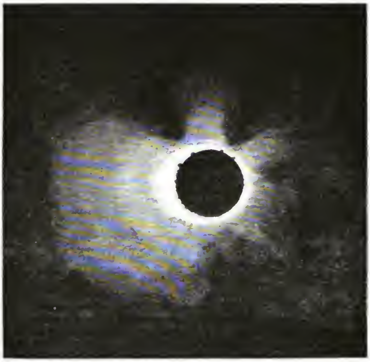


FIG. 9.—Eclipse of 1870. Photograph of the corona taken at Syracuse.

during the whole time; we shall find that relatively the line will be much brighter than the continuous spectrum by the time the experiment is concluded. That was the principle which it was suggested would enable the spectroscope to be used in making what have been called artificial eclipses.

Now if we ask what are the phenomena presented by eclipses, the sort of thing the spectroscope is called upon to observe, we shall see the very considerable advantage of the introduction of the new method. In the first place the eclipses, which are so full of the precious knowledge to be got only at that moment, are almost instantaneous, so far as each particular phenomenon is concerned; and, secondly when the duration is say, four or five or six minutes, which is a very considerable time during an eclipse, and which allows a great deal of work to be done; only a very small part of the more interesting regions of the solar atmosphere is uncovered; one part, of course, when the moon is passing over one limb of the sun, and the other when the moon in passing, liberates it, and brings it into light again. What I would draw chief attention to is the lower part of the brilliant portion seen around the dark moon. We shall have to discuss the upper portion, which is called the coronal atmosphere, or corona, on a later occasion. This mere visual reference, of course, is simply in anticipation of the chemical nature of the different strata upon which we have to operate by the spectroscope, and about which I shall have therefore to tell you in that part of the lecture which has to do with localisation. We shall thus determine, after what has been already said with regard to Kirchhoff's hypothesis as to the position of the region where the lines ought to be seen in the corona, whether during an eclipse we get anything like a justification of this hypothesis. This drawing is really a very beautiful reproduction of an eclipse. We have a round dark moon, which in this case is represented as entirely covering the sun; then these different prominences and luminosities, this wonderful set of streamers, or whatever you like to call them, which seem to veil, or to render less distinct, something else which is lying beyond them. You will see here that some of these prominences are red, and others have a yellow tinge, and that, quite independent of the colour of the prominences, we have the most exquisite coloured effects. Sometimes the radial structure is not so marked, and reveals indications of structure further away from the sun. You see wonderfully delicate tracery, lines being seen now in one part and now in another. In the photograph taken during the eclipse of 1870 we see that the luminosity of the solar atmosphere was excessively irregular, by which I mean that in one part we get a very considerable excess of light, quite independent of the sharply

defined prominences, whereas in other portions the atmosphere of the sun at the same height is not nearly so luminous. Now in none of these cases have we been able to see the thing which struck us most clearly the moment the artificial eclipse system was set at work.

The drawings of the eclipse of 1842 show us that before it was possible to observe the edge of the sun without the intervention of the dark moon there was much evidence which went to show that these red prominences or flames, these different coloured phenomena, were really, so to speak, upper crests of an almost continuous sea round the sun.

In the drawings in question, connecting the prominences,

C

FIG. 10.—Line C (red), with radial slit.

D_1 D_2 D_3

FIG. 11.—Line D_3 (yellow), with radial slit.

F

FIG. 12.—Line F (blue-green), with radial slit.

there is a fine low level of the same colour as the prominence itself. The other drawings give us those prominences after the moon had covered all the lower portion, and that is as good an indication as I can think of of the extreme difficulty of making observations during eclipses, and how important it is that one should have a method which makes us independent of them.

How then is this method carried on? It should be perfectly clear that if instead of using our slit to bisect a spot we allow the slit to fall on the edge of the sun, and then fish round it, if the method is competent to abolish the illumination of our atmosphere, to make the bright lines visible, that here and there if we catch a prominence the slit will be illuminated by the light of the prominence; and if we have the image of the sun very accurately focussed on the slit, if we know the size of the image of the sun, and if we know the length of our slit, the length of the slit illuminated by the prominence will enable us readily to determine the exact height of the prominence; so that if it should happen that there is a sort of external invisible sea round the sun usually invisible, but which this new method will pick up, that we shall get the depth of the sea sounded for us by the length of the line on the slit; and further, if that sea is not absolutely level, but if it swells here and there into waves and prominences, the slit will enable us to determine the height of the prominences. Some copies of very early drawings show exactly what is seen when a prominence is thrown on the slit, and show very well the point at which I have been driving.

Again, if we do fish round the sun in this way, and if these prominences really do give us lines, we have exactly the same method of determining the chemical nature of this exterior sea as Kirchhoff employed in determining the composition of the general light of the sun; only we have this great addition to our knowledge in this case, that whereas Kirchhoff had to suggest an hypothesis to explain the possible locus of the region which produced the lines due to the different chemical substances, we have the hard fact beneath our eyes, because if we pass over the prominence, and if it is built up of iron, let us say, then we shall see iron lines; if it is built up of calcium, then we shall see calcium lines, and so on. Now what are the facts? Here is the first observation that was recorded with absolute certainty touching the chemical nature of the exterior envelope of the sun. We find that we are dealing with the line C; and although Kirchhoff did not tell us the origin of this solar line, he showed that it was quite possible to determine the origin of the lines even if they were produced by gaseous bodies. Ångström went further, and added gaseous bodies to the subject of his investigation, and he found by using a Geissler tube he got a line in the red exactly coincident with the line C in the spectrum of the sun. When therefore we had such an observation as this, showing one of the lines produced by this external sea, coincident with the C line of the solar spectrum, we knew at once

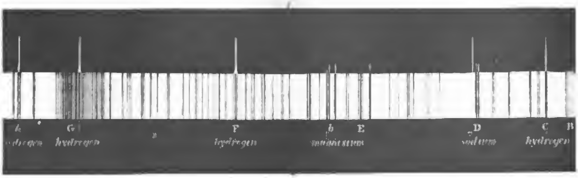


FIG. 13.—Spectrum of the sun's photosphere (below) and chromosphere (above).

that that line was produced by hydrogen. It was obvious, of course, that at once the other lines of hydrogen should be investigated. The next obvious line of hydrogen is F in the blue-green, and when the question was put to this line, in that case also it was found that the prominences gave out no uncertain sound—that the prominences were really and truly composed to a large extent of what we call hydrogen; that is to say, the spectral lines observed when we render hydrogen incandescent are identical with the spectral lines observed when we throw one of the solar prominences on the slit. It was soon found that this continuous ocean, this continuous outer shell of the sun, varied considerably in thickness from time to time, and it was

also found that other substances besides hydrogen, some of which at present we know nothing of, others of which we now think we know a great deal of, also appear side by side with the lines of hydrogen. I have already stated that other substances besides hydrogen have been determined to exist in this lower chromospheric layer. In almost all cases, however, we find that these lines are never so long as the hydrogen line, from which we gather that the magnesium sea, to take a case, is a much shallower one than the hydrogen sea. I should further add here that when the sun is moderately active and can be well observed, as in a fine climate like that in Italy, this magnesium sea can be detected all round the sun, so that we have in the chromospheric layer, first of all, a sea of hydrogen with its prominences, and then at the bottom of this sea another sea of magnesium, which wells up sometimes where the prominences are strongest.

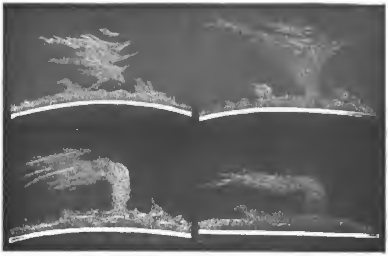


FIG. 14.—Solar prominences (Young), showing lateral currents.

The different forms which these prominences assume are very striking. You will have no difficulty in seeing that there is really a fundamental difference between them, and that all present us with indications of movement, and these movements enable us to apply a test to the theories of the formation of spots and prominences to which Prof. Stokes referred. Prof. Stokes pointed out that a great many phenomena require that the sun-spot shall consist of descending currents, and that these prominences, which when we can see them are fed by the incandescent matter of the sun, should not be descending currents like those in spots, but should be ascending currents,—in fact masses of incandescent vapour shot out from the very bowels of the sun itself. The drawings, which we owe to the skill of Prof. Respigi, drawn by the simple contrivance of opening the slit after we have got the image of the prominence carefully upon it, give us a great many cases of upward move-

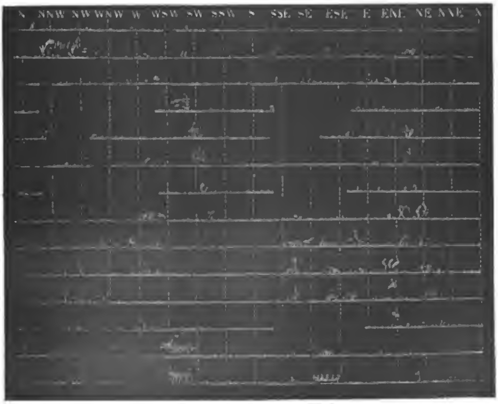


FIG. 15.—Diagram showing how the prominences are daily recorded (Respighi).

ment and lateral movement, sometimes excessively intense, giving indications of their being carried either to the right or to the left of the picture by horizontal currents.

Such then is a first preliminary survey of the method of observing the chemistry of the sun, not as a whole, but of each particular little bit of the sun, chosen here and there, and brought upon the slit of the spectroscope.

J. NORMAN LOCKYER

SOME OBSERVATIONS ON THE MIGRATION OF BIRDS[1]

WHILE showing some friends the astronomical observatory and accessories connected with the College of New Jersey at Princeton, on the night of October 19, 1880, after looking at a number of objects through the 9½-inch equatorial, we were shown the moon, then a few days past its full phase. While viewing this object my attention was at once arrested by numbers of small birds more or less plainly seen passing across the field of observation. They were in many cases very clearly defined against the bright background; the movements of the wings were plainly to be seen, as well as the entire action of flight. In the same way the shape of the head and the tail were conspicuous, when the bird was well focussed. As the moon had not been very long above the horizon the direction of observation was consequently toward the east, and the majority of the birds observed were flying almost at right angles to the direction in which the glass was pointed.

Here then was opportunity for the determination of two points —the kind of birds that were flying, and the general direction in which they were moving. Respecting the first, it was comparatively easy to decide as to what families the species belonged. This point was gained by observing the general shape of the birds, their relative size, the motion of their wings, and their manner of flying; that is, whether the flight was direct or undulating, by continuous strokes of the wings or by an intermittent motion of those members.

Most of the birds seen were the smaller land birds, among which were plainly recognised warblers, finches, woodpeckers, and blackbirds; the relative numbers being in the order of kinds above named. Among the finches I would particularly mention *Chrysomitris tristis*, which has a very characteristic flight; and the blackbirds were conspicuous by the peculiar shape of the tail, from which characteristic I feel most positive in my identification of *Quiscalus purpureus*. I mention such details to explain just how observations were made and conclusions arrived at.

In regard to the second point, with rare exceptions the birds were found to be flying from north-west to south-east. I do not mean that this was absolutely the direction, but that it was the approximate and general one.

It is not within the scope of the present paper to do more than give details on two other points, namely, the estimated number of birds passing through a given space during a given time, and the height at which the birds were most abundant. For the basis of the first of these points it was necessary to note, first, how many birds passed through the field of observation per minute, and second, how near or how far distant from the glass the birds would have to be in order to be seen at all, that is to be in focus.

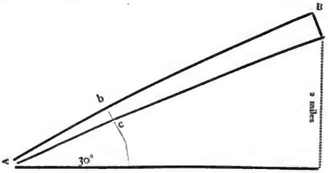
The height of the moon above the horizon in degrees and the two limits of the area of observation—that is how near or how far the birds noted were from the glass—supply the data for determining how high the birds seen were flying, and this, combined with the number noted as passing per minute through the field of observation, gives the basis for computing how many birds were passing through a square mile in a given time.

In this connection it may be well to specify how the two limits of observation were defined. The inferior limit, that is, the nearest point where objects could be seen with distinctness, was easily determined by the power of the glass; this is about one mile distant. The superior limit, or the most distant point, is provisionally assumed to be not more than about four miles away, on the hypothesis that the birds would not fly at a greater height than ten thousand feet. It may appear, as future observations are made, that this last limit is not correct, but the reasons for assuming such a height as the superior limit are sufficient to warrant its use in this case, for birds were observed on this same night at a late hour when the height of the moon above the horizon would make the point at which the birds were noted almost at this great elevation, viz. ten thousand feet.

[1] From the *Bulletin* of the Nuttall Ornithological Club for April.

I am greatly indebted to Prof. Charles A. Young for assistance in these observations, and with his aid have arrived at the conclusion that the average number of birds passing through the field of observation per minute was four and a half. Prof. Young has also kindly assisted me with the details of the problem in regard to the limits and area of the field; and the following diagram and computations are from his study of the matter.

Moon's altitude = 30°; moon's semi-diameter = 15′ 05″. The area of observation is a flat triangle = B, A, C. From this must be deducted the small triangle b, A, c, the area within a mile of the glass. The flight of the birds is thought to be nearly at right angles to the field of observation.



Area of triangle B, A, C = 0·07020 miles.
Area of triangle b, A, c = 0·00439 miles.
Therefore b, B, C, c = 0·06581 = $\frac{1}{15\cdot 2}$ mile.
Distance from A to B = four miles.
Number of birds seen per minute = 4½.
Number of birds per square mile per minute = 68.

W. E. D. SCOTT

[Mr. Scott's novel and important observations definitely establish on a scientific basis several points in relation to the migration of birds that have heretofore rested almost wholly on conjecture and probability.

We have, first, the fact that the nearest birds seen through the telescope must have been at least one mile above the earth, and may have ranged in elevation from one mile to four miles. It has been held that birds when migrating may fly at a sufficient height to be able to distinguish such prominent features of the landscape as coast lines, the principal watercourses, and mountain chains over a wide area. Of this, thanks to Mr. Scott, we now have proof. It therefore follows that during clear nights birds are not without guidance during their long migratory journeys, while the state of bewilderment they exhibit during dark nights and thick weather becomes explainable on the ground of their inability to discern their usual landmarks—points that have been assumed as probable, but heretofore not actually proven.

These observations further indicate that many of our smaller birds migrate not only at night but at a considerable elevation—far beyond recognition by ordinary means of observation. A promising field is here opened up, in which it is to be hoped investigation will be further pushed, not only by Mr. Scott but by others who may have opportunity therefor.—J. A. ALLEN.]

ON THE EQUIVALENTS OF THE ELEMENTARY BODIES CONSIDERED AS REPRESENTING AN ARITHMETICAL PROGRESSION DEDUCIBLE FROM MENDELEEFF'S TABLES

THE relatively quick succession of new elementary bodies which has marked the last decade of scientific progress and which must be considered as the result of chemical research, pioneered and guided by spectroscopic study, has brought very prominently into notice Mendeleeff's most remarkable law of the periodicity of the chemical elements.

Originally published in Russian in 1871, his memoir has since been translated and reprinted by the author in the *Moniteur Scientifique* (July, 1879), and thence has been translated into some of the English journals.

TABLE I.—MENDELEEFF, *Moniteur Scientifique*, No. 451, p. 692, July, 1879.

TYPICAL ELEMENTS. | EXTENDED PERIODS.

Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Values of Progression π.	Symbols and Equivalents.	Approximate Terms of Progression π.
						K=39	12½π=39. 27	Rb= 85	27 π= 84. 82	Cs=133 [3]	42⅓π=132, 99		55 π=172. 79		70 π=219. 91 71 π=223, 06
						Ca=40	12¾π=40, 06	Sr= 87	27¾π= 87, 19	Ba=137 [4]	43⅔π=137. 18		56 π=17 93		72 π=226, 19
							14 π=43. 98	Y= 88	28 π= 87, 96	Di=138 ?	44 π=138. 23	Er=178 ?	56⅔π=178. 02		
						Ti=48 ?	15⅓π=48, 17	Zr= 90	29 π= 91, 10	Ce=140 ? [5]	44⅔π=140, 32	La=180 ?	57⅓π=180. 12	Th=231	73 π=229. 34 73½π=230, 91 74 π=232, 48
						Va=51	16⅓π=51, 31	Nb= 94	30 π= 94, 25		46 π=144, 51	Ta=182	58 π=182, 21		75 π=235, 62
						Cr=52	16½π=51, 81	Mo= 96	30⅔π= 96, 34		47 π=147. 65	W=184	58⅔π=184, 21 60 π=188, 49	Ur=240	76½π=240, 33
						Mn=55	17½π=54, 98	(Terbium ? =99) [2]	32 π=100, 53		48 π=150, 79		61 π=191, 64		
						Fe=56	18 π=56, 55	Ru=104	33 π=103, 67			Os=195	62 π=194. 77		
						Co=59	19 π=59, 69	Rh=104	33⅙π=104. 19	Davyum ? [6]	49 π=153, 93	Ir=197	62⅔π=196, 83		
						Ni=59	19 π=59, 69	Pd=106	33¾π=106, 63			Pt=198	63 π=197, 92		
H=1	⅓π=1, 047	Li= 7	2¼π= 7. 07	Na=23.	7⅓π=23. 04	Cu=63	20 π=62, 83	Ag=108	34⅓π=107, 86	Decipium ?	50 π=157. 08	Au=199	63⅓π=198, 97		
		Be= 9, 4	3 π= 9. 42	Mg=24	7⅔π=24. 08	Zn=65	20¾π=65, 19	Cd=112	35⅔π=112, 05		51 π=160, 22	Hg=200	64 π=201, 06		
		Bo=11	3½π=10, 99	Al=27. 3	8⅔π=27, 23	(Gallium ?)	22 π=69, 12	In=113	36 π=113, 09			Tl=204	65 π=204, 20		
		C=12	4 π=12, 57	Si=28	9 π=28, 27	(Norwegium ?) [1]	23 π=72, 26	Sn=118	37½π=117. 81			Pb=207	66 π=207, 35		
		N=14	4½π=14. 14	Ph=31	10 π=31, 42	As=75	24 π=75. 40	Sb=122	38⅚π=121, 95		52 π=163, 36	Bi=208	66⅔π=201,'13 67 π=210, 49		
		O=16	5⅛π=16, 10	S=32	10¼π=32, 20	Se=78	25 π=78, 54	Te=125	39½π=124. 09		53 π=166, 50		68 π=213, 63		
		Fl=19	6 π=18, 85	Cl=35. 5	11⅓π=35. 60	Br=80	25½π=80, 11	Io=127	40½π=127, 23		54 π=169, 65		69 π=216, 77		

1 $72, 2 = \frac{145.9}{2}$ approx. See NATURE, vol. xx. p. 304. 2 *Jahres Berichte der Chimie*, Von Wagner, 1879, p. 7. 3 132, 56 Goddefroy, NATURE, vol. xv. p. 282. 4 136, 8, *Encyc. Br.* 5 141, *Encyc. Br.* 6 NATURE, vol. xvii. p. 246.

The prevision which it has afforded of yet undiscovered elements, and the fact that the equivalent and properties of gallium correspond with those attributed by Mendeleeff to a then undiscovered metal of one of his series, gives singular importance and weight to this law, and must render it a useful, if not an indispensable guide in the research and determination of further elementary bodies and their equivalents. This point of view has been strongly insisted on by the author in the Chapters iv., v., and vi. of his memoir. In the introductory letter published in the *Moniteur Scientifique* (July, 1879), the author thus speaks of the development of the law:—

"Antrefois ce n'était qu'un schème, qu'un groupement, qu'une subordination à un fait donné, tandisque la loi périodique possède des faits, et tend à approfondir le principe philosophique qui régit la nature mystérieuse des éléments. Cette tendance est de la même catégorie que la loi de Prout, avec cette différence essentielle, que la loi de Prout est arithmétique et que la loi périodique puise son esprit dans un enchainement de lois mécaniques et philosophiques qui constituent le caractère et l'éclat de l'impulsion actuelle des sciences exactes. Elle proclame hautement que la nature des élements depend avant tout de leur masse, et considère cette fonction comme périodique."

Since in all homogeneous bodies the mass is proportional to the volume the consideration of mass leads to and admits that of volume. Therefore the basis of Mendeleeff's law involves the consideration of the volumes of the elementary bodies considered as independent atoms having necessarily limiting forms as well as characteristic weights. Although it may not be possible to fix or define rigorously at present the form of the elementary bodies or atoms when free to move, it is reasonable to admit that the spherical form is that which concords best with the nature of their movements and with their necessary freedom of action.[1]

Parting from this idea, I had been attempting, like many others, to trace out some law connecting the elementary bodies considered as having when free to act a spherical form, but abandoned the idea until I became acquainted with Mendeleeff's law and his Tables. Reverting then to the consideration of the forms and the corresponding relative volumes of the atomic elements, I remarked that a relation could be established between successive elements, as represented by their equivalents given in Mendeleeff's tables, from

1 See Gauden, "Le Monde des Atomes," p. 32 (1873); also NATURE, vol. xxii. p. 56, "On the Physical Aspect of the Vortex-Atom Theory," by S. Tolver Preston.

the point of view of spherical volume expressed by the simple formula $\frac{4}{3}\pi r^3$.

Thus any two spheres being one to the other as γ^3 is to γ_1^3, in atomic elements γ and γ_1 are inconceivably minute, and consequently the values of the two expressions $\frac{4}{3}\pi\gamma^3$ and $\frac{4}{3}\pi\gamma_1^3$ for two given atomic elements if calculated would bear no comparison with the nature of the units by which the equivalents are expressed, while, however, the ratio of their volumes might be adequately represented by entire numbers of the order of the equivalents.

The law of proportionality, according to which the atoms combine, implies that the volumes of the atoms should present

TABLE II.—MENDELEEFF, *Moniteur Scientifique*, 1879, No. 451, p. 701.

Series.	Group I. R^2O.				Group II. RO.				Group III. R^2O^3.				Group IV. RH^2 RO^2.			
		π.[1]		π.		π.		π.		π.		π.		π.		π.
1			H= 1	⅓ π= 1,05												
2	Li= 7	2¼ π= 7,07			Be=9,4	3 π= 9,42			Bo= 11	3½ π= 10,99			C= 12	4 π= 12,56		
3			Na= 23	7⅓ π= 23,04			Mg= 24	7⅔ π= 24,08			Al=27,3	8⅔ π= 27,22			Si= 28	9 π= 28,27
4	K= 39	12½ π= 39,27			Ca= 40	12¾ π= 40,06			—	14 π= 43,98			Ti= 48	15⅓ π= 48,17		
5			(Cu=63)	20 π= 62,83			Zn= 65	20¾ π= 65,19			(Gallium ?)	22 π= 69,12			(Norwegium ?)	23 π= 72,26
6	Rb= 85	27 π= 84,82			Sr= 87	27¾ π= 87,19			Yt= 88	28 π= 87,96			Zr= 90	28⅔ π= 90,06		
7			Ag=108	34⅓ π=107,86			Cd=112	35⅔ π=112,05			In=113	36 π=113,09			Sn=118	37½ π=117,81
8	Cs=133	42⅓ π=132,99			Ba=137	43⅔ π=137,18			Di=138	44 π=138,23			Ce=140	44⅔ π=140,32		
9			(Davyum ?)	48 π=150,79			—	49 π=153,94			(Decipium ?)	50 π=157,08			(Scandium ?)	51 π=160,22
10	—	55 π=172,79			—	56 π=175,93			Er=178	57 π=179,07			La=180	57⅓ π=180,12		
11			Au=197	63⅓ π=198,97			Hg=200	64 π=201,06			Tl=204	65 π=204,20			Pb=107	66 π=107,34
12	—	70 π=219,91			—	71 π=223,05			—	72 π=226,19			Th=231	73½ π=230,91		

Series.	Group V. RH^3 R^2O^5.				Group VI. RH^2 RO^3.				Group VII. RH R^2O^7.				Group VIII. RO^4.			
		π.		π.		π.		π.		π.		π.		π.		π.
1																
2	N= 14	4½ π= 14,14			O= 16	5⅛ π= 16,10			Fl= 19	6 π= 18,85						
3			Ph= 31	10 π= 31,42			S= 32	10⅕ π= 32,04			Cl=35.5	11⅓ π= 35,60				
4	Na= 51	16⅓ π= 51,31			Cr= 52	16½ π= 51,84			Mn= 55	17½ π= 54,98			Fe= 56	18 π= 56,55	Co= 59	19 π= 59,69
													Ni= 59	19 π= 59,69	Cu= 63	20 π= 62,83
5			As= 75	24 π= 75,39			Se= 78	25 π= 78,54			Br= 80	25½ π= 80,11				
6	Nb= 94	30 π= 94,11			Mo= 96	30⅔ π= 96,34			(Terbium ?)	32 π=100,53			Ru=104	33 π=103,67	Rh=104	33 π=103,67
													Pd=106	33¾ π=106,02	Ag=108	34⅓ π=107,86
7			Sb=122	39 π=122,52			Te=125	40 π=125,66			Io=127	40½ π=127,23				
8	—	45 π=141,37			—	46 π=144,51			—	47 π=147,65						
9			—	52 π=163,36			—	53 π=166,30			—	54 π=169,64	—	60 π=188,49	—	61 π=191,64
													Os=195	62 π=194,78	Ir=197	62⅔ π=196,83
10	Ta=182	58 π=182,21			W=184	58⅔ π=184,31			—	59 π=185,35			Pt=198	63 π=197,92	Au=199	63⅓ π=198,96
11			Bi=208	66¼ π=208,13			—	68 π=213,63			—	69 π=216,77				
12	—	75 π=235,62			Ur=240	76½ π=240,32			—	77 π=241,90						

[1] Approximate values of equivalents in terms of progression.

some such proportionality, consequently that for a series of atoms their volumes might be expressed by the terms—

$$\frac{4}{3}\pi\gamma^3 : \frac{4}{3}\pi m\gamma^3 ; \frac{4}{3}\pi n\gamma^3, \text{ \&c., \&c.,}$$

and therefore stand, one to the other, as the succession of terms—

$$\pi ; m\pi ; n\pi, \text{ \&c., \&c.,}$$

that is, be related by a series of terms in function of π.

It may be remarked that this would still be admissible if it be assumed that the forms of the atoms are that of vortex rings as argued in the very remarkable article by Mr. Tolver Preston referred to.

But if such a relation be allowed to exist it is also legitimate to admit a series of which $\frac{1}{3}\pi$, or $\frac{2}{3}\pi$, or π would be the ratio or

difference, the law of proportionality being thus still observed. Now taking the simple arithmetical progression having for difference $\pi = 3, 1416$, it furnishes a series of terms which markedly concord with the series of the atomic weights or equivalents as presented by Mendeleeff's Tables, and the successive blanks occurring in the series established by him in his Tables are very approximately filled up by the succession of the terms of this progression both as regards numerical values and order of succession.

To demonstrate this Mendeleeff's Tables have been drawn out, with the addition opposite each equivalent of the corresponding approximate value in terms of the progression π.

In Table I., containing his grouping into *Typical* and *Great Periods*, there is shown the succession of the elementary bodies and their equivalents, as also the comparative concordant succession of the terms of the progression π with approximate values mostly in terms of $\frac{1}{3}\pi$.

In the Table II., or of *Periodic Series*, the blanks existing in these series as indicated by Mendeleeff are shown to be very approximately filled up by corresponding terms, in value and order of succession, of the progression π. This is markedly the case as regards the gap existing between the series 8 and 10, where are wanting *twelve terms*, which, being filled up by successive terms of the progression π, the thirteenth term, corresponding to the equivalent of Lanthanum = 180 (?) is represented in the progression π by the value $57\frac{1}{3}\pi = 180, 1164$, or approximatively by the value $57\pi = 179, 0712$.

Considering this progression of terms of π it will be found that of the sixty-five elementary bodies given in Mendeleeff's Tables the following corresponding equivalents are represented, with an approximation of less than a unit, by terms of the progression :—

H	=	1	...	$\frac{1}{3}\pi$ = 1, 0472
Be	=	9, 4	...	3π = 9. 4248
C	=	12	...	4π = 12, 5664
O	=	16	...	5π = 15, 7080
Fl	=	19	...	6π = 18, 8496
Si	=	28	...	9π = 28, 2744
Ph	=	31	...	10π = 31, 4159
Ca	=	40	...	13π = 40, 8404
Ta	=	48 ?	...	15π = 47, 1240
Va	=	51	...	16π = 50, 2656
Fe	=	56	...	18π = 56, 5488
Ni	=	59	...	19π = 59, 6904
Co	=	59	...	" "
Cu	=	63	...	20π = 62, 8313
Zn	=	65	...	21π = 65, 9736
(Gallium ?	=	69·1	...	22π = 69, 1150)
(Norwegium ?	=	72	...	23π = 72, 2566)
As	=	75	...	24π = 75, 3984
Se	=	78	...	25π = 78, 5400
Rb	=	85	...	27π = 84, 8232
Yt	=	88	...	28π = 87, 9648
Zr	=	90	...	29π = 91, 1064
Nb	=	94	...	30π = 94, 2477
(Terbium ?	=	99	...	32π = 100, 5312)
Ru	=	104	...	33π = 103, 6725
Rh	=	104	...	" "
Pd	=	106	...	34π = 106, 8144
In	=	113	...	36π = 113, 0976
Sb	=	122	...	39π = 122, 5224
Te	=	125	...	40π = 125, 6636
Cs	=	133	...	42π = 131, 9472
Di	=	138 ?	...	44π = 138, 2300
Ce	=	140–141	...	45π = 141, 3720
(Davyum ?	=	153	...	49π = 153, 9384
Decipium ?	=	157	...	50π = 157, 0795)
Er	=	178 ?	...	57π = 179, 0712
La	=	180 ?		
Ta	=	182	...	58π = 182, 2128
Os	=	195	...	62π = 194, 7786
Ir	=	197	...	63π = 197, 9208
Pt	=	198		
Hg	=	200	...	64π = 201, 0624
Tl	=	204	...	65π = 204, 2040
Pb	=	207	...	66π = 207, 3450

Such a concordance must be taken as some proof of the reality of a certain correspondence between the values of the equivalents and those of the terms of the progression π.

It is fully admitted that the equivalents are but relative, both as regards their number and their numerical values, to the forces which the present state of chemical analysis can bring to bear on matter, and admitting the existence of a law of progression by which the equivalents may be connected, such a progression should as a matter of necessity differ both as regards the number of representative terms and their values from the present received succession and numerical values of the equivalents, and consequently show discordances in certain places and approximate concordances in others; such is shown by the terms of the progression π.

J. P. O'REILLY

May, 1880

BIRDS OF THE SOLOMON ISLANDS

IN a paper "On the Birds of the Solomon Islands," by E. P. Ramsay, F.L.S., &c., Curator of the Australian Museum, Sydney, read before the Linnean Society of New South Wales, February 23, 1881, the following new birds were described :—

1. *Graucalus elegans*, sp. nov.—A species allied to *G. hypoleucus* of Gould, but differing in its smaller size, whiter undersurface, broad jet black band on loreal region, extending below the eye, and in having ashy grey shoulders.

2. *Piezorhynchus Richardsii*, sp. nov.—A remarkably distinct species, with the body and the wings and tail above black, ossified nape, and hind neck white, head and throat black, chest and remainder of the under surface chestnut; this species comes from the Island of Nyi, and has been named in compliment to Lieut. Richards, R.N.

3. *Myzomela Tristrami*, sp. nov.—A jet black myzomela of large size, the bill strong and yellow, with end black, bases of the inner webs of the quills below ashy. This species is allied to, but distinct from, *M. nigra*, *M. Forbesii*, *and M. pammelana.*

4. *Mygomela pulcherrima*, sp. nov.—This fine species has the whole of the head, neck, chest, breast, and sides of the body and flanks, the interscapular region, rump, and upper tail coverts of a rich deep crimson, the remainder of the plumage black. The extent of the scarlet on the flanks and breast is greater than in either of the allied species *M. cardinalis* and *M. nigriventris.*

5. *Zosterops* (*Tephras?*) *olivacens*, sp. nov.—In this genus there is no trace of white round the eye, and the bill has quite a different contour than that of any species of the genus *Zosterops.* The first and sixth primary quills in this species are equal, and the third is equal in length to the fourth. The general colour above is a uniform dull brown washed with olive, inclining to smokey brown on the head, inner webs of the quills below margined with white, under surface light ashy brown, almost white on the abdomen, length about five inches.

6. *Nasiterna finschii*, sp. nov.—A very distinct species of a uniform grass-green tint, paler on the abdomen, under tail coverts yellow, length 3·8 inches.

The paper contains notes on six or eight other species of interest and a fine collection of Solomon Island birds were exhibited—about fifty species.

OUR ASTRONOMICAL COLUMN

THE VARIABLE STAR χ CYGNI.—A maximum of this variable should now be close at hand. Prof. Winnecke assigns it to July 31, rather later than the average period of the last few years would give it. Its brightness at maximum has varied during the present century from 4m. to a very little above 7m. In vol. vi. of the Bonn Observations, Argelander has given nine observations of the position of this star, about which there has been so much and unnecessary confusion. Its place for 1880·0 is in right ascension 19h. 45m. 57·33s., declination 32° 36′ 42″·1. A comparison of Lalande's observation in 1793 with Argelander's shows that there is no appreciable proper motion. The variability of χ Cygni was discovered in 1686 by Kirch, whose first observed maximum is dated November 28, 1687.

COMET 1881 *b*.—It appears this comet was detected at Sydney as early as May 22, so that we may yet receive observations from the Australian observatories made nearly a week before the first of those made at Rio Janeiro. The orbit, founded upon post-perihelion places, which we published last week, gives the comet's place on May 22 at 10 p.m. at Sydney in right ascension 4h. 58·5m., declination 35° 33′ S., and at this time the comet was distant from the earth 0·772 of the earth's mean distance from the sun. M. Cruls' first position, deduced from the observations at Rio is as follows :—

M.T. at Rio.		R.A. h. m. s.		Decl. ° ′ ″
May 29·27980	...	5 2 3·8	...	−31 15 24·9

The comet was nearest to the earth about midnight on June 19, when its distance was 0·283.

The ephemeris snbjoined is for Greenwich midnight :—

	R.A. h. m.	N.P.D. ° ′	Log. distance from the Earth.	Log. distance from the Sun.
July 22	11 44·2	8 5·0	9·9290	0·0065
23	11 55·5	8 13·5		
24	12 5·9	8 22·7	9·9479	0·0173
25	12 15·5	8 32·4		
26	12 24·5	8 42·5	9·9657	0·0281
27	12 32·9	8 52·9		
28	12 40·7	9 3·4	9·9825	0·0388
29	12 48·0	9 13·9		
30	12 54·9	9 24·5	9·9982	0·0493
31	13 1·3	9 35·2		
August 1	13 7·4	9 45·9	0·0131	0·0597
2	13 13·2	9 56·4		
3	13 18·7	10 7·8	0·0271	0·0700
4	13 24·0	10 17·2		
5	13 29·2	10 27·4	0·0404	0·0800
6	13 34·3	10 37·4		

COMET 1881 *c*.—Telegrams from the Smithsonian Institution at Washington notify the discovery of a comet at the Observatory of Ann Arbor, by Mr. Schäberle, apparently on July 16; it was situate, according to the telegrams, nearly in the right ascension of Capella in 48° declination (or ? 38°).

NEAR APPROACH OF VENUS TO 107 TAURI.—Prof. Winnecke has circulated a note in which he suggests a method of determining the solar parallax from observations of this planet, when it approaches or occults a fixed star. We refer to the note at this time, only to draw attention to a close approach of the planet to 107 Tauri, a star of 6·5m. on the morning of July 24. According to a calculation by one of Prof. Winnecke's pupils the star will be occulted, but there appears to be some mistake here. Taking the star's place from the Greenwich Catalogue of 1864 and the Radcliffe Observations 1870-75, its apparent position will be R.A. 5h. 1m. 51·33s., Decl. +19° 42′ 15″·1, and at conjunction in R.A. July 23 at 20h. 26m.·2 G.M.T., the geocentric difference of declination (Venus-star) is 28″·7; this difference is reduced by the effect of parallax at Greenwich to 22″·9, and Wichmann's value of the semi-diameter of the planet being 10″·8, it appears neglecting tabular error of place, that at conjunction in right ascension, the sonth limb of Venus will be 12″ north of the star.

[Since the above was in type Dr. Gould's observations of the great comet at Cordoba have been received; they show that at the end of May the elements upon which our ephemeris is founded may give the comet's position with errors of + 1′·0 in R.A., and + 3′·8 in declination.]

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale des Sciences de Belgique, No. 3.—On the intensity of scintillation during auroræ boreales, by M. Montigny.—Observations on the anatomy of the adult African elephant, by MM. Platean and Liénard.—On a general property of liquid sheets in motion, by M. Vander Mensburghe.—On the triangulation of the kingdom, by M. Adan.—On the magnetism of bodies in relation to their atomic weight, by M. Enera.—On the broadening of the lines of hydrogen (third communication), by M. Fiévez.

No. 4.—Liberty, and its mechanical effects, by M. Delbœuf.—Note on *Pretswichia rotundata*, J. Prestwich, discovered in the coal schist of Hornu, near Mons, by M. de Koninck.—On the transformation of methylchloracetol into acetone and thiacetone, by M. Spring.—On the blood of insects, by M. Fredericq.—Note on certain co-variants, by M. le Paige.—Researches on the reproductive apparatus of osseous fishes, by Mr. MacLeod.—On the stratigraphic position of remains of terrestrial mammalia discovered in Eocene strata of Belginm, by M. Rutot.

Archives des Sciences Physiques et Naturelles, June 15.—International Geological Congress at Bologna (1881); report of Swiss Committee on unification of nomenclature, by Renevier.—On an artificial reprodnction of Gaylussite, by MM. Favre and Soret.—Study on palæontological and embryological development, by M. Agassiz.—Researches on alternating generations of Cynipides of oak, by M. Adler.—Observations on luminous plates, by M. Dufour.—Apparatus for Lissajous' curves, by the same.—The telephone and return eurrents of telegraph lines, by M. Cauderay.

Atti della R. Accademia dei Lincei, vol. v. fasc. 13.—Astronomical and physical observations on the axis of rotation and the topography of Mars, at the Royal Observatory of Brera, in Milan, with the Merz equatorial, by S. Schiaparelli.—Preliminary note on the volcanic ejection of tufa of Nocera and Samo, by S. Scacchi.—Researches on the variations of tone in the human blood-vessels, by Signori Rajardi and Mosso.—On observations of solar spots, faculæ and protuberances, at the Royal Observatory of the Roman College, during the first quarter of 1881, by V. Tacchini.—On the mean monthly and annual temperatures and the daily thermometric excursions dedneed from observations at the observatory of the Roman College, by the same.—A supposed new red star, by the same.—Observations on small planets, by the same.—On the depolarising property of saline solutions, by S. Macaluso.—On the constitution of derivatives of santonine, by S. Cannizano.—On the action of bromine on naphthaline, by S. Magatti.—Attempt at synthesis of pyragallic acid, by the same.—On a new (3rd) homologue of pyrol contained in oil of Dippel, by Signori Clamician and Dennstedt.—On cadaveric poisons, by S. Moriggia.—On the saccharifying ferment of wine, by S. Selmi.—Some theorems on geometry of *n* dimensions, by S. Veronese.—On the skeleton of Scelidoterian exhibited in the geological museum at Bologna, by S. Capellini.—Primordial fauna in Sardinia, by S. Meneghini.—On botanical taximony, by S. Carnel.—Ephemerides and statistics of the Tiber in 1880, by S. Betocchi.—Determination of the difference of longitude between Rome and Milan, by Signori Respighi and Celoria.—Absolnte value of gravity at Rome, by S. Respighi.—On corrections in elliptical co-ordinates in the calculation of planetary perturbations, by S. de Gasparis.—Some artistic, literary, and geographical fragments of Leonardo da Vinci, by S. Govi.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 22.—R. Etheridge, F.R.S., president, in the ehair.—Thomas Hart and David William Jones, Coronel, Chili, South America, were elected Fellows of the Society.—The following eommnnications were read:—Description of a new species of coral from the Middle Lias of Oxfordshire, by R. F. Tomes, F.G.S. The species of coral described in this paper was referred by the anthor to the genus *Thamnastræa* and the sub-genus *Synastræa*, under the name of *Thamnastræa Walfordi*, in honour of its discoverer, Mr. E. A. Walford. The specimen was from the *Spinatus*-beds of the Marlstone, at Aston-le-Walls, Oxfordshire. Like *Thamnastræa Etheridgei*, previously described by the author (*Q. J. G. S.* xxxiv. p. 190) from the Middle Lias of Oxfordshire, this species presents the same sub-generic characters as *T. arachnoides* of the coral rag of Steeple Ashton; and the anthor remarks upon the fact that the only species known from the English Lias resemble corallian rather than Inferior-Oolite forms.—Note on the occurrence of the remains of a Cetacean in the Lower Oligocene strata of the Hampshire basin, by Prof. J. W. Judd, F.R.S., Sec.G.S. With a note by Prof. H. G. Seeley, F.R.S., F.G.S. The author referred to the rarity of remains of marine mammalia in the Lower Tertiaries of Britain, the only recorded species being *Zeuglodon Wanklyni*, Seeley, from the Barton clay. The single specimen in his possession was obtained at Roydon, about a mile and a half north of Brockenhurst, where the beds exposed in the brickyard consist of sandy elays crowded with marine fossils, and resting upon green freshwater clays, with abundance of *Unio Solandri* belonging to the Headon series. The author briefly referred to the qnestion of the horizon of these deposits, which he regards as belonging to the same great marine series as the beds of Brockenhurst and Lyndhurst, which he holds to be Tongrian or Lower Oligocene. The Cetacean vertebra obtained by Prof. Judd was stated by Prof. Seeley to be a caudal vertebra, probably the eighth, but not later than the twelfth, of a species belonging, or closely related to the genus *Balænoptera*, and especially approaching *Balænoptera laticeps*, a species of the North Sea which appears to range to Japan. Prof. Seeley regarded it as representing a new species, which he named *Balænoptera Juddii*.—Descrip-

tion of a peat-bed interstratified with the boulder-drift at Oldham, by G. H. Hollingworth, F.G.S. The author described a deposit of peat interstratified with boulder-drift, exposed in a railway-cutting at Rhodes Bank, Oldham. The depth of the section was only 14 feet, and it showed:—

1. Soil	8 to 10 inches.
2. Boulder-clay, with beds and strings of peat	2 to 6 feet.
3. Main bed of peat, containing mosses, exogenous stems, and beetles	2 in. to 1 ft. 9 in. (average 15 in.).
4. Fine blue clay (floor)	2 inches to 1 foot.
5. Current-bedded coarse sand and fine gravel	4 inches to 2 feet.
6. Boulder-clay.	

The mosses in the peat are of northern type.

Silurian uniserial *Stomatopora* and *Ascodictya*, by G. R. Vine, communicated by Prof. P. Martin Duncan, F.R.S. For the genus *Stomatopora* the name *Alecto* has priority; but as that had previously been applied to a member of the class Echinodermata, the author preferred the later name. Species of the genus have also been described under the generic name *Aulopora*. The author has received from Mr. Maw more than two hundredweight of washed *débris* of Wenlock shale, about thirty pounds of which, from twelve localities, he has examined. It contains a moderate amount of Polyzoan remains, generally water-worn. The author described the following species:—*Stomatopora inflata* and *dissimilis*, *Ascodictyon stellatum* and *radians* (with a variety *siluriense*), and discussed the characters of the genera.—Note on the diamond-fields of South Africa, by E. J. Dunn, communicated by Prof. Ramsay, F.R.S. The passes or necks of decomposed gabbro, &c., at the Kimberley, Bultfontein, and other diamond-mines have now been excavated to a considerable depth, and have allowed excellent sections of the sedimentary beds through which they have broken to be examined. These are generally but little disturbed, and may be traced over an area of many square miles. Immediately beneath the surface are, generally, yellowish shales, with remains of small Saurians: and beneath these a mass, certainly more than a hundred feet thick, of black carbonaceous shales, with occasional thin bands of coal. It is found that the diamonds are more abundant and of better quality when the level of the black shales is reached. It seems, therefore, not improbable that the carbon requisite for the formation of diamonds was obtained from these shales. Some other points of minor interest were also noted in this paper.—On a new *Comatula* from the Kelloway Rock, by P. H. Carpenter, M.A., Assistant Master at Eton College, communicated by the President. The specimen, to which the author's attention was called by R. Etheridge, jun., is in the national collection; he proposes for it the name *Actinometra calloviensis*. The specimen is from the Kelloway rock, of Sutton Benger; the whole diameter is 15 mm.; diameter of centrodorsal ·6 mm. Three species of this genus are already known from the British Jurassic rocks; two are only known from their centrodorsals, which are each different from that of *A. calloviensis*. The third is *A. cheltonensis*, from the Inferior Oolite, known only by its radials and basals, which are different from those of the present specimen. To this *Antedon Picteti*, from the Valangian of the Continent, has some resemblance. It is, however, a true *Actinometra*, differing chiefly from existing forms in retaining its primary basals without their having undergone transformation into a rosette.—Descriptive catalogue of Ammonites from the Sherborne district, by Sydney S. Buckman. Communicated by Prof. J. Buckman, F.G.S., F.L.S., &c. In this paper the author gave a list of the Ammonites from the Inferior Oolite of the neighbourhood of Sherborne, in which he enumerated about forty-seven species, and stated that he had about fifty more which appear to be undescribed; fully one half have the mouth-termination perfectly preserved. The author indicated the zones into which the rocks furnishing these Ammonites could be divided, as shown at Oborne, near Sherborne, at Wyke Quarry, and at Bradford Abbas, and indicated the characteristic fossils of each; he also gave the principal synonyms of the species referred to, and discussed some of their characteristic peculiarities.—The next meeting of the Society will be held on November 2, 1881.

Entomological Society, July 6.—Mr. H. T. Stainton, F.R.S., president, in the chair.—One new Member and one Subscriber were elected.—Mr. W. L. Distant exhibited the sexes of *Morpho Adonis*.—Miss E. A. Ormerod exhibited some elm-leaves bleached by the attacks of a Coleopterous larva; and larvæ of a species of *Dolerus* and of *Charæas graminis*, feeding on grass.—Rev. E. A. Eaton exhibited drawings by Mr. A. T. Halleck of the nymphs of various *Ephemeridæ*.—The Secretary read the report of the Committee appointed at the last meeting to inquire into the history of an insect found feeding on the eggs of locusts in the Troad. It proved to be a dipterous insect apparently belonging to the *Bombyliidæ*; and specimens were exhibited by Sir S. S. Saunders.—The following papers were then read:—Mr. F. Moore, descriptions of new Asiatic diurnal *Lepidoptera*.—Mr. D. Sharp, on the species of the genus *Euchroma*.—Mr. J. W. Douglas, observations on the species of the homopterous genus *Orthezia*.—Mr. A. G. Butler, on the Lepidoptera of the Amazons collected by Dr. Trail during the years 1873-1875. Part iv. Geometrites.—Baron Osten-Sacken, note on the larva of Nycteribia.—Mr. W. F. Kirby, notes on new or interesting species of *Papilionidæ* and *Pieridæ* collected by Mr. Buckley in Ecuador.

Edinburgh

Royal Society, June 6.—Sir Wyville Thomson, vice-president, in the chair.—Prof. H. Alleyne Nicholson, in a paper on the structure of the skeleton in *Tubipora*, and on the relations of the genus to *Syringopora*, argued that the similarity between the skeletons of these genera was only apparent, and that careful and minute microscopic study proved them to be built up in very different ways. In the former genus the skeleton is porous and made up of fused spicules. There are no tabulæ, and the axial tube, when present, seems to be simply the calcified wall of the body cavity, coming into contact with the external walls only at the nodes which mark the stages of growth. In the Syringopora again the skeleton is not porous, while there are true septa and funnel-shaped tabulæ which give rise to an axial tube.—Prof. Tait communicated a note by Mr. A. P. Laurie on an iodine battery, whose great merit is that it combines the simplicity of a single fluid cell with an electromotive force practically constant. Carbon and zinc plates dip into a solution of iodine in iodide of zinc, the iodine preventing polarisation. The zinc should not be amalgamated, and should be removed from the solution when the cell is not working. As tested by a quadrant electrometer, the electromotive force was very approximately one volt, and was hardly diminished, even after half an hour's short-circuiting.—In a note on chemical affinity and atomicity Mr. W. Durham brought forward certain objections to the generally-accepted theory of atomicity, arguing that there was no sufficient ground for assuming that one atomicity of a given element was saturated by one atomicity of another element in the compound, that this assumption led to the necessity of giving to certain elements different atomicities, and that it was more rational to suppose a given atomicity distributed among several of the like constituents of the compound.—Sir Wyville Thomson communicated a paper on the physical and biological conditions of the channel between Scotland and the Farö Islands. A series of soundings taken last summer had proved the existence of a narrow ridge running across this channel and flanked on both sides by deep water. Down to a depth of 260 fathoms (the depth of the ridge) the ocean water on both sides of this ridge was at much the same temperature, while at lower depths the water to the north-east was markedly colder than that to the south-west; thus at 450 fathoms the temperatures of these regions were respectively 30°·5 F. and 47°·2 F. The characteristic fauna of these regions showed a corresponding diversity, that of the north-east basin being similar to the Scandinavian fauna, and Arctic in character, that of the south-west being similar to the fauna found in the warmer waters all over the ocean bed. Many new forms were discovered in both of these regions.

June 20.—Prof. Maclagan, vice-president, in the chair.—Prof. Chrystal, in a note on Sturmian functions, gave a simple demonstration of a theorem of Joachimsthal, expressing a class of these functions as the successive minors of a symmetrical determinant.—Dr. Herdman communicated Part iv. of the Preliminary Report on the *Tunicata* of the *Challenger* Expedition.—Mr. D. B. Dott gave a short account of a series of experiments which he had made on comenic acid and its salts, which he regarded as establishing its dibasic character.—Dr. Macfarlane read a paper on Morgan's systems of consanguinity and affinity, which he had examined with the help of his analysis of relationships. The paper consisted of two parts, the first being a criticism of the tables of data, the second of an explana-

tion of the so-called classificatory methods. The classification proceeds according to difference of generation, and is merely one mode in which the relationship ideas may be expressed in words. Mr. Morgan's hypothesis of a consanguine and of a Panaluan family are contradicted by the data which they are introduced to explain.—Prof. Tait communicated a note on a proposition in the theory of numbers.

Paris

Academy of Sciences, July 11.—M. Wurtz in the chair.—The following papers were read:—On the formation of the tails of comets, by M. Faye. A reply to M. Flammarion. The tail is not rigidly connected with the nucleus. The repellent force is proportional to the surfaces, is weakened by interposition of a screen, is not propagated instantaneously, and varies in inverse ratio of the square of the distance.—Theory of plane flexion of solids and consequences relating to construction of astronomical telescopes, and to their regulation, getting rid of deviations of the optic axis due to flexion, by M. Villarceau.—On the velocity of propagation of explosive phenomena in gases, by M. Berthelot. The experiments were with mixtures of hydrogen and oxygen and of carbonic oxide and oxygen (2 vols. to 1). These were placed in a long iron tube (open or close, fixed in various positions, &c.), and were inflamed with an electric spark; the passage of the wave was measured by an electric method. The velocity was in general about 2500 m. per second. Explosive phenomena are more complex than a simple motion of translation or even than the propagation of a sound wave.—Reply to M. de Lesseps on M. Roudaire's project, by M. Cosson.—On the borings made in strata to be traversed by the Panama Canal, by M. de Lesseps. The strata of large section will offer sufficient consistency for formation of talus, while not preventing the resistance of hard rock.—Study in experimental thermodynamics on steam-engines, by M. Ledieu.—Photography of the spectrum of comet *b* 1881, by Dr. Huggins. M. Berthelot thought the spectra rendered probable the electric origin of the proper light of comets.—Influence of phosphoric acid on phenomena of vegetation, by M. de Gasparin.—Remarks on the accidents caused by use of sulphide of carbon in treatment of vines in the South of France, by M. Cornu.—Shocks of an earthquake at Gabes on June 13 were reported.—On the comet of 1881 observed at the Imperial Observatory of Rio de Janeiro, by M. Cruls.—Observations on the same comet at Algiers Observatory, by M. Trépied.—Further observations by M. Wolf and by M. Thollon (See page 261).—Attempt at explanation of the tails of comets, by M. Picart. A comet, consisting of gaseous matter and luminous ether, appears, at a distance from the sun, in spheroidal form (the luminous ether being then invisible). But on nearing the sun the luminous ether of that star repels the luminous ether of the comet; hence the tail.—On the polarisation of the light of comets, by M. Prazmowski. The comet is shown to reflect solar light abundantly.—New method of determining certain constants of the sextant, by M. Gruey.—On Kleinian groups, by M. Poincaré.—On a general means of determining the relations between constants contained in a particular solution, &c. (continued), by M. Dillner.—On the three centrifugal axes, by M. Brassinne.—On the absolute measurement of currents by electrolysis, by M. Mascart. By careful experiment he finds the intensity of the current capable of producing in one second the electrolysis of 1 equiv. of a substance expressed in milligrammes is equal to 96·01 w., or say 96 webers.—On the reality of kinematic equivalence in undulatory optics, by M. Crouillebois. M. Cornu made some remarks.—On the chlorides of iron, by M. Sabatier.—On the oxychlorides of strontium and of barium, by M. André.—Experimental researches on decomposition of picrate of potash; analysis of products, by MM. Sarrau and Vieille.—On decipium and samarium, by M. Delafontaine. He reserves the name *decipium* for the radical of the earth having an equivalent of about 130; *samarium* for the other metal (in samarskite) whose absorption spectrum was described by M. Lecoq. (The equivalent of samarine is probably under 117.)—Action of peroxide of lead on alkaline iodides, by M. Ditte.—On ethers of morphine considered as phenol, by M. Grimaux.—Researches on tertiary monamines; III. Action of triethylamine on ethers with hydracids of secondary and tertiary alcohols, by M. Reboul.—On cyanised camphor, by M. Haller.—On the composition of hydrosulphite of soda and of hydrosulphurous acid, by M. Bernthsen.—Two facts relating to decilene (oil of turpentine), by M. Maumené.—On *viscose*, or the gummy substance of viscous fermentation, by M. Béchamp.—Determination of urea with the aid of titrated hypobromite of soda, by M. Quinquaud.—Researches on animal heat, by M. d'Arsonval. By direct calorimetry he proves the great absorption of heat by the egg in incubation during the first day (a fact otherwise proved by M. Moitessier). Oxygen is abundantly absorbed and carbonic acid emitted. During sleep or complete rest, animals absorb much oxygen and make little heat, the emission of carbonic acid varying slightly. The author hardly ever found agreement between the heat measured directly and the heat calculated from respiratory combustions; this is because organic combustion is of the order of fermentation. The chemical method gives the sum; direct calorimetry the difference. The two methods should be combined.—Action of maté on gases of the blood, by MM. d'Arsonval and Couty.—Absorbed by the stomach or the veins, maté diminishes the carbonic acid and the oxygen of arterial and venous blood enormously (sometimes a third or a half of the normal quantity).—On the seat of cortical epilepsy and of hallucinations, by M. Pasternatzky. Cortical epilepsy is really what the name implies. The hallucinations he produced in a dog with absinthe he attributes to excitation of the sensitive sub-cortical centres by that substance.—On the alterations of the cutaneous nerves in pellagra, by M. Dejerine.—On venous circulation by influence, by M. Ozanam. Among the various causes of progression of blood in the veins is an important influence exercised on each vein by the artery associated with it. The vein-walls experience a rebound from the arterial movements.—On the structure of the oothecæ of Mantes, and on the hatching and first moulting of the larvæ, by M. Brongniart.—Chemical researches on the product of secretion of the ink-bag of Cephalopoda, by M. Girod.—On the synchronism of the marine carboniferous fauna of Ardoisière (Allier), and the anthraciferous fauna of Roannais and Beaujolais, by M. Julien.—M. Laurey noted, about the comet, that the sunlight illuminated only the left part, leaving the right dark—a true cometary phase.

Vienna

Imperial Academy of Sciences, June 23.—L. T. Fitzinger in the chair.—L. T. Fitzinger, examination of some species which were till now incorporated with the species *Ursus arctos*.—F. Steindachner, contributions to the knowledge of African fishes (contains a description of a new species of *Sargus* from the Galapagos Islands).—Job. Mayer, on the trajectory of the 1880 *b* comet.—Max. Margules, on the motions of viscous liquids and on the figures of motion.—F. Strohmer, on the occurrence of ellagic acid in pine-bark.—Ernst Schneider, a sealed packet (experiment on the construction of high-power telescopes).—MM. Neumayr and Emil Holub, on the fossils at the Uitenhage formation in South Africa.—M. Neumayr, studies on fossil Echinodermata.—V. v. Lang, on the coefficient of refraction of concentrated solutions of cyanine.—L. Haitinger, on the occurrence of malic and citric acid in *Chelidonium majus*.—Dr. T. Puluj, a sealed packet (without inscription).

CONTENTS

THURSDAY, JULY 28, 1881

MISS GORDON CUMMING'S "FIJI"

At Home in Fiji. By C. F. Gordon Cumming. In Two Volumes. With Map and Illustrations. Second Edition. (London: Blackwood and Sons, 1881.)

MISS GORDON CUMMING is a most indefatigable traveller, daunted by no hardships or discomforts, ready to push her way anywhere, and as happy and contented almost in a Fijian dwelling as if at home. She has travelled over most of the world, and being a most skilful draughtswoman, has, like Miss North, brought back with her a vast series of large coloured sketches of all the principal points of interest visited by her. Whilst however Miss North's fine and most instructive collection is executed in oils, the author of the present work sketches in water colours. Miss Gordon Cumming's drawings are very beautiful and, as all those who have been fortunate enough to see them can testify, extremely faithful representations of the scenes which they depict, and she has sketched some of the most interesting scenes existing, such as the hot springs and geysers of New Zealand, the ruined ancient cities of Ceylon, the summit of Adam's peak at sunrise, with the curious coloured edged shadow then cast by the mountain, and the ever-surging lava lakes of Kilauea in Hawaii. She went to Fiji as companion to Lady Gordon on the appointment of Sir Arthur Gordon as first governor of the islands in the beginning of 1875. She stayed there more than a year and a half, seeing a great deal of the people and constantly travelling in various parts of the group. The present book is a bright and pleasant account of what she saw and did. She made a large series of sketches, and seven of these, reproduced by the autotype process, illustrate the present work. Any one who knows Fiji will at once recognise the minute accuracy with which they represent the scenery of that beautiful group, though they are, of course, but feeble substitutes for the coloured originals.

It is pleasing to learn that the Wesleyan missionaries, to whom the entire credit of the civilisation of the Fijis is due, expressed their satisfaction at the annexation when it actually took place. They certainly had very serious apprehensions as to its effect on the well-being of the native population some years before, when the matter was only under consideration. It is a pity indeed that the hoary old cannibal Thackombau was taken down to Sydney to bring back the measles to his islands and thus destroy a third of the population. The difficulties of the problem with which Sir Arthur Gordon had to deal at the outset of his governorship were greatly enhanced and complicated by the effects of this terrible mishap. It is pleasing to learn that old Thackombau is still as fond of his bible as when we saw him seven years ago; he cannot read it, apparently, but, as our authoress tells us, "it makes him feel so good." No doubt if he could read some of the battle scenes in Kings he would feel better still.

Fijians seem to be rather a failure as domestic servants.

"Day after day you must show them exactly how everything is to be done, and may be certain that each time it will be done wrong, and that the moment your back is turned they will proceed to bruise up a bit of tobacco in a banana leaf and deliberately smoke their cigarette before touching the work you have given them. Probably they will follow you to ask where the matches are, and the only answer to any remonstrance is 'malua' (by and by), a universal principle which is the bane of Fijian life. They are honest, though sometimes they cannot resist borrowing large English bath towels, which make most tempting *sulus* (kilts), and nice cambric handkerchiefs are a tempting covering for carefully dressed hair. It would be right and proper that they should use things belonging to their own chiefs, so we need not wonder that they cannot always discriminate."

The authoress was especially struck by the absence of flowers in the island, and describes this fact as all the more striking to her after a vogage direct from Australia (Sydney), where the whole country was aflame with blossom. She walked day after day till she was weary without finding as many flowers as would fill a small vase. She bears testimony, on the other hand, to the profusion of ferns. Mr. Wallace has dwelt in his work on "Island Life" on the causes to which this absence of flowers in oceanic islands is due. Miss Gordon Cumming's testimony on the matter is interesting.

Sir Arthur Gordon was punctilious in matters of native etiquette, and constantly attended kaava yangona drinkings. It is quite new to us to learn that the process of preparing kaava by chewing in Fiji was imported from Tonga, and that in the interior the old custom of grating the pepper root survives. If this statement is correct the process must have come from Tonga some time ago, for the root is only grated there now by order of the missionaries, who introduced this method in order to prevent the spread of disease occurring in consequence of the chewing. As chewing was the method adopted originally all over Polynesia, it would be strange if Fiji had been an exception in the matter.

Some remarks are made in the work on the so-called *orthodox* mode of Fijian spelling, that is to say, the strange mode of spelling which the missionaries have thought fit to adopt. According to this spelling Thackombau is spelt Cacobau, *c* being used instead of *th*, and an *n* sound, which invariably occurs before *d*, *g*, and *q*, being omitted in the spelling. There seems nothing to be gained and all to be lost by this arrangement, a mere going out of the way to create a difficulty, unless indeed it be a device to prevent the Fijians when taught to read and write Fijian from being able to read English.

Amongst the crowds of plantation labourers from all parts of Polynesia the authoress specially mentions the Tokelau islanders, "with their long straight hair, large dark eyes, and sallow faces." There is something very peculiar in the Tokelau race, and we believe there is much of importance yet to be made out with regard to it. Every visitor to Levuka cannot help being struck at once by the very marked difference in appearance between the Tokelau men and women and all other Polynesians and Melanesians taken together amongst the labour population of the place. They alone of all the islanders can be recognised in any crowd at once by any one without any fear of mistake. They have a Mongolian look, and we cannot help thinking there is something remarkable in their origin, although their language is very closely allied to Samoan.

A lively account is given of a Palolo fishery, which

the authoress was lucky enough to witness. There is a great deal in the book about cannibalism. A representation of one of the well-known so-called cannibal forks adorns the covers of the books. These forks are so much prized by visitors as curiosities that we caught a boy on the Rewa River making a couple of trade ones, and have got one of them now in our possession. It is a mistake to suppose that they were never used for eating any food except long pig (*bokola*). A young chief of one of the mountain tribes was asked whether women joined practically in the delights of cannibal feasts: he said, "I'd like to see the woman that would not eat her full share." He bore testimony to the superiority of long over short pig as food.

An interesting account is given of one of the native missionary meetings and of the set dances (*meke*) which take place at them. One of the most curious dances described is one representing a tide rising on a reef:—

"The idea to be conveyed is that of a tide gradually rising on a reef, till at length there remains only a little coral isle, round which the angry breakers rage, flinging their white foam on every side. At first the dancers form in long lines and approach silently, to represent the quiet advance of the waves. After a while the lines break up into smaller companies, which advance with outspread hands and bodies bent forward to represent rippling wavelets, the tiniest waves being represented by children. Quicker and quicker they come on, now advancing, now retreating, yet, like true waves, steadily progressing and gradually closing on every side of the imaginary islet round which they play or battle after the manner of breakers, springing high in mid-air, and flinging their arms far above their heads to represent the action of spray. As they leap and toss their heads, the soft white *masi* or native cloth (which for greater effect they wear as a turban with long streamers, and also wear round the waist, whence it floats in long scarf-like ends) trembles and flutters in the breeze. The whole effect is most artistic, and the orchestra do their part by imitating the roar of the surf on the reef—a sound which to them has been a never-ceasing lullaby from the hour of their birth."

The Fijians are, with little doubt, the best dancers in the world, and it is interesting to contrast their condition in this respect with that of ourselves, amongst whom dancing has degenerated in proportion as music has become highly developed, until it may almost be said that practically only one dance survives amongst us, and that a monotonous performance, which, by a very slight revival, is just being promoted from two to three steps. Yet Englishmen can dance when the Fijians teach them All the dancers were of course fantastically painted.

"We were chiefly puzzled and attracted by one very fine fellow, all painted black, with a huge wreath and neck garland of scarlet hybiscus and green leaves, and rattling garters made of many hanging strings of large cockle-shells, and the usual *liku* (a sort of kilt or waist drapery) of fringes of coloured pandamus leaves. Of course he carried a club, and was barefooted. This man distinguished himself greatly, and afterwards acted the part of a huge dog in a dance where all the children appeared on all fours as cats. Eventually we discovered him to be a European known as Jack Cassell."

When the short war with the Kai Volos, the till then unsubdued cannibals of the mountainous interior of Viti Levu, took place, all the chiefs sent small detachments of fighting men to the governor to help in the fight. One hundred and fifty such men came from Mbau. They marched up on to the governor's lawn armed with Tower muskets, and performed the wildest war *meke*, ending with unearthly yells. They then advanced two or three at a time, brandishing their weapons, and trying who could make the most valiant boast concerning his intended progress. One cried, "I go to the mountains, my feet shall eat the grass." This was to express his eager speed. Another, "I long to be gone, I crave to meet the foes. You need not fear; here is your safeguard." "This is only a musket," cried another, "but I carry it." Said the next, "We go to war; what hinders that we should fill all the ovens?"—a hungry cannibal ally that. One company which advanced with more stately gait, "This is Bau, that is enough."

It makes our legs tingle now a little to hear that a boy was torn and killed by one of the freshwater sharks, *Carcharias Gangeticus*, inhabiting the great Kewa River in Viti Levu during Miss Gordon Cumming's stay there, for we spent most of one night in and out of the water of the river not so long ago, pushing off our boat as she grounded constantly with the falling tide. We trusted to the sharks in the upper part of the river being only small ones, but the boy was killed at a distance of thirty miles from its mouth. The authoress had bathed in the river herself occasionally. She does not seem to be aw that the shark in question regularly inhabits the fresh

We cannot follow the authoress in her account o. . n feasts, Fijian puddings—twenty-one feet in circumference —Fijian weddings, where the bride's dress is so cumbrous that it is carried by her friends to the church and put on outside on the shore under the cocoanut trees; of the hot springs of Savu Savu, used for cooking and for getting rid of superfluous babies; of the details of the process of making the beautiful Fijian pottery, and many other matters on which we would wish to dwell.

The book loses somewhat in general effect from being retained in the form of a series of letters, an arrangement always somewhat irksome to the general reader. Perhaps also for the taste of many there is a little too much about the missionaries in the book; but as there were 900 Wesleyan chapels in the islands, and, as said before, the missionaries have brought about all the civilisation existing, they necessarily must appear a good deal in such a work. They seem sometimes to excite the admiration of their flock in a rather dangerous direction. An old ex-cannibal crept close to one who apparently is somewhat stout, "and then, as if he could not refrain, he put out his hand and stroked him down the thigh, licking his lips, and exclaiming with delight, 'Oh! but you are nice and fat.'" We always thought that Fijians, like cannibals elsewhere, had found out by experience that white men are comparatively poor eating.

We thank Miss Gordon Cumming much for her very interesting book, but before we close this notice we have one bone to pick with her. She falls into the really unpardonable popular error of talking of coral insects, and even talks of the parrot-fish extracting from the coral the insects on which it feeds. We hope she will learn before a third edition of her work appears that the animals, the skeletons of which are commonly called corals, are no more like any insects than a whale is to a blue-bottle. The fact is, coral skeletons look a little like honeycomb, and so we suppose the popular delusion will flourish for ever.

OUR BOOK SHELF

Practical Botany for Elementary Students. Introductory to the Systematic Study of Flowering Plants. By D. Houston, Science Master, South London Middle-Class School Association. (London: W. Stewart and Co., 1881.)

THIS book differs from Mr. Bettany's (vol. xxiv. p. 235) in being less general in its treatment. It is in fact a series of studies of the coarse anatomy of a number of common plants much on the plan first given in Huxley and Martin's "Elementary Biology." Each study is followed by a technical description, notes on the distinctive characters of a few allied plants, and some miscellaneous matter. "The plants selected are well-known and easily-procured types of the fifteen natural orders included in the Syllabus of the First Stage of Elementary Botany issued by the Science and Art Department, as it is believed that no better selection of natural orders, intended as introductory to the study of classification, could possibly be made." As far as can be judged without working through it, the book is well done, and will be a valuable aid to the teacher if honestly used. Mr. Bettany's plan of teaching the art of describing is soundest. His book can hardly be abused, while with Mr. Houston's there is the risk that incompetent teachers may make their pupils simply learn a large part of it by heart, on the chance of one or more of the plants being set in an examina[illegible] Occasionally, where the author abandons the sure[illegible]d of personal study, he makes slips. Thus [illegible] half the short list of exotic genera of orchids [illegible] the names misspelled. The distribution in time and space of the several orders illustrated is given, though somewhat meagrely. Perhaps in the present state of palæophytology the former is not very important. Under *Orchidaceæ*, for example, the Distribution in Time is given as "not represented," which, apart from the fact that it is a contradiction in terms, means nothing more than that fossils referable to this group of plants have not been found, and are perhaps not likely to be. It may be asked, too, what is the value of the evidence upon which the liliaceous genus *Yucca* is dated back to the Trias?

Von den Umwälzungen im Weltall. Von Rudolf Falb (Vienna, Pesth, and Leipzig: Hartleben, 1881.)

THIS work is divided into three parts, with separate headings: (1) In den Regionen der Sterne; (2) Im Reiche der Wolken; (3) In den Tiefen der Erde. The author, whose name has frequently been mentioned in our columns, has lectured in various German cities on volcanic and cosmological phenomena, and eventually went to South America in order to study the great volcanoes of the Cordilleras. He conceived an earthquake theory, and his South American friends induced him to publish it. The result is the book now before us, which was published in Spanish at Valparaiso as far back as 1877 ("Estudio sobre los templores de tierra fundado en la historia del universo"). After a sojourn in South America extending over three years, Herr Falb returned to Europe, and his earthquake theory was frequently mentioned in the press in connection with the Agram earthquakes. The theory is simple enough in itself, and the author has at least the credit of being most enthusiastic in its support and in adducing as many facts in proving the same as can possibly be found. Whether he succeeds in proving it is another question. According to Herr Falb's view, all earthquakes, or at least by far the larger majority of earthquakes, are of a volcanic origin; or, to express it concisely, "earthquakes are subterraneous eruptions." The basis of this theory is naturally the supposition that the whole interior of the earth is an ocean of incandescent matter. This is affected by the attraction of sun and moon in exactly the same manner as the sea and atmosphere are acted upon. The second division of the book therefore represents sun and moon as the generators of storms and tides; and in the third division, the principal one, we see the cause of earthquakes traced to the influence of sun and moon. There is no doubt that the author has a special gift of representing his subject clearly and popularly; his eloquence keeps the reader interested from the first line to the last. He quotes no less than thirty facts, from which he draws thirteen different inferences in proof of the volcanic nature of earthquakes. We regret that space does not permit us to enter further into details, but we can heartily recommend the book to our readers.

The Quantitative Estimation of Phosphoric Acid. By M. H. Joulie. Translated by J. Barker Smith. (Dulwich: Published by the Author, 1881.)

THE laudation of M. George Ville, with which the translator opens his preface, discouraged us at first from further perusal of this pamphlet on the citro-uranic method of determining phosphoric acid in manures; and when we did peruse these sixty pages, our chief impression was derived from the comical literality with which French idioms had been rendered into English words. But after all a good many useful hints may be gathered by practical analysts from this little book. Of course most agricultural chemists are familiar with the difficulties which beset the fair sampling and preparation of manures for analysis, and they are also acquainted with many special contrivances for overcoming these difficulties. But information as to new and improved methods of operating, and as to modifications of old processes, is always acceptable.

The essence of M. Joulie's method consists in the precipitation of the phosphoric acid in a prepared solution of a manure by means of a solution containing citrates of ammonium and magnesium. The precipitate which forms is thus produced in the presence of the lime as well as of the iron and alumina of the original liquid; we should like further proof that the whole of the phosphoric acid is invariably precipitated under the conditions described by M. Joulie, especially as he directs the solutions, if rich, to be kept no more than two hours before the ammonio-magnesian phosphate is filtered off. The second and final stage in M. Joulie's method is the solution of the precipitated phosphate and its titration by a standard solution of uranium nitrate.

The second part of this pamphlet describes the treatment of manurial phosphates with solutions of ammonium oxalate and ammonium citrate in order to determine their "relative assimilability." We are not aware that M. Joulie was the first to employ these reagents in the analysis of phosphates—his announcement of the use of the oxalate being in 1872, and of the citrate during the next year. Anyhow, we must demur to some of the conclusions which M. Joulie draws from his experiments, nor can we accept as satisfactory the final directions for the "assay of superphosphates" with which the last fifteen pages of his manual are occupied. The determination of the phosphoric acid and phosphate dissolved by distilled, or, if you will, carbonated water, from a superphosphate cannot be safely replaced by a determination of the phosphates soluble in ammonium citrate. For we lack proof that retrograded phosphates are equal in value with monocalcic phosphate, which alone possesses an initial diffusive power when it is introduced into the soil.

The Butterflies of Europe. Illustrated and described by Henry Charles Lang, M.D., F.L.S. Part I. (L. Reeve and Co., 1881.)

WE have received Part I. of this work, the approaching publication of which was announced in these columns a few weeks back. The whole of the species (and some prominent varieties, &c.) inhabiting Europe proper will occupy about twenty monthly parts, each containing sixteen pages of text and four coloured plates. The plates

are chromolithographed from the author's own drawings, which appear to be exceedingly well done. We defer a more extended notice until more parts shall have appeared, especially because the subjects illustrated in the first part are almost the least difficult for the chromolithographic process. The text is clearly printed, but a little more care in writing the short descriptions should be exercised. Thus at the very commencement we read as one of the characters of the family *Papilionidæ*, "Larva cylindrical, not spiny, furnished with two retractile tentacles on the second segment." We doubt if this is correct for *all* the European species of *Papilio;* it certainly is not so if exotic species of the same genus are considered; and almost immediately afterwards the author, in defining the genus *Thais* (one of the *Papilionidæ*), says, "Larvæ armed with spines." Nowhere do we find any reference to the veining of the wings, which certainly should have formed part of the sketch of the principal groups given in the Introduction. The author will do well to consider the importance of this suggestion. We presume the chief object of the work is to enable collectors of European butterflies to name their captures, and especially by means of the figures. For this purpose it promises to be exceedingly well adapted.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Chemical Equivalents

MR. J. P. O'REILLY'S paper in your last number (p. 274) appears to involve a complete misconception of the theory of chemical equivalents. The equivalents are mere ratios, and are not altered by multiplying their representative numbers throughout by any factor, whether π or any other.

In fact we may write the equivalents of hydrogen, carbon, and oxygen as x, $12x$, and $16x$, without troubling ourselves about the value of x. This is not only the theoretical view, but the one actually used in practice. So far, there is nothing new or special about writing, as Mr. O'Reilly does, $x = \frac{1}{3}\pi$.

But then Mr. O'Reilly goes wrong, and gets results which contradict his hypothesis. When he writes $H = \frac{1}{3}\pi$, and $O = 5\pi$, the real inference is that the equivalents of H and O are as 1 : 15, instead of as 1 : 16 which he started from.

If his π-values are to be taken as corresponding with the equivalents, this simply means that the latter are not to be depended upon within a limit of error of 5 per cent. I think the mistake is not in this respect, but in overlooking the circumstance that the chemical equivalents are not absolute values, but ratios.

July 24 C. W. M.

Slow Lightning

HAVING just seen the statement of Prof. Tait (NATURE, vol. xxii. p. 341) quoted, as a final authority, against the possibility of distinguishing the source from the termination of a lightning flash, I wish to record a storm that I saw. On May 19 there had been a brisk, hot south-west wind blowing at Gizeh, off the Libyan Desert, at about or over 100° F.; at near sunset a north wind began to come up against it, and there was heavy thunder and lightning all along the line of the mingling of the winds, extending as far as I could see to east and west, and passing a few miles to the north of the Pyramids: the lightning was solely between the clouds, at a height of about one and a half miles; the air around me was 94°, though almost dark. I sat on a rock in front of the door of my tomb (from which I could see eighteen miles over the Delta) and quietly watched the lightning. To my sight there were distinctly differences in the duration of the flashes: some appearing instantaneous and others in which I could see a spot of light occupying an appreciable interval to travel from one cloud to another; and I should be puzzled to draw a hard and fast line between the classes. Does this moving spot-lightning merge insensibly into the variation, of which I saw a fine case years ago near Guildford, where a spark would slowly sail down in the air and then move over the ground before it disappeared?

In any case can these slow flashes (lasting perhaps half a second), seen as well as instantaneous flashes, be disposed of by that blessed word *subjectivity*, which is so comforting to theorists on many objects? Or may not the confession of our ignorance of the cause of ball-lightning be extended to slow flashes in general, instead of treating them just as meteorites were put out of court a century ago? W. M. FLINDERS PETRIE

Bromley, Kent

[Several instances are recorded by Faraday, Joule, and others of flashes which seemed to last for a sensible time. But they are easily explained by one or other of two *veræ causæ*, viz. (1) oscillatory discharges along the same path, succeeding one another at smaller intervals than one seventh of a second; or (2) phosphorescent matter in the track of the flash. More definite particulars would be necessary before one could decide which was active in the present case.—ED.]

Thought-Reading

As having a bearing upon the hypothesis that in "thought-reading" the information is transmitted by unconscious muscular exertion, allow me to state a modified form of the experiment I tried in the presence of two or three others with Mr. J. R. Brown, who, a few years ago, attracted considerable attention in various parts of the United States by doing precisely what is related of Mr. Bishop in your issue of June 23 (p. 171). After witnessing experiments of the same kind as those stated by Mr. Romanes and performed under the same conditions, I thought to vary them by using a flexible copper wire as a connecting medium. Selecting one, two or three yards long, I held one end in my hand, while Mr. Brown, winding the other end once or twice around his fingers, held it against his forehead, the wire being all the time kept slack between us. Here evidently there could have been no indications received through muscular movements. Yet in this way Mr. Brown would find things concealed or go to certain points determined upon, though apparently with not quite the same readiness and confidence as when the subject's hand was placed against his forehead. Once he partially failed, selecting, instead of a particular spot on the wall I had fixed my mind upon, a small object near it. The experiment in this form was tried with another as his subject, and with equal, if not better, success. GEO. B. MERRIMAN

Rutger's College, New Jersey, July 11

Optical Phenomena

THE photographic halo phenomena described in NATURE, vol. xxiv. p. 260, seem analogous to some observed by me, and upon which, in the spring, I read a paper (since published in the *Notices*, vol. xli. No. 6) before the Royal Astronomical Society. In this I described that not only the sun's disk and the moon's full and partial phases, but also apertures (of similar shape to these) in the shutter of a dark room, when photographed, were, one and all, surrounded by a strong ring halo not visible to the eye. A correspondent essayed some time since to prove in your journal that this halo only surrounded the moon when at full, but on trial the question proved one of time of exposure; and it now seems pretty clear that whatever may be its form and nature, a very bright object when photographed (especially in relief against a dark ground) is found, if sufficient exposure be given, surrounded on the plate by a halo separated from the object by a dark space. Mr. Cowper Ranyard and others attribute these halos to reflection from the back of the plate, a point on which I have not experimented. The dark spot mentioned in connection with the aperture in the rock is probably a reversal of the brightest light owing to the length of exposure.

In two seconds, with Wratten and Wainwright's instantaneous plates, I have found the sun's image so reversed in a camera landscape, showing as a white spot on the negative and a black dot when printed. J. RAND CAPRON

Guildown, July 23

Symbolic Logic

MR. MCCOLL still expresses surprise at my declining to answer a Yes or No question which he was pleased to put to me in

NATURE (vol. xxiv. p. 124). It was, I should think, almost unique in a scientific journal. It turned upon a contradiction which he had detected between a statement which I never made and the fact that he surely entertained an impression that he had somewhere or other (he did not say where) seen me quoted as holding an opinion at variance with that statement. I did not think that time would be well employed in answering it. Even now he talks of his "quotation or misquotation"; a convenient but unusual latitude of expression for a serious case of the latter of these alternatives. Really I am not in the examination-room, and will not therefore attempt to compress into a few paragraphs the answers I should give upon intricate philosophical points, even were the questions reasonably framed. Moreover I must remind Mr. McColl that he does not profess to write as an impartial inquirer or critic, but asked for the insertion of his letter upon the ground that he conceived himself to have been attacked. I have fully explained the only point upon which he had any claim to call for an answer, and therefore now close the correspondence. J. VENN

Achensee, Tirol

Jupiter

THE great red spot on the planet seems unchanged as I saw it on July 8, while the north temperate zone belt (Gledhill's No. 2) shows a development nearly as striking as the equatorial belts.

Millbrook, Tuam, July 20 J. BIRMINGHAM

New Red Variable

THIS star, which I found on May 22 only 9 magnitude (see NATURE, vol. xxiv. p. 164), progressively increased up to 8 m. on June 6, and is now again no more than 9. Its deep crimson colour is unchanged. It is 2° 51'·7 north of α Cygni, and forms, with three other stars, the southern end of a little inverted and irregular cross. It will probably decrease to complete disappearance, at least from telescopes of moderate power.

Millbrook, Tuam, July 20 J. BIRMINGHAM

A Fireball

ON Wednesday the 20th ult., about midnight, a house at Mont Dore, in the Auvergne, was destroyed by a fireball during a severe thunderstorm. My brother, who has lately arrived from thence, did not see the ball himself, but his valet, an intelligent Italian, saw it distinctly. He describes it as a globe of fire about half a metre in diameter, which approached the house obliquely, seeming to pass over a distance of 200 metres in about half a minute. It entered the door of the house and there burst. My brother heard the explosion as well as his valet, and describes it as a dull thud like that of a smothered blast. The house, which was a wooden one, was set on fire, a child burnt to death, and another inmate seriously, if not mortally, injured. Several inhabitants of Mont Dore are said by the valet to have seen the ball, one of whom lived in the adjacent house.

It will be interesting to readers of NATURE to compare the accounts given by Prof. Tait in NATURE, vol. xxii. p. 409.

19, The Boltons, S.W. JOHN TENNANT

Meteor

ON July 23, at 7.15 p.m. (Irish railway time) a meteor passed, travelling nearly from south to north, being lost in a bank of black cloud. It must have been of considerable brilliancy, as it was quite distinct, although at the time the sun was well above the horizon. July 22 from ten to fourteen brilliant red pencils and thin columns of auroric lights were rising at intervals. There were also auroric lights on July 23 between 11 and 12, but much less brilliant. G. H. KINAHAN

Ovoca, July 24

THE COMET

WE have received the following further communications on this subject:—

The following "Preliminary Note on the Photographic Spectrum of Comet *b* 1881," has been communicated to the Royal Society by Dr. Huggins, F.R.S. (For Dr. Huggins's first note on this subject, see NATURE of June 30.)

ON the evening of June 24, I directed the reflector furnished with the spectroscopic and photographic arrangements described in my paper "On the Photographic Spectra of Stars" (*Phil. Trans.*, 1880, p. 669) to the head of the comet, so that the nucleus should be upon one half of the slit. After one hour's exposure the open half of the slit was closed, the shutter withdrawn from the other half, and the instrument then directed to Arcturus for fifteen minutes.

After development, the plate presented a very distinct spectrum of the comet, together with the spectrum of the star, which I have already described in the paper referred to above.

The spectrum of the comet consists of a pair of bright lines in the ultra-violet region, and a continuous spectrum which can be traced from about F to some distance beyond H.

The bright lines, a little distance beyond H, with an approximate wave-length from 3870 to 3890, appear to belong to the spectrum of carbon (in some form, possibly in combination with hydrogen), which I observed in the spectra of the telescopic comets of 1866 and 1868.

In the continuous spectrum shown in the photograph, the dark lines of Fraunhofer can be seen.

This photographic evidence supports the results of my previous observations in the visible spectra of some telescopic comets. Part of the light from comets is reflected solar light, and another part is light of their own. The spectrum of this light shows the presence in the comet of carbon, possibly in combination with hydrogen.

On the next night, June 25, a second photograph was obtained with an exposure of an hour and a half. This photograph, notwithstanding the longer exposure, is fainter, but shows distinctly the two bright lines and the continuous spectrum, which is too faint to allow the Fraunhofer lines to be seen.

Postscript, July 9, 1881.—I have since measured the photographs of the comet's spectrum, and I find for the two strong bright lines the wave-lengths 3883 and 3870. The less refrangible line is much stronger, and a faint luminosity can be traced from it to a little beyond the second line 3870. There can be, therefore, no doubt that these lines represent the brightest end of the ultra-violet group which appears under certain circumstances in the spectra of the compounds of carbon. Professors Liveing and Dewar have found for the strong line at the beginning of this group the wave-length 3882·7, and for the second line 3870·5.

I am also able to see upon the continuous solar spectrum, a distinct impression of the group of lines between G and *h*, which is usually associated with the group described above. My measures for the less refrangible end of this group give a wave-length of 4230, which agrees as well as can be expected with Professors Liveing and Dewar's measure 4220.

In their paper "On the Spectra of the Compounds of Carbon" (*Proc.* Roy. Soc., vol. xxx. p. 494), Professors Liveing and Dewar show that these two groups indicate the presence of cyanogen, and are not to be seen in the absence of nitrogen. If this be the case, the photograph gives undoubted evidence of the presence of nitrogen in the comet, in addition to the carbon and hydrogen shown to be there by the bright groups in the visible part of the spectrum. On this hypothesis we must further suppose a high temperature in the comet unless the cyanogen is present ready formed.

I should state that Mr. Lockyer regards the two groups in the photograph, and the groups in the visible spectrum, to be due to the vapour of carbon at different heat-levels (*Proc.* Roy. Soc., vol. xxx. p. 461).

It is of importance to mention the strong intensity in the photograph of the lines 3883 and 3870, as compared with the continuous spectrum, and the faint bright group beginning at 4230. At this part of the spectrum, there-

fore, the light emitted by the cometary matter exceeded by many times the reflected solar light. I reserve for the present the theoretical suggestions which arise from the new information which the photographs have given us.

THE second evening of its appearing I examined the head of this comet with a McClean spectroscope (with slit) and also with a Hilger's half-prism instrument (a half-size model of the Greenwich one).

The appearances were mainly those seen by other observers, viz., a bright continuous spectrum from the nucleus and a much fainter one crossed by bright lines from the coma. There were however two points of interest which struck me, as I see by NATURE, vol. xxiv. p. 261, they did M. Thollon in Paris. These were: (*a*) The continuous spectrum from the nucleus had a mottled or striated look, but I could not be certain whether dark lines or bright lines or spaces predominated in causing this effect; (*b*) the presence of shorter and additional lines to the three carbon ones, extending beyond the continuous spectrum.

These appearances, I admit, I only recognised indistinctly and with doubt at the time, but, corroborated as they now seem to be, I do not question that there was some ground for them. With reference to the nucleus spectrum it could only have comprised a small portion of solar light as shown by the few Fraunhofer lines detected by Dr. Huggins and others in it. The residue of the bright stripe has been attributed (because continuous) to some incandescent solid or liquid substance; but is this necessarily the case? Is it not possible that the matter yielding this spectrum is still in a truly gaseous form, and do not the appearances above described rather point to the character of a gas spectrum passing from the line or band condition to the continuous one, under its existing circumstances of ignition, pressure, &c. (whatever these may be)?—an effect not without parallel, I fancy, at least in the case of hydrogen. J. RAND CAPRON

Guildown, July 23

A COMET is now visible here. I saw it last Thursday, June 30, at 3·10 a.m. It was in the west, and appeared to me about 30° from the pole star, and 20° above the horizon. The tail was straight and directed towards the pole star. A local paper says this comet was seen to the east at 8 p.m. the preceding day, and that the tail was 20° in length—it appeared to me only 5°.

I regret I cannot send fuller information, but probably the comet is to be better seen in England.

Karáchi, July 2 F. C. CONSTABLE

SEA-SHORE ALLUVION

IT is somewhat remarkable at the present day to find even professional men, when dealing with works of coast defence, attributing the movement of littoral shingle to the tidal currents.

The late Mr. Palmer, C.E., in a well-known paper read before the Royal Society nearly half a century back, Col. Reid, R.E., in an essay published in the commencement of the series of quarterly papers by officers in the corps of Royal Engineers, Mr. Redman, M.Inst.C.E., in a paper on the South Coast of England, read before that society some thirty years back, and another on the East Coast of England seventeen years back, as well as in very numerous reports made by him for a Government department (the War Office) during the last quarter of a century, have all shown that these shingle formations are in no way affected by the tide, which must exercise only a negative influence, the flood and ebb setting in contrary and opposite directions, equal in duration, and neutralising each other. Shingle moles are in effect resultant on the wind waves alone, and are deposited in two parallel ridges or hummocks locally termed "*fulls*," marking the relative range of neap and spring tides, the crest of the last being normally (except in some exceptional cases such as the *Chesil*) ten feet above high water of spring tides with a broad, gently sloping foreshore of sand down to low water; an abnormal tide, resultant on exceptional gales occurring at rare intervals, sometimes breaches the crest and produces great mischief, as at Seaford on the Sussex coast a few years back, which was inundated by the sea, and where the authorities are about to carry out artificial works of defence.

The prevailing movement in the English Channel is to the eastward, or up Channel, due to the fact that south-west winds prevail for nine months in the year; and along the East Coast the movement is southward, due to the particular trend of the coast and the North Sea offing. It really hardly appears necessary to insist on these well-known facts to any one practically acquainted with the subject, or to hydraulic engineers conversant with the surrounding physical conditions of our tidal harbours, estuaries, and rivers.

Notwithstanding this, strangely enough we find a contemporary journal, the *Engineer*, in a series of articles on the Brighton, Hove, and Shoreham beaches, professedly written for the education of public opinion on the subject, themselves ignoring the fundamental laws governing the motion of this marine alluvion, and attributing it to tidal currents instead of to the wind waves, and yet insisting at the same time that the question, as doubtless it is, is an imperial one, demanding the attention of the Legislature.

Thus, October 3, 1879, "Brighton Beach" (*The Engineer*):—

"A very strong *tidal current* sets up the Channel to the eastward, and sweeps with it the rolling shingle"! (*sic.*)

"So rapidly did this disappear under the influence of this *current* that it became necessary to stay its travel by the erection of heavy timber groynes."

"Knowing what we do also of the effect of *sea currents*, it is in our opinion exceedingly questionable *if their carrying powers* can be arrested by anything short of a check which shall produce almost dead water."

One of the last papers read at the Institution of Civil Engineers, on "Upland and Tidal Scour," also attributes the movement of the Norfolk and Essex beaches to the tidal currents.

Nor are local authorities, highway boards, vestries, district boards, and large landowners any more at one than these would-be educators of public opinion on the subject, for we find farmers as a rule sending down their teams and waggons to the sea-shore during winter slack time to collect boulders and pebbles from the sea moles of Nature's forming; railway companies where allowed, and a convenient communication effected, removing it wholesale for ballast of the iron road; lords of the manor conveying it equally wholesale to shipping craft for ballast, until stopped by the strong arm of the law brought to bear on the question by some Government department.

Local magistrates are equally offenders, as recently, about twelve months back, the magistrates sitting at Canterbury authorised their surveyor, after long discussion, as the order was given with the fear of an impending injunction hanging, "Damocles" like, over their heads, to quarry shingle from the sea-shore at Herne Bay for the repairs of the highways; thus robbing the supply travelling up the estuary of the Thames to the westward (the general movement of the belt of shingle being diverted up such estuaries as those of the Thames, Wash, &c.), the material being at the same time so much wanted along the Blue Town frontage at Sheerness, where grave fears have long prevailed, due to the insufficiency of the sea-shore works of defence. This Canterbury decision, taken in the month of January in last year, appeared to us at the time the extreme of rashness, when the interests to

leeward of the proposed road quarry at Herne Bay were considered. The Trinity Corporation would be affected in reference to the defences of the Reculvers—the two spires of the ancient church having been maintained by them for many years as sea marks with stone slopes and groynes for the protection of the cliff. From the fact of their being to the eastward of Herne Bay they may be said to be to windward of the site, still the hastening of the recession at Herne Bay, which has from natural causes alone increased in a marked degree of late, would tend to increase the projection and consequent exposure at the Reculvers.

Next we have the entire landowning interest of the Isle of Sheppy affected by this Canterbury decision, for its northern seaboard retreats at a rapid rate, evidenced by the recent removal of Warden Church, which had been left on the extreme verge of the cliff, due to the extensive slips in the London clay to the westward, which must of course be aggravated if the natural barrier formed by the sea at the base of the cliff is weakened by cutting off the supply coming from the eastward, tending always in its normal state to travel onwards to increase Garrison Point at Sheerness at the outfall of the Medway.

Lastly we have the whole Mile Town and Sheerness frontages affected, where the Government have erected from time to time sea-walls and groynes for the collection of this very beach that the Canterbury magistrates covet for the repairs of their roads. Nor is Sheerness alone affected, but the Queenborough district also, as was evidenced in the great tide of February, 1791, when the whole of the marsh forming the north-west promontory of the Isle of Sheppy was under water, and great loss and damage sustained.

Canvey Island, on the opposite Essex shore, suffered in a similar manner at the same time.

For some years past this practice of removing littoral gravel has been stopped on the Kentish southern coast since Mr. Redman reported for the War Office on the condition of the beaches at Sandown, Deal, Walmer, Dover, Eastbourne, &c., who strongly urged the suicidal nature of the practice, since which the Government and local authorities have had notice-boards planted along the beach imperatively forbidding the removal of shingle.

This general *leeward* movement of shore detritus, due to the prevailing wind waves, has been of late years so clearly demonstrated by the authorities cited, and accepted generally by marine engineers, that it appears strange to find editorial articles for months in a magazine of wide circulation dealing with engineering science which resuscitate the old and exploded theories on the question which are to be found in early geological works, and these articles, if not accepted, have at least remained hitherto apparently unchallenged.

We would sum up this perhaps somewhat lengthy review of a topic, of no mean importance however, having reference to our insular position, by saying that the passage of the heavier particles (the shingle) of a marine mound or natural mole is due universally to the action of the waves, although attributed by many early geological writers to the ocean currents—and its influence on the tidal harbours of our shores, is very important.

The masses of shingle are heaped up coincident in direction with the waves which sort the material in regular gradation; an alternate renewal and withdrawal, due to change of wind, produces a resultant leeward motion due to the wind the particular coast is most exposed to, and the largest pebbles in all these marine alluvion are universally accumulated on the summit, and to leeward of the prevailing winds, due to their greater momentum and to their being less influenced by the recoil wave, compared with sand and the smaller stones.

At the last meeting of the South-Eastern Railway Company we find the chairman (Sir E. W. Watkin, M.P.) stating in reference to the proposed Lydd Railway and the line to Dungeness, that it not only would secure in the future the shortest route to the Continent, but that it also gave them access to an important bed of shingle, from the sale of which they anticipated great benefit! and that they saw their way to *do a large trade* in its conveyance. It was important for road-making, railway-ballasting, and concrete foundations and walls.

This is the not over scrupulous view of the chairman of a leading railway company of one of the most important natural breakwaters on the south-eastern coast, and the uses to which it may be applied as a quarry for the benefit of his company.

The great land-slip which left Warden Church on the verge of the cliff, causing its ultimate recent removal, occurred in September, 1859, and this had been preceded by a similar great fall to the westward about the year 1856, that of 1859 being in effect a prolongation or extension of the earlier one towards the eastern end of the island. The falls are the result of a gradual subsidence occupying some hours, due to the thorough saturation of the London clay by land drainage down several small chines, and the effect of atmosphere and weather on the face of the cliffs and their degradation at the base by the sea during spring-tides. In effect a broad belt of land moves seaward (not a mere abrasion or undermining of the cliff alone), settles vertically downwards, or spreads out, and slides seawards, presenting a new cliff landward at the last parallel fissure, the moving mass attaining a state of rest in the shape of an under-cliff, with a series of parallel terraces rising and falling in the valley of the fall, with the turf and vegetation undisturbed; and the foreshore and shingle are ploughed up by the fall, forming a kind of "moraine" at the base. After a fall the ordinary waste goes on at an average rate of one yard per annum. Some of the trees near the church had settled down bodily on the prisms of earth to which they were attached, some fifty feet lower in level than when they were *in situ*, showing how gradual and vertical had been the subsidence.

The question arises, To what extent are the cliffs in the Tertiary formations saturated or affected by percolation through fissures from the sea, and how far this may be the first cause? There are no appearances of land springs from the cliff face. The whole appears to have squeezed down into a saturated or partly fluid base. The rapid degradation of the Sheppy cliffs was pointed out in an article in the *St. James's Gazette* of May 23, and the absence of any attempts to arrest it. But this constan loss has been eloquently described by Lyell in his great work, "The Principles of Geology," affording as it does a constant supply to the fluctuating foreshores of the River Thames carried up by the superior power of the flood compared with its ebb tide, and brought down again by the prolonged duration of the ebb, aided by upland waters in steps downwards.

SCIENCE AT ETON

ALTHOUGH Eton still ranks as a purely classical school, and has not established a modern side as her rival Harrow has done, yet the study of science is pursued within her walls to an extent which—in some respects at least—is unequalled at any other school. The numbers of the school vary somewhat on each side of 900 boys, about 120 of whom, constituting the Fourth Form, do no science. About seventy boys more from the Fifth Form make up the Army Class, and do no Science unless they take in Physical Geography and Geology for their final examination. But in the Remove and in the greater part of the Fifth Form, which constitutes the chief mass of the school, two lessons a week in science enter into the regular work of each division.

At the present time the Head-Master has twenty-two Classical Assistants, and the Lower Master two. There are nine Mathematical Masters, and four for science, two

of whom also give a little help in the mathematical, and one in the literary teaching. On emerging from the Fourth Form a boy spends a year in Remove, during which his two weekly science lessons are devoted to Physical Geography (*Erdkunde*). Passing on into the Lower Division of the Fifth Form, he is taught the elements of Mechanics, treated experimentally; and during the next year, spent in "Middle Division," he is occupied with Heat, including the principles of the steam-engine. Except in the case of the Army Class, therefore, science is compulsory during three years of every boy's school life. When he enters the Upper Division of the Fifth Form he may drop science altogether, and devote the two lessons a week to additional mathematics. If he does not do this he has his choice between Chemistry, Geology, Physical Geography, and Biology. Many boys leave the school without reaching any higher stage than this; but those who pass on into the "First Hundred" can either give up science in favour of mathematics or of some literary subject, or, on the other hand, they may take up a second scientific subject, and get an additional two lessons a week. During the present School-time they can select from Chemistry (Metals), Physics (Light), Geology (Elementary and Advanced), Physiography, Botany, and Physiology (Muscle and Nerve). Hence a boy who enters Remove at thirteen and stays at Eton until he is nineteen, has the opportunity of acquiring a considerable amount of elementary scientific knowledge. He hears two lectures a week and writes out an abstract of them which is looked over and corrected. His progress is tested by written examinations, the frequency of which depends upon the master he is "up to." Oral

— VIEW — OF — SOUTH — FRON.

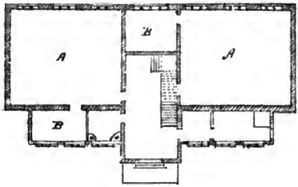
examination is rarely possible except in the upper parts of the school, owing to the necessarily large size of the classes.

At the examinations for promotion which occur every Half, though they are annual as regards individual boys, science is allotted from one-tenth to one-twelfth of the total marks. Although it is not a "pluck subject" like mathematics, yet many a boy who has failed by a few marks in his "general total" regrets not having paid more attention to his science lectures, which would have enabled him to make up the deficit; and the effect is often visible in the improvement in his abstracts during the next School-time.

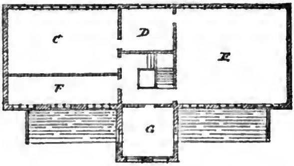
Besides the compulsory science, which is a part of the regular school-work, many boys devote three or four hours of their spare time every week to some form of practical scientific work. Most of them are preparing themselves to try for the prizes given annually by the Governing Body for Chemistry and Physics, Geology and Physical Geography, and Biology respectively. A chemical laboratory, with accommodation for twenty-eight boys at once, has been in use for some years. It owes its existence in great part to the liberality of the present Head-Master, Dr. Hornby, some of the Assistant Masters, and some old Etonians. The laboratory and chemical lecture-room, together with a private laboratory, apparatus-room, store-rooms, &c., form a handsome building in Keate's Lane. Opposite to it is the Round School, formerly used for mathematical teaching, and now converted into a museum. It contains a fine collection of British birds made by the late Provost of King's College, Cambridge, Dr. Thackeray, and presented by him to the school. Some of the specimens are valuable, owing to their being mentioned by Yarrell. There is also a good collection of Lepidoptera made by a former pupil of the school, and presented by his parents after his untimely

death, within two years of his leaving Eton. A tolerably complete series of recent shells has been obtained, and a collection of the more important zoological types is in process of formation. Geology is represented by a fair series of British rocks and fossils, and by remains of the mammoth, hippopotamus, reindeer, and *Bos primigenius*, from the river gravels of the neighbourhood, together with flint implements and neolithic axe-heads which have been dredged out of the river. There are also a few cases of specimens illustrative of volcanic and glacial action, and of the more important processes of Metallurgy, pottery, &c.

Beyond the laboratory, and separated from it by the racquet-courts, are the new Science Schools, which are



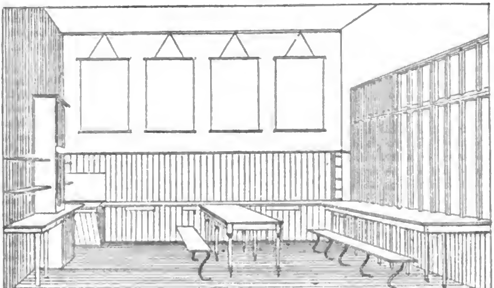
— GROUND-PLAN —



— FIRST-FLOOR-PLAN —

— SCALE — OF — FEET. —

being used this Half for the first time, three out of the four Science Masters having been hitherto accommodated in some discarded mathematical schoolrooms. On the ground floor there are two schoolrooms (A, A), each accommodating forty boys, and two preparation-rooms (B, B) opening into them, besides furnace-room, lavatory, &c. On the first floor are the biological laboratory (C), preparation-room (D), and schoolroom (E), all *en suite*, together with a general apparatus-room (F) and stores room (G). The laboratory, which is 28 feet by 16 feet has accommodation for seventeen boys at once, the table, running along the length of the room, so that the workers shall face the window. Except for the necessary supports, this is continuous along almost the whole of one side of



— GENERAL — VIEW — OF — BIOLOGICAL — LABORATORY —

the room. The other side is occupied by shelves and a large cupboard, half of which is divided up into compartments, one for each boy. A sink, a bookshelf, and a large slate occupy three of the corners, the door being at the fourth. The rooms are all panelled with stained deal to a height of about 5 feet from the floor, which leaves ample space for diagrams above.

The building has been designed and erected by Mr. E. Aborn of Eton, at the cost of the Governing Body of the College, of which Prof. Huxley became a member rather more than a year ago as the representative of the Royal Society. At the request of his colleagues he provided Mr. Aborn with a statement of what he thought was required for the proper accommodation of the three houseless Science Masters, and the result is the eminently practical but inexpensive building, with which all who

use it, both boys and masters, are alike pleased. The schoolrooms are the best in the College as regards light and accommodation for teachers and taught, and offer a striking contrast to the dark and uncomfortable rooms which were built a few years ago for the Mathematical Masters.

Besides providing a home for the science-teaching, the Governing Body have also placed a grant of money at the disposal of the teachers for the purchase of microscopes, diagrams, maps, &c., so that a boy who is really anxious to learn has plenty of opportunities for so doing. The number is not very large at present, as so many other interests have to be considered, such as the classical work and the games. Those, however, who are really taking up science with a view to University honours, find their tutors less exacting about their classical studies; and that the time spent in the laboratory need not interfere with athletics is shown by the fact that the two boys who last year divided the Governing Body's prize for Biology are both distinguished football players. One of them was a prominent member of the school football eleven during the past winter, and has this year rowed for the fourth time in the Eton crew at Henley Regatta. His numerous official duties as "captain of the boats" during the last two years have not prevented his acquiring sufficient knowledge to gain him an entrance exhibition for Natural Science at Oxford, an honour which has only once before been gained by an Etonian.

In addition to the Chemical and Biological Laboratories Eton possesses a well-organised School of Practical Mechanics, consisting of a drawing-room, smithy, and shops for carpentry and metal-working, the whole being under the superintendence of a specially-qualified instructor. Much work is done here out of school-hours, oars, bookcases, cupboards, lathes, and a small steam-engine being some of the practical results of this institution, which is now completing the second year of its existence.

NOTES

THE death, on Sunday, is announced of Prof. Bruhns, the Director of Leipzig Observatory.

THE greatest activity prevails in the Palais de l'Industrie, preparing for the Exhibition of Electricity, but it is feared that, in spite of all efforts, the day of the formal opening will have to be postponed.

THE Municipal Council of Paris has finally granted to Siemens Brothers the authorisation to place rails in the streets for their electrical railway from the Place de la Concorde to the Exhibition. But no viaduct will be constructed, as formerly contemplated, and the rails will be merely laid flat on the pavement.

MR. MUNDELLA stated in the House of Commons the other day that Mr. Samuelson, M.P. for Banbury, and Mr. Slagg, M.P. for Manchester, had consented to investigate the whole question of technical education on the Continent at their own expense. Mr. Mundella was in communication with two other gentlemen, who, he hoped, would join the two mentioned.

A PUBLIC meeting will be held on August 8 at the Society of Arts for the purpose of re-organising the Trades Guild of Learning on a larger basis. Among the supporters of the movement is Sir John Lubbock.

A MEETING was held on Tuesday afternoon at Grosvenor House, Park Lane, to receive and consider a report from the Smoke Abatement Committee on the subject of an exhibition and trials of improved heating and smoke-preventing appliances, to be opened at South Kensington in October next. There was a numerous attendance. The exhibition in question will be held in the East and West Arcades and in buildings adjoining the Royal Albert Hall; and gold, silver, and bronze medals, together with certificates of merit, will be awarded upon the report of a special committee. It will be opened on October 24 and closed on November 26. Dr. Siemens moved the first resolution, declaring that the present smoky condition of the atmosphere of London injuriously affects the health and happiness of the community, besides destroying public buildings, deteriorating perishable fabrics, and entailing in various ways unnecessary expenditure. The speech of Dr. Siemens was a powerful argument in support of the principles thus enunciated, and was warmly cheered. Sir Henry Thompson, in seconding the motion, pointed out that the admixture of unburnt fuel in the shape of smoke with the atmosphere must of necessity be prejudicial to those who had the misfortune to breathe it. He also showed by illustration how highly desirable it was on all grounds of health that the air should be perfectly free from smoke. Dr. Quain, Mr. Spencer Wells, and Mr. G. J. Romanes supported the proposition, which was carried unanimously. Other resolutions were carried, and it was resolved by the meeting to support the efforts now being made to reduce the evil arising from coal-smoke, and to assist in raising the funds necessary for constructing and carrying out, on a practical scale, the competitive testing of the various appliances to be shown, and for providing suitable prizes, medals, &c.

THE new building of the "Deutsche Seewarte" at Hamburg will be inaugurated on September 14. The Emperor William will be present, and will at the same time open a Marine Exhibition connected with the building.

THE annual meeting of the British Medical Association has been delayed a week beyond its usual time in consequence of the Medical Congress meeting in London. It will extend from the 9th till the 12th of August, and is to be held at Ryde, Isle of Wight. The address on medicine will be delivered by Dr. J. S. Bristowe of St. Thomas's Hospital, that on surgery by Mr. Jonathan Hutchinson of the London Hospital, and that on obstetric medicine by Dr. J. Sinclair Coghill, visiting physician to the National Hospital for Consumption at Ventnor.

THE Harvard Geological Museum has lately received from the Smithsonian Institution a series of the types of the fossil plants described by Prof. Lesquereux in connection with his various Government publications. Prof. Lesquereux is also engaged in identifying a large collection of fossil plants made by Mr. Sternberg for the museum. The collection contains nearly all the species thus far described from the Cretaceous beds of Western Kansas.

AN exhibition and congress in connection with the culture of vines is appointed to take place in Milan in September next. The congress, which will be composed of experts from various countries, will be charged with the duty of inquiring into the subject of the disease which has of late years rendered the wine crop so uncertain. The exhibition will consist mainly of appliances for the application to vegetation of insect-destroying agencies, of machines and instruments for grafting, of photographs, plates, and printed publications relative to the vine disease, specimens of vine parasites, and numerous other objects serving similar purposes.

ACCORDING to the Mineral Statistics of Victoria for 1880 the quantity of gold raised in the Colony for the past year was 829,121 oz. 5 dwt., being upwards of 70,173 oz. more than in 1879. While there has been a decided increase in the quantity raised in alluvial mines, the main increase has been in the quartz mines, which have yielded 529,195 oz. The increase in alluvial mines is partly due to the opening-up of new gold-producing areas, and partly to the operations of the diamond drills recently imported.

AN interesting discovery has been made in one of the limestone quarries of Stromberg (Rhenish Prussia). In a small cave, such as occur frequently in the calcareous rock, the skeleton of a cave-bear was found. To judge from the thickness and length of the bones the animal must have measured at least 2½ or 3 metres in length and 2 metres in height. The teeth, of which sixteen were found, are of enormous size. Discoveries of this kind however are by no means rare in this neighbourhood, nor indeed in limestone caves generally.

AN earthquake shock was felt in Eastern France on the night of July 21-22 at Aix-les-Bains, Lyons, Grenoble, Chalons, and other places. The time of the phenomenon was 2h. 3s. a.m. at Lyons and Chalons, and the direction from north to south. No accident is reported in either of these places. The shock was also felt in Switzerland in a large number of places, at Geneva, Morges, Lausanne at about 2h. 45m. a.m. local time; indeed it is stated to have been the sharpest felt in the district since 1854. Nowhere has any accident been recorded. A terrible storm was raging in these regions on the 21st, a few hours before the earthquake took place. Spontaneous currents have interrupted also the telegraphic communications.

A SPECIAL library has been established by subscription in Paris for secular education. The subscribers have resolved to adopt a scientific creed, and to prescribe the u e of fiction in books written for young people.

MR. RICHARD ANDERSON, the author of the well-known work on "Lightning Conductors," will contribute a short series of articles on "Thunderstorms: their History and Mystery," to the *St. James's Magazine*. The first article will appear in August.

M. BRUGSCH, conservator of the Boulak Museum, has returned from Thebes with the contents of twenty sarcophagi recently discovered behind the ancient Palace of Queen Hatason. Amongst some 5000 antiquities which have been obtained the most remarkable are several mummies in a perfect state of preservation, and of considerable historical interest.

THE boring of the Arlberg tunnel is proceeding with great rapidity. The length finished since June, 1880, is 1720 metres, and an average yearly advance of 2160 metres is confidently expected after a while. The average of Mont Cenis and the St. Gothard was only 1112 and 1670 respectively. The St. Gothard tunnel will be completed by the end of September, but the lines of approach are not likely to be ready before next spring.

THE *Weserzeitung* reports that near the village of Rantrum a quantity of silver has been found buried about one foot deep in the ground. It consists of thirty-four small bars, six fragments of antique silver ornaments, and eight coins; the latter bear Arabic inscriptions, and may probably be dirhems of the Abassidic Caliph of Bagdad, who lived in the eighth century of the present era. The small bars were formerly used as money, and were weighed, before coins had any conventional value. Ornaments were frequently used in the same way. All the objects found were in a vase.

THE seventh Annual Exhibition held by the British Beekeepers' Association was opened at the Horticultural Gardens on Tuesday, and will remain open till Monday next.

A REMARKABLE eruption was recently observed by the passengers of the ss. *Glenelg*, at the northern end of the Bay of Plenty, New Zealand. The water rose suddenly to a height of four feet, and spread over a circle of sixty feet in diameter, throwing up sand, shells, stones, and mud. The steamer was only about twenty yards outside the circle. The water continued boiling for some time.

THE *Italia Centrale*, a paper published at Reggio (Emilia) announces that the most remarkable mud-volcano of the province of Emilia, the Salsa di Querzola, has developed an extraordinary activity for a few days past, and has greatly frightened the neighbouring inhabitants. Loud subterranean noise was heard even in the plains around, incandescent lava was ejected to a height of several metres, and an earthquake was also noticed. Large numbers of tourists and curious inhabitants are proceeding to Regnano to witness the spectacle.

A REMARKABLE natural phenomenon is reported from Cs. Gorbo (Szolnok-Doboka Comitat, Hungary). On June 27 the Buznau Mountain, situated close to the village of Paptelke, suddenly broke in two. The fissure measures 30 to 40 metres in breadth, 25 to 30 metres in depth, and 400 to 500 metres in length. Some of the houses in Paptelke also show cracks, so that the whole seems to have been the effect of an earthquake. A landslip took place at the same time, and a field with an apple-tree in the middle of it has moved about 10 metres nearer to the village. Great excitement prevails in the neighbourhood.

VARIOUS antique bronze arms and implements, altogether weighing about four or five kilogrammes, have been found by forest labourers at a place called Friedhofstannen, in the district of Cattenbühl, near Oberode (Hanover). They were buried in the ground at a very slight depth. The objects consist of battle-axes, a sickle, a knife for taking off the hides of animals, a bracelet, rings. They are supposed to be of Celtic or Phœnician origin. In the neighbourhood of the spot where they were found there is an ancient earth-mound, dating from a very remote period, and inclosed by a circular pit of some 400 yards in circumference, the so-called ring or "kring." Above it was the Hessian fronter fortress of Friedeweh, below it the Spiegelburg.

THE tenth general meeting of the Saxo-Thuringian Apicultural Society will be held at Quedlinburg on July 31-August 2. An exhibition of living bees in hives, also of implements and products of bee-culture, will take place simultaneously.

WE have already received the Calendar of the Newcastle-on-Tyne College of Physical Science for 1881-82.

AN interesting paper on "Prehistoric Hackney" by Mr. J. E. Greenhill, read before the Hackney Natural History Society, has been printed in a separate form.

FROM the *Proceedings* of the Liverpool Naturalists' Field Club we learn that that society has reached its majority. There is the usual account of excursions and an address by the president, the Rev. H. H. Higgins, on "Animal Defences."

THE additions to the Zoological Society's Gardens during the past week include a Silver Fox (*Canis fulvus*, var. *argentata*) from Nova Scotia, presented by Mr. S. R. Platt; three Hedgehogs (*Erinaceus europæus*), British, presented by Mr. W. Dunn, C.M.Z.S.; two Black-tailed Parrakeets (*Polytelis melanurus*) from New South Wales, presented by Mr. Gerald Arbuthnot; a Green Tree Frog (*Hyla arborea*), European, presented by Mrs. Humphrey; six Black and White Geese (*Anseranas melanoleuca*), seven Australian Wild Ducks (*Anas superciliosa*) from Australia, received in exchange. The following, amongst many other insects, may now be seen in the Insectarium:—Perfect specimens of the Swallow-tailed Butterfly (second brood from small larvæ), Camberwell Beauty, Spurge Elephant and Privet Hawk-moths, Northern Brown Butterfly, Chalk-hill Blue Butterfly and Burnet Moth. There are also fine examples of the imago of the Atlas Moth, and larvæ of this moth larger than any yet grown in England.

OUR ASTRONOMICAL COLUMN

ENCKE'S COMET.—The ephemeris of this comet for its approaching re-appearance was issued from Pulkowa last month; but unfortunately the editor of the *Astronomische*

Nachrichten has not considered it was necessary to reprint it in that journal, where the ephemerides for previous appearances have always found a place.

After the death of Dr. von Asten, the calculations for this comet were taken up by Dr. O. Backlund, who has continued the computation of the perturbations by Venus, the Earth, Mars, Jupiter, and Saturn from 1878 to 1881, taking account also of the effect of the so-called resisting-medium on the mean motion and angle of excentricity. The following are the elements of the comet's orbit:—

Epoch 1881, July 2.0 M.T. at Berlin

	°	′	″	
Mean anomaly	319	26	48.7	
Longitude of perihelion	158	30	5.5	M.Eq. 1881.0.
" ascending node ...	334	34	3.1	
Inclination	12	53	0.3	
Angle of excentricity	57	43	30.75	
Mean daily sidereal motion ...	1072″.65852			

From these elements we find—

Semi-axis major ...	2.22005	Perihelion dist. ...	0.34301
Semi axis minor ...	1.18547	Aphelion dist. ...	4.09709
Excentricity	0.8454969	Period	1208.21 days

The track of the comet in the heavens at this appearance is a favourable one for observation in this hemisphere. It will be nearest to the earth on October 11, when it will be distant 0.543 of the earth's mean distance from the sun, and situated in the constellation Leo Minor, in the vicinity of the star Fl 21, and the theoretical intensity of light will attain a maximum on November 9, when the comet situated near 89 Virginis will rise about 2h. 15m. before the sun.

The following ephemeris for the month of August is contracted from the accurate one given by Dr. Backlund, and applies to mean midnight at Berlin:—

	R.A. h. m. s.	Decl. ° ′	Log. distance from Sun.	Log. distance from Earth.
August 1	2 56 22	+26 31.0	0.2701	0.2400
3	3 0 23	27 0.3		
5	3 4 31	27 30.2	0.2592	0.2173
7	3 8 46	28 0.7		
9	3 13 9	28 31.9	0.2478	0.1934
11	3 17 41	29 3.9		
13	3 22 22	29 36.6	0.2359	0.1683
15	3 27 14	30 10.2		
17	3 32 18	30 44.5	0.2233	0.1419
19	3 37 35	31 19.8		
21	3 43 6	31 56.0	0.2101	0.1142
23	3 48 53	32 33.1		
25	3 54 58	33 11.2	0.1962	0.0850
27	4 1 23	33 50.2		
29	4 8 10	34 30.3	0.1814	0.0543
31	4 15 23	+35 11.3		

It remains to be seen whether the comet can be perceived with the larger telescopes of the present day with a less intensity of light than 0.24, which was that at the time of its discovery in August, 1848, with the 15-inch refractor at Harvard College, U.S., and which will correspond to about the day of new moon, August 24.

COMET 1881 *c*.—Elements of this comet have been published in circulars issued from Lord Crawford's Observatory at Dun Echt, from which it appears that it will increase very considerably in brightness. The perihelion passage does not take place until August 21. The comet is rapidly approaching the earth.

BIOLOGICAL NOTES

ON SOME NEW LOWER GREEN ALGÆ.—George Klebs publishes some very interesting facts about a number of forms of green Algæ found living within the cell-tissues of some flowering-plants. The pains-taking way in which the life-history of these have been worked cannot be too sufficiently admired. For full details the student should refer to the numbers of the *Botanische Zeitung* for April and May, where also will be found excellent coloured illustrations of all the species. In order to call attention to these curious species we give the specific diagnosis in detail:—*Family Protococcaceæ. Genus Chlorochytrium.*—Through continued division into two parts each cell becomes resolved into spherical zoospores, which upon leaving the mother-cell conjugate within the gelatinous envelope. The zygozoospores before becoming surrounded with a membrane make their way by means of processes into the intercellular spaces of living plants. During the time favourable for vegetation many generations follow one another in a single year; that nearest to the winter falls into a resting stage. *Chlorochytrium lemnæ.*—This species lives in the widened intercellular spaces of the parenchyma of the *Lemna trisulca*: cells chiefly spherical or elliptical; the part of the growing zygospore which remains in connection with the epidermis becomes a spherical cellulose plug. In the next genus, Endosphæra, through continued division into two, each cell falls into a number of daughter-cells surrounded with a membrane, from which, by further division, the spherical zoospores result; those, taking their origin from the same mother-cell, immediately upon leaving it conjugate; they make their way into living tissues like those of the Chlorochytrium. The formation of zoospores only takes place in the spring; the new generation requires a full year to reach maturity. The species *Endosphæra biennis* lives in the intercellular spaces of the sub-epidermal parenchyma of leaves of *Potamogeton lucens*: its cells are mostly spherical; the part of the germinating zoospore which remains in connection with the epidermis soon dies off. In the genus Phyllobium at the time of maturity, the protoplasm of every cell containing chlorophyll is differentiated into cylindrical or spherical portions, through the changing of some of these into smaller ones, zoospores—both macro and micro are formed—these conjugate. The zygozoospores make their way into the stomates of partly living, partly dead leaves of phanerogams. The development of every cell takes a year. The species *Phyllobium dimorphum* lives in the leaves of *Lysimachia nummularia*, Ajuga, Chlora, &c.; the zygozoospores develop processes which grow into branched green tubes among the vascular bundles belonging to the veins of the leaves. The protoplasm of those zygospores which develop a process forms into either a spherical or longish resting cell, which lasts during the winter, and in the next summer again develops zoospores. According to the surrounding circumstances the processes are well developed or not. They may be quite rudimentary, in which case small tubeless resting cells become formed, which form asexual zoospores. In the genus Scotinosphæra every cell shows at the time of maturity a differentiation of its green protoplasm into cylindrical or spherical bodies; by their conjugating, during which a reddish granular substance is secreted, a single mass is formed, through whose repeated division, during which division the granular substance is gradually again taken up, the zoospores are formed. These are asexual, and make their way into decaying vegetable tissues. Their development lasts a year. *Scotinosphæra paradoxa* lives in the dead or dying tissues of *Lemna trisulca*, and also in species of Hypnum. Its cells are mostly spherical, and the zoospores are spindle-shaped. (*Botanische Zeitung*, May 27, 1881.)

ON THE INFLUENCE OF INTERMITTENT ILLUMINATION ON THE DEVELOPMENT OF CHLOROPHYLL.—Dr. Karl Mikosch and Dr. Adolf Stöhr publish the result of their investigations made in the Physiological Institution of the Vienna University. The results of these they summarise as follows:—If a continuedly-lasting 2.5-minutes illumination of etiolated seedlings of barley or oats is compared with an intermittent illumination in the relation of 1 : 1 lasting five minutes, then one will find that in both cases the light is throughout present an equal time. Now if the chlorophyll-formation takes place at the same time as the illumination, then the working of the continued illumination must exactly correspond at the end of 2.5 minutes with the sum of the single effects of the intermittent illumination. As a matter of fact, however, at the end of the continued illumination there has been either no chlorophyll formed, or at any rate no quantity of it that can be pointed out anywhere. On the other hand, the mass of chlorophyll which is formed during the intermittent illumination is beyond doubt capable of being pointed out with a spectroscope. One must therefore imagine that a certain time elapses between illumination and chlorophyll-formation. From this however it follows:—1. That the chlorophyll-formation is a process of photochemic induction. The first trace of chlorophyll that can be pointed out with a spectroscope appears in seedlings of barley and oats grown in the dark after illumination lasting five minutes; it is a matter of indifference whether it is illuminated the whole time through, or only in the relation of 1 : 1 second. One cannot take for granted that in the one case only the half quantity of chlorophyll is formed when an alcoholic solution even shows the absorption-lines of the chlorophyll spectrum, still this will clearly disappear if the solution is made

half as weak again. Consequently the smallest possible effective light for the formation of chlorophyll is attained by intermittent illumination. During the formation of chlorophyll light is supplied in superabundance by a continued illumination in the same manner as at the heliotropic bendings.

A FRENCH physiologist, M. Gley, has made some delicate experiments on himself with regard to the effects of attention and intellectual work on cerebral circulation. His results confirm those of M. Mosso, and he has added some new observations. He finds that the rhythm of the heart through intellectual work is slightly accelerated; and this increase seems in direct ratio of the intensity of the attention. Thus the pulse was more frequent when the author studied geometry, with which he had little familiarity, than when he studied philosophy, of which he had a good knowledge. While the heart-rhythm is accelerated the carotid artery is dilated during cerebral work, and the carotidian pulse becomes dicrotic. But the radial pulse becomes smaller and less ample. The phenomena of congestion observed in the brain persist a certain time after cerebral activity.

CHEMICAL NOTES

By the action of methylic iodide, in presence of sodium, on an alcoholic solution of morphine, M. Grimaux has succeeded in producing codeine, identical in properties with the naturally-occurring alkaloid (*Compt. rend.*). If ethylic iodide is employed in place of the methyl salt, a new alkaloid differing in composition from codeine by CH_2, is produced. M. Grimaux proposes to call all the homologous bodies of this series *codeines*, and to distinguish the commonly called codeine as *codomethyline*, the new homologue as *codethyline*, &c.

IN *Gazzetta Chimica Italiana* S. Valente describes a striking lecture experiment illustrative of the fact that chlorine replaces iodine from binary compounds. A jar, 500 c.c. capacity, is filled with dry hydriodic acid gas, and another, 250 c.c. capacity, with dry chlorine, the jars being separated by a glass plate, and the larger being uppermost; on withdrawing the plate decomposition of the hydriodic acid occurs with a flash of rose-coloured flame, and separation of iodine.

SS. BARTOLI AND PAPASOGLI claim to have prepared mellitic and hydromellitic acids by the long-continued electrolysis of water, using carbon electrodes (*Nuovo Cemento*).

S. FUNARO describes two nickeliferous minerals from the Apennines in the *Gazzetta Chim. Ital.*, to one of which he gives the formula $(FeNi)_7S_8$, and to the other the formula $Cu_2R_{10}Sb_4S_{17}$, where R = Cu : Fe : Ni = 3·4 : 4·2 : 2·4.

IN continuing his investigation of the action of hydrogen peroxide on aromatic compounds (NATURE, vol. xxiv. p. 111) Dr. A. R. Leeds shows that in some of these compounds the peroxide acts only as an oxidiser, in other cases it replaces hydrogen by (OH), and sometimes both actions occur together (*Berliner Berichte*).

THE same chemist has repeated (*Amer. Chem. Journ.*) many of these experiments, wherein ozone is said to be produced by the action of heat on metallic and non-metallic oxides; he finds that in every case the supposed ozone reaction, obtained by bringing the evolved oxygen into contact with potassium iodide and starch, is due to traces of impurities, generally to traces of chlorine.

ACCORDING to M. Chappuis (*Bull. Soc. Chim.*) the phosphorescence of phosphorus in oxygen or air is an accompaniment of the combustion of phosphorus vapour by ozone. Phosphorus is not luminous in pure oxygen at 15°, and at the ordinary pressure, introduction of a trace of ozone causes luminosity; those substances which hinder the luminosity of phosphorus, *e.g.* turpentine oil, are substances which destroy ozone. If a little turpentine oil is brought along with phosphorus into a tube containing pure oxygen, and a small quantity of ozone is then passed in, the phosphorus exhibits luminosity for a few moments only; M. Chappuis supposes that this is due to the combustion of phosphorus vapour by the ozone, and that the transiency of the phenomenon is explained by the rapid removal of the ozone by the turpentine oil.

EXPERIMENTS on the action of heat on oxides of manganese, by S. V. Pickering, are detailed in *Chem. News*. According to this chemist some specimens of manganese oxides undergo a slow molecular change when kept. Thus a sample containing, when freshly prepared, 85·149 per cent. MnO_2, 9·356 per cent. MnO, and 5·490 per cent. H_2O, *lost* 1·065 per cent. oxygen when heated to 100°, but after eighty days the same sample *gained* 0·24 per cent. oxygen when heated to 100°, and 1·114 per cent. at 195°.

HERR E. RAMANN concludes from his experiments (*Berliner Berichte*) that the passivity of iron is always caused by the formation of a layer of magnetic oxide (Fe_3O_4) on the surface of the iron. In addition to nitric acid, the following liquids induce passivity in iron, viz. ammoniacal silver nitrate solution, solutions of nitrate of silver, ammonium, aluminium, nickel, cobalt, or iron.

THE same author describes an amalgam of iron, nearly of the composition expressed by the formula Hg_3Fe_2, prepared by the action of sodium-amalgam on finely-divided iron in presence of water. Dry sodium-amalgam has no action on iron.

HERREN V. MERZ AND W. WEITH have investigated the action of heat on various amalgams with the view of determining whether these bodies lose mercury regularly as temperature increases, or whether they exhibit the properties of definite compounds. The results, which are detailed in the *Berliner Berichte*, seem to show that many amalgams, *e.g.* of gold, silver, copper, bismuth, lead, cadmium, &c., although very easily decomposed by heat, nevertheless contain their component elements in definite proportions by weight; such amalgams are probably to be classed as molecular compounds. Amalgams of the alkali metals exhibit the properties of definite compounds in a greater degree than amalgams of the other metals.

IN the *Berichte* Herr V. Meyer publishes a note on the densities of the vapours of the halogens, in which he states that he means to relinquish the further working out of these problems to M. Crafts. He states that he has obtained numbers for the densities of phosphorus and arsenic which stand midway between those required by the formulæ P_4 and As_4, and P_2 and As_2.

VARIOUS papers on new nitrogen derivatives of carbon compounds are published in the same *Berichte*, by Prof. V. Meyer and his students; these papers promise results of much interest. Hitherto "azo-compounds" have only been known in the aromatic series; nitroso-substitution compounds of what is apparently azo-ethane are described by Prof. Meyer, especially $NO-C_2H_4-N_2-C_2H_4-NO$. A new series of organic bases called "*ketines*" is also described. The starting-point of this series is *ketine* or nitrosoacetene, $CH_3-CO-CH_2(NO)$.

HERR STRECKER (*Annalen Phys. Chem.*), from determinations of the velocity of sound in chlorine, bromine, and iodine gases, has obtained the following numbers for the specific heats of the e gases:—

	Chlorine.	Bromine.	Iodine.
At constant pressure ...	0·115 ...	0·05504 ...	0·03489
At constant volume ...	0·08373 ...	0·04257 ...	0·02697
Ratio of values of the two specific heats ...	1·323 ...	1·293 ...	1·294

From these results it is concluded that the action and reaction between the atoms in the molecules of these gases is different in kind from that which subsists in other diatomic molecules, *e.g.* oxygen or carbon monoxide.

REMSEN has again investigated the action of finely-divided iron in inducing the formation of cyanide when nitrogen is passed over a hot mixture of carbon, iron, and an alkaline metal; he finds (*American Chem. Journ.*) that freshly reduced iron induces a large formation of cyanide, but that iron after keeping for some time loses this power.

FROM experiments on the decomposition of barium carbonate by ammonium chloride solution, Tommasi (abstract in *Berliner Berichte*) concludes that an aqueous solution of sal-ammoniac contains free ammonia and free hydrochloric acid.

REFERENCE was recently made in these Notes to the experiments of Jones on gaseous boron hydride; Reinitzer describes experiments (*Wien. Akad. Ber.*) which appear to show that when dilute hydrochloric acid acts on potassium boride the solid green-brown amorphous powder which is formed is a boride of hydrogen approximately of the formula $B_{3\cdot32}H$.

CONSIDERABLE doubt has been expressed whether calomel is or is not liable to decomposition in the human system, with production of corrosive sublimate. According to experiments described by P. Hoglan (*Chem. News*) calomel is slowly changed

by the action of water at the temperature of the body with formation of corrosive sublimate: and this change is accelerated by the presence of citric acid, sodium chloride, or sugar.

FROM analyses and examination of the distillation vessels used in zinc furnaces, Herren Schulze and Steiner (*Jahrb. für Mineral.*) have found that these vessels contain well-formed crystals of *zinc-spinell* (or zinc aluminate) along with crystals of tridymite. The authors discuss the bearing of their results on the natural formation of minerals of the spinell group in limestones; they point out that the generally accepted hypothesis that such limestones must have been in a fused state for some time, is not necessary, but that the minerals may have been formed by the action of vapour penetrating the solid hot limestone. The action of gases on a softened rock mass may give rise to molecular changes resulting in the production of various minerals.

PHYSICAL NOTES

A CONTINUOUS registering thermometer for recording the temperature of the body has just been described by its inventor, M. Marey. It consists of a brass tube communicating with a Bourdon manometer, containing oil, and closed. Any change of temperature, by altering the internal pressure, makes the curved manometer tube curl more or less, and to it is fixed an index which registers the movements by inscribing them on a recording cylinder. The thermometric bulb may be at some distance from the inscribing apparatus, being connected by a flexible tube of annealed copper. Two such bulbs may be applied to different parts of the body, even to the interior. It is possible therefore to note the relations between the temperatures of the interior and exterior of the body. If we remember rightly, an analogous but more portable instrument was suggested some time ago by Mr. Donald Macalister, but we are not aware whether his instrument is yet before the public.

PROF. E. LOMMEL describes in *Wied. Ann.* a new polarising apparatus in which two plates of platinocyanide of magnesium, cut perpendicularly to the optic axis, are used as polariser and analyser, just as in the tourmaline pincette. Such a section of this crystal transmits a blue light, which, when the angle of incidence exceeds 2°, is found to be perfectly polarised in the plane of incidence, and it therefore can be used, if tilted to that extent out of perpendicularity to the axis, as a polariser for a pencil of parallel blue rays. One curious point in respect to the behaviour of a thin film thus prepared is the following:—Let ordinary non-polarised light be looked at through the crystal while the latter is normal to the line of sight. A white central spot, perfectly circular in form, and non-polarised, is observed in the middle of a blue field, which is polarised at every point radially. The only other crystals which can be used for polarising pincettes are the tourmaline and herapathite (iodo-sulphate of quinine): the point of difference between these and the platinocyanide of magnesium is that while the two former (which are negative crystals) absorb the ordinary ray, and must therefore be cut parallel to the optic axis, the latter absorbs the extraordinary ray, and must therefore be cut at right-angles to the optic axis.

THE galvanic properties of carbon have been closely examined by Dr. Hanichi Muraoka, a Japanese student at Strassburg. He determined the specific resistance and the change of resistance with increase of temperature of all kinds of hard carbon, including Siberian graphite, gas-retort carbon, the artificial carbons used for electric lighting by several well-known firms, and even the graphitic compound used in Faber's lead-pencils. The specific resistance (at 0° C.) of the last was 952·0, while that of the first was 12·2. The artificially-prepared carbons ranged from 36·86 to 55·15. In all however the resistance *decreased* with a rise of temperature, the coefficient of decrease being greatest for the Siberian graphite, least for a carbon pencil prepared from coke by Heilmann of Mühlhausen. This result entirely confirms the recent researches of Siemens and Beetz. The thermo-electric powers of the various samples of carbon were also determined, with respect to that of graphite; their thermo-electromotive force was in every case + to graphite, and varied from 423 microvolts for the Faber pencil carbon to 9·26 microvolts for the gas-retort carbon (of Parisian manufacture) used for battery plates.

HERR P. VOLKMANN observes that in the determination of the specific gravity of heavy liquids, such as quicksilver, by means of the specific gravity bottle or pyknometer, the change of volume of the vessel caused by the internal pressure may introduce a source of error, especially as the glass vessel may suffer a sub-permanent strain from which its recovery is not immediate. He gives an example of this error in the case of a pyknometer provided with a capillary tube marked in equal divisions. This pyknometer was filled with mercury *while standing in mercury* until the top of the column stood at 68·1 divisions. On taking it out of the mercurial bath the column fell to 65·4, and on dipping it it again rose to 68·5. The necessary precautions to avoid this error having been taken, a redetermination was made of the specific gravity of distilled mercury at 0° C., the density of water at 0° C. being assumed (at Pierre's value) as 0·999881. The new value for the density of mercury comes out as 13·5953 ± ·0001, which is a little less than the lowest of the values given by Regnault.

PROF. S. P. LANGLEY has made the following calculation:—A sunbeam one square centimetere in section is found in the clear sky of the Alleghany Mountains to bring to the earth in one minute enough heat to warm one gramme of water by 1° C. It would therefore, if concentrated upon a film of water 1-500th of a millimetre thick, one millimetre wide, and ten millimetres long, raise it 83⅓° in one second, provided all the heat could be maintained. And since the specific heat of platinum is only 0·0032, a strip of platinum of the same dimensions would, on a similar supposition, be warmed *in one second* to 2603° C.—a temperature sufficient to melt it!

THE alteration of the zero of thermometers after undergoing sudden changes of temperature is a well-known phenomenon, as is also the gradual rise in the zero in thermometers during the first few months after they have been made. M. Pernet has lately examined the question whether the distance between the "boiling point" and the "freezing point" of a thermometer is constant at all different stages of secular alteration in volume of the bulbs, and finds that this is so, provided the freezing point be determined immediately *after* the boiling point. On the other hand, if the boiling point be determined and a long interval elapse before the zero is determined, there is considerable error. Suppose a thermometer to be (owing to recent heating or to long rest) in any particular molecular state. In this state its reading will probably be in error: but this amount (so far as due to the above cause) may be ascertained by immediately plunging the thermometer into ice, and observing the error of the zero reading. In order that a thermometer should read rightly at any particular temperature it should be exposed for a considerable time to the temperature for which exact measure is desired, or else for a few minutes to a slightly *higher* temperature.

THE transparency of ebonite to heat rays may be shown by the following pretty and simple experiment. A radiometer is set revolving by the light and heat radiated from an argand gas-flame or the flame of a paraffin lamp. When a thin sheet of ebonite is interposed the rotations continue though with slightly diminished energy. But the thinnest sheet of note-paper interposed suffices to check the revolution of the vanes.

PROF. GRAHAM BELL has sought to prove whether the diaphragms subjected to intermittent radiation in one of the forms of the radiophone did or did not execute mechanical vibrations. The experiment of Mr. W. H. Preece of attaching a Hughes' microphone to the disk had led to negative results. But Prof. Bell has shown that the central region of the disk (on which the rays fall) is set into mechanical vibration; and he has proved the point by employing a modification of the mechanical microphone of Wheatstone. A stiff metallic wire is fixed to the centre of a thin metallic disk mounted at the extremity of a flexible hearing tube. When the end of the wire is pressed against any vibrating body its sounds are heard, and the vibrations at different points of the disk of a radiophone can be successively explored. The vibrations are found to be almost entirely confined to the illuminated area at the centre of the disk. A Hughes' microphone attached to the edges of the disk would therefore not easily give any indications. With this simple apparatus one very curious effect was obtained. An intermitted beam of rays was focussed upon a brass kilogramme weight, and the surface was explored with the point of the metallic microphone. Over all the illuminated area and for a very short distance outside it a feeble but distinct sound was detected, but not over other parts.

MR. EDISON has devised a new meter for voltaic currents even more ingenious than the "Weber-meter" which he proposed a year ago to fix in houses supplied with electric lamps. In the

new instrument two copper plates are suspended in an electrolytic cell containing sulphate of copper in solution, and placed in a branch circuit through which a known fraction of the main current is shunted. The copper plates are hung upon a lever arm so adjusted that when by electrolysis one has grown a certain amount heavier (by deposition of copper) and the other grown an equal amount lighter, the lever tips up and reverses the current through the cell, and at the same time moves a registering dial-apparatus through one tooth. The action goes on again until the tilting lever is again overbalanced, and tipped back, when the current is again reversed, and another registration effected. Each "tip" clearly corresponds to the passing of an exact quantity of electricity through the cell, and the registered indications are therefore proportional to the total consumption. *But will it work?*

HERR ED. DORN has investigated the relation between the absolute diameters of molecules of gases and their dielectric capacity on the lines of a suggestion due to Mossotti, that the properties of dielectrics might be explained by supposing them to consist of non-conducting material, in which innumerable minute particles of conducting matter are imbedded.

EVERYONE knows that the very feeblest currents produce audible sounds in the telephone, which is more sensitive than any galvanometer to feeble currents. M. Pellat lately declared that the heat necessary to warm a kilogramme of water one degree would, if converted properly into the energy of electric currents, suffice to produce in a telephone an audible sound for ten thousand years continuously.

GEOGRAPHICAL NOTES

THE preparations for the International Geographical Congress, to be held in September next at Venice, together with a Geographical Exhibition, are advancing rapidly. The *Bolletino* of the Italian Geographical Society announces in its last number that the saloons for the Exhibition are already distributed among the exhibitors, and that the nations which will occupy the most space will be Italy, France, Germany, Austria and Hungary, Russia, and Switzerland. The saloons allowed for the Exhibition in the royal palace being insufficient, it was agreed immediately to proceed to the construction of provisional buildings. The Italian railway companies have granted a reduction of 30 per cent. on the prices of tickets, and of 50 per cent. on goods for members of the Congress. The Austrian Lloyd and the Navigation Company, "Rubattino e Florio," grant a reduction of 50 per cent. on passengers' fares. As to the questions to be discussed at the Congress, the Commission has already published in the *Bolletino* its reports on most of them. Among the questions are:—On the Present State of Telegraphic Determinations of Longitude, by G. Lorenzoni.—On the Determination of the Temperature of Sea-water at Different Depths; on the Measurement of Depths; on the State of the Surveys of Coasts, &c., by G. B. Magnaghi; on the Extinction of Aboriginal Races, by L. Hugues; and on the Teaching of Geography in Schools, by L. Schiaparelli. We do not hear of any great activity in the collection of British exhibits for the annexed exhibition of geographical apparatus, &c. In England, indeed, no great interest is felt in these congresses. In Russia, on the contrary, a collection of apparatus has for some time been in preparation. M. Grigorieff is to represent the Russian Government and the Imperial Geographical Society at Venice.

THE Swedish Government has decided to send a scientific expedition to Mossel Bay in the course of next year, for the purpose of collecting meteorological information. The expedition will be directed by Capt. Malmberg, and will have to remain during the summer of 1882 and the winter of 1883, in order to obtain the observations of an entire year. Mossel Bay is situated to the north of Spitzbergen, lat. 79°54, long. 16°15. The locality is well known to the Swedes. Prof. Nordenskjöld stayed there in the winter of 1872–73 with three ships. A Swedish man-of-war will take the expedition to Mossel Bay, under the command of Capt. Palander, who, after having fixed the special meteorological station of Capt. Malmberg, will return to Sweden.

WE find in the last number of the *Bolletino della Società Geografica Italiana* a paper on the journey of the late Signor G. M. Giulietti from Zeila on the Gulf of Aden to Harar. This journey was accomplished in 1879, and the narrative was intended to form part of the complete description of all Signor Giulietti's travels, but after his death M. Guido Cora published this small fragment with a map of the country. We notice also in the same publication a paper by Prof. G. Pennesi on the Italian missionaries who travelled in Lower Guinea during the second half of the seventeenth century; also accompanied with a small map of the country. The author speaks at some length of the two most interesting journeys of P. Dionigi Carli from Piacenza, and of P. Gio. Antonio Cavazzi from Montecuccolo.

COUNT WALDBURG-ZEIL, the well-known scientific explorer, started from Bremerhafen on board the steamer *Luise* for the River Yenisei on the 22nd of last month. The journey is undertaken solely for scientific purposes, Count Waldburg-Zeil intending to make collections illustrating the fauna of the Siberian coast and the sea in that district.

IN a letter just received from the Gaboon Père Delorme reports the foundation of a mission station on the Ogowé River, which the French are making peculiarly their own. The station is placed at the east end of a large island in the river, called by the natives Ozangé-Nengé, *i.e.* Island of Light, which is conveniently situated for communicating with the tribes on the banks of the Ogowé and the Ngunié, one of its principal affluents. Immediately round the station are the Galois; next to them, on the right bank of the Ogowé, come the Eningas, while further south, on the left bank, or rather on the banks of a branch of the Ogowé, which goes to form Lake Ajingo, are found the Adyombas. Père Delorme expresses a decided opinion that these three tribes are really one people; they all speak the same language and have the same laws. All of them are very vain and voluptuous. The Galois despise agriculture, and are a trading people. They go up beyond the rapids of the Ogowé in search of india-rubber, ivory, and ebony. The slaves, or in default of them the women, are left to attend to the cultivation of manioc, banana trees, ground-nuts, and sugar-cane.

THE statement that an instalment of the Geographical Society's large map of Eastern Equatorial Africa will be issued this month is, we learn, unauthorised; and though, probably owing to the long delay which has already occurred, the propriety of issuing the map in parts has been discussed, the question is still left open. When ready, the map will be published by Mr. Stanford.

THE fourteenth Congress of the Italian Alpine Club will meet at Milan on August 29 to September 2 next. An Alpine exhibition will also be held, and three excursions will be made: the first to Erba in the Brianza and the grotto of Pinto, the second *viâ* Como to Varenna on the Lake of Como, and the third to Etico, coupled with an ascent of Monte Grigna.

THE death is announced of the well-known African traveller Herr J. M. Hildebrandt. He died on May 29 last at Tananarivo (Madagascar).

DR. O. FINSCH, the Polynesian traveller, safely arrived at Sydney from New Britain at the beginning of May. He stayed over eight months in New Britain, and has thence sent forty-five cases containing natural history collections to Berlin *viâ* Hamburg. These collections consist of no less than 12,000 zoological specimens, a large number of anthropological objects, besides a series of ethnographical specimens, surpassing in number and completeness any collections yet made in this field. Dr. Finsch intends staying only a short time at Sydney, and then proceeds to New Zealand in order to become acquainted with real Maoris, for the sake of comparison with the Polynesian and Mikronesian races he has studied so minutely. Afterwards the traveller, in continuation of his Melanesian researches, intends to visit North Australia to see and study the so-called Australian negroes. For the same purpose he will try to stay upon New Guinea for some time, as he considers the minute study of real Papuans of great importance.

NEWS has been received from Commander van Boekhuyzen, the leader of the Dutch North Polar Expedition. He writes from Vardö to say that the *Willem Barents* could not reach Spitzbergen. The ice extended in a compact mass from 68° 30′ N. lat. and 6° W. long. to 73° 30′ N. lat. and 14° E. long., some twelve geographical miles to the north of Vardö. There was ice also some thirty geographical miles south of Bear Island. Commander van Boekhuyzen will make another attempt to get northwards in 72° N. lat., and then return home after a month, as he is convinced that Novaya Zemlya is completely inclosed in a barrier of ice.

LETTERS from Dr. W. Kobelt have just been received by the Rüppell Institution at Frankfort, who are the promoters of the expedition. The letters are dated from Oran. Dr. Kobelt's

travels were much impeded by a revolution among the natives and the prolonged drought. Nevertheless four cases, containing collections of seeds, plants, reptiles, insects, and mollusks, have arrived at Frankfort, and Dr. Kobelt has obtained valuable results concerning the geographical distribution of mollusks. With regard to the revolution among the Arabs it appears that they are of opinion that the fifty years during which the Prophet has permitted the French to hold Algiers are now over. Dr. Kobelt has left for Spain, where he will continue his researches.

WE are informed that Mr. J. M. Schuver, the adventurous Dutch traveller, who not long ago started on his formidable journey from Cairo to the Cape, is not at Famaka, on the southern frontier of Fazokl, as has been stated, but has established his head-quarters for the present a considerable distance to the south, and actually in the Galla country. A quantity of stores have lately been sent from London to Fazokl for him by way of Suakim, and it is Mr. Schuver's intention to return to Fazokl for them in November next, before proceeding on his southward journey. In the meantime he has established a dromedary post between his camp in the Galla country and Khartum.

IN the July number of *Petermann's Mittheilungen* Lieut. Kreitner describes at considerable length the observation made by him while in company with Count Szechenyi, journeying from Sayang in Yunnan to Bhamo in Burmah; a useful map accompanies the paper. Dr. Junker continues his letters describing his travels in the Niam-Niam country, concluding with some important observations on a visit he paid to some of the Monbuttu tribes. Dr. Radde concludes the narrative of his journey to Talysh, Aderbéjan, and Savulan.

WE have received from Perthes of Gotha parts 23 to 26 of the new edition of Stieler's Hand-Atlas. This edition has continued to appear with praiseworthy regularity, and will be completed in other six parts.

AMONG the papers in No. 20 of the *Bulletin* of the Lyons Geographical Society are the following:—The Economic Unity of the Globe, by Prof. C. Stewart Merritt; the South Pole, by M. E. Chambeyrin; the Slave Coast, by Dr. Chappet; South Africa, a lecture by the Rev. M. Coillaird, the missionary who succeeded Serpa Pinto; Lake Fucino, by M. Math. Desgrands.

THE U.S. steamer *Alliance*, in search of the *Jeannette* expedition, arrived at Hammerfest on its way to the Siberian Arctic Seas on the 24th inst.

THE Egyptian Geographical Society does not often issue a *Bulletin*, but when it does the number usually contains some good matter, often drawn from the archives of the General Staff, the chief of which is President of the Society. The number just published contains, among other matter, a paper on Cape Guardafui by Col. J. Graves of the Staff, and another on the country between the coast and the lofty plateau of Abyssinia by Gen. Stone Pasha.

COMMANDANT TITRE, who was formerly at the head of the Survey Department in Algiers, has lately published a large map of Algeria, which embodies all the most recent topographical information.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

WE have next to consider another method, which enables us to determine the motions of the solar gases. It has been already noticed that it is easy to see the prominences rushing with extreme velocity upwards in radial lines from the photosphere, and that while they are thus being carried up by some violent motion of ejection from below, they are twisted out of the radial line, now to the right, and now to the left, by what we are justified in describing as winds in the atmosphere of the sun. Those were the mere visual phenomena which were incidentally observable the moment a method was obtained of viewing the forms of prominences as well as the bright lines produced by the vapours of which they were built up, and they afforded us an opportunity of getting an insight into solar meteorology.

It was soon however perfectly clear that there was another method, in some respects a much better method, of doing this work. When we consider how it happens that we get any phenomena visible in our universe at all, we are driven to the conclusion that it depends on the fact that bodies in a state of agitation reflect, so to speak, their own state of agitation on the ether, and that the ether carries those vibrations, those agitations to our eyes. So that if we can assume, as we must assume, that the sun with its gases, consisting of hydrogen, magnesium, &c., was communicating its vibrations to the ether, and the ether was communicating in its turn its vibrations to us, it was obvious we had there an opportunity of testing a view which had been put forward by Doppler a good many years ago, to the effect that the light from a moving light source is not the same in all its qualities as light from a fixed one.

The colours which we see in the spectrum are exactly analogous to the notes which we hear in a piano when we go from one end of the scale to the other. Doppler imagined the equivalent of a piano going away from or coming towards the listener with considerable velocity — a velocity comparable, in fact, to the velocity of sound through the air. It is perfectly clear that under these circumstances we should no longer get true concert pitch, for the reason that the note which gives us a certain tone, because it produces in the air so many waves per second, will change its tone if the source of the note is coming to us. Take, for instance, a tuning-fork giving concert C, and imagine it rapidly coming to us: the waves of sound will be crushed together, we shall have more waves in a second falling on the ear, and we shall get a higher note. If we imagine, on the other hand, the tuning-fork is going away from us, the notes will be paid out at longer intervals, so to speak, and we shall get a lower note. In neither case shall we continue to have concert C. A very familiar instance where we do get this change of pitch due to change of motion, is produced in these days of very rapid railway travelling. Any of us who have been at a country railway station when the express is coming by will know that as the train approaches us the note of its whistle is at one pitch, and as it goes from us after passing it changes and gets lower, according to the velocity of the train. A familiar experimental illustration of this principle is to attach a whistle to the end of a long india-rubber tube. If then a person sounds the whistle by blowing through the open end of the tube, and while still blowing whirls it round rapidly in a vertical plane in which an observer is standing, that observer will note that when the whistle is approaching him in one part of the curve, and the waves are therefore being crushed together, the note will appear higher than when it is receding from him in the opposite part of the curve, where the waves are being, as it were, pulled asunder. Now apply that to the light of the sun. The long notes of light are red, and the short notes are blue, and if we sharpen or shorten any light note in any part of the spectrum we shall give that light a tendency to go towards the blue, and if we lengthen or flatten it we shall give it a tendency to go towards the red, so that, for instance, if a mass of magnesium gas giving the line or note in the green indicated by "*b*" is approaching us with a velocity comparable to the velocity of light, the line will change its position in the spectrum towards the blue; and if we are careful to note the exact amount of change of refrangibility as it is called, we shall have then an absolute method of determining the rate of motion of that mass of gas. This will help us in more ways than one. Suppose we observe the gas at the limb of the sun, we shall then, if we get any change of refrangibility, be justified in calling it a solar wind, because the motion thus indicated would be very nearly parallel to the surface of the sun; but if on the disk of the sun itself—take a spot, for instance, in the very middle of the disk—we get any change of wave-length such as I have referred to, it is perfectly clear that we shall no longer be dealing with what we can justly call a wind, it will really be an upward or downward current. So that this principle enables us at the limb of the sun to determine the velocity of solar winds, and at the centre of the sun to determine the velocity of those up-rushes or down-rushes, in fact, those convection currents to which Prof. Stokes has already directed attention.

The accompanying drawings (Fig. 16) were made when the sun was in a considerable state of agitation in the year 1872. They give us one of the lines of hydrogen, and indicate, I think, amply this kind of phenomenon. We have in the first figure on a large scale the "F" line of hydrogen, the line in the green at the edge of the sun. The slit—the perfectly straight slit—has been worked round the limb in search of a prominence, and it has found one. But the slit is no longer shown us as a perfectly straight line, it is in fact a very irregular one; and further than this it branches

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150) Revised from shorthand notes. Continued from p. 274.

at a certain height. The line of hydrogen has really divided into two lines of hydrogen, so that there we get, according to the principle just laid down, an indication of the fact that the hydrogen up to a certain height was very nearly at rest, and that beyond part of it was torn away, the line being deflected towards the blue, indicating that it is approaching us. Now the other Fraunhofer lines in the diagram may be looked upon as so many milestones which enable us to measure by the deflection the number of miles traversed by the gas in one second; for these deflections are nothing more or less than alterations of wave-length, and, thanks to Angström's map, we can measure distances along the spectrum in $\frac{1}{10000000}$ mm., and we know that an alteration of $\frac{1}{10000000}$ mm. in the wave-length of the F line towards the violet means a velocity of thirty-eight miles a second towards the eye; and that a similar alteration towards the red means a similar velocity from the eye; so that carrying the part

FIG. 16.—Alterations of wave-length in prominences. The dots show $\frac{1}{10000000}$ mm.

of the line which has the greatest deflection from the normal down to the dots, we find that the velocity of the solar wind under observation at that time was something like 114 miles per second.

In the second figure this same prominence is seen a short time afterwards. The tremendous rush of hydrogen has descended somewhat nearer the sun, and bringing that in the same way down to our milestones, we can give that velocity at something like fifty miles per second. The wind velocities measured in this way have amounted to 140 miles a second. The phenomena of convection currents give us velocities which very often amount to forty or sixty miles a second.

This method enables us to determine a matter which a few years ago we could not have determined in any other way. I refer to the fact that the motions of the solar winds are to a very large extent cyclonic. These various effects have been produced by varying the position of the slit a very little indeed over a small prominence.

In the first of the accompanying diagrams it will be seen that the hydrogen line indicates by its change of refrangibility that the gas is receding from us, that the waves are being lengthened out, and that they therefore have approached towards the less refrangible end of the spectrum. In the third diagram we see that in that part of the prominence the rays were being deflected towards the violet; that is to say, they were approaching us. In the middle of the prominence we get indications that they were both receding and approaching, as shown in the second diagram. Now if anybody in the moon had as good a method as this of measuring an earthly cyclone, he would see exactly this sort of thing—the part of the cyclone receding from him would give a deflection in one direction, the centre of the cyclone would give him both deflections, because he would get currents going in both directions, and on the other side of the cyclone he would get a deflection in the other direction.

So obvious and so very definite did these observations at last become that a new word had to be coined to separate the forms of

FIG. 17.—Solar cyclone. Left-hand diagram, retreating side of cyclone on slit; centre diagram, both sides on slit; right-hand diagram, advancing side on slit. The right-hand side of each diagram is the most refrangible.

the prominences as seen with a widened slit from the forms which were assumed by the prominences depending on their rate of motion.

Fig. 18 is a diagram of what have for this reason been called "motion forms," because such forms are really not the forms of the prominences at all—have nothing whatever to do with the shape of the prominences, but are simply produced by the various changes in the refrangibility of the light brought about by the varying motions in different parts. It is a very remarkable fact, noticed at the time, that some prominences seem to be shot up like so many smoke rings—little cyclones. And many of the strangest motion-forms are due to this cause. The velocities in the same prominence vary very much from the time it leaves the photosphere until it arrives at its greatest elevation in the sun's atmosphere, indeed the variations in any one prominence are almost as great as the variations observed between any two prominences.

There is another important fact connected with this: when the phenomena are observed close to the limb it is very often seen that the dark line on the surface of the sun is broken; in fact we got a doubling of the dark "F" line in exactly the same way as we got this doubling of the line in the prominence itself. That taught us that not only were these motions enormous in the case of vapours ejected from the sun, but that the subjacent parts of the sun itself—of the photosphere rather—felt that same influence.

The next point observed was (and this was an observation very difficult indeed to make near the limb) that whenever we got any very considerable velocity we got a new order of phenomena altogether, indicated in these two diagrams (Figs. 19 and 20).

It was found that the absorption of the hydrogen, or of the magnesium, or of the sodium, as the case might be, was enormously reduced; that for that part of the sun there was practically no absorption; but instead of absorption an excessive brilliance

in that part of the spectrum where the dark line would otherwise be. In the brighter portion between the two small spots (Fig. 19) the absorption is replaced by an exceedingly brilliant radiation, so brilliant indeed that it is quite impossible to draw a diagram so as to give any idea of the intense brilliancy of some of these little spots of light which one sees in the spectroscope; they fatigue the eye enormously, although they cover such a very small portion of the field of view.

Accompanying this intense radiation there is a gradual fading away of the absorption line; it wanes, and fades, and becomes almost invisible; while, on the other hand, on the other side or in other places, instead of getting a brilliant patch of light of the same width as the "F" line, we get one many times broader. We have also the absorption deflected to the left, or red end of the spectrum, and on this side it is gradually fined or eased off, so that it is very difficult to determine exactly where this broadened, deflected "F" line actually ceased to give us absorption; whereas at the other side, where it changed its refrangibility towards the blue end of the spectrum, we have an enormous patch of light. Now the explanation of that is perfectly simple: we have at one part of the spot an enormous up-rush, an ejection of hydrogen so intensely hot that it declines naturally to absorb the light from anything behind it, because it finds nothing hotter. This gradually replaces the absorbing hydrogen which was driven down again with considerable velocity, and so changed its refrangibility towards the the red.

Enough has been said already to show that this method of studying solar phenomena *in situ* has really helped us enormously with regard to the chemical nature of the sun. We can allocate the absorption of the hydrogen, magnesium, and so on; we can see *where* they are absorbing, and in the phenomena just referred to, where they cease to absorb, we get bright lines.

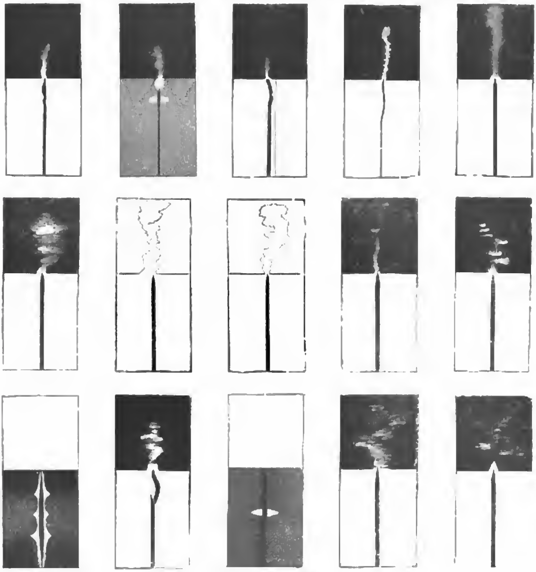


FIG. 18.—Motion-forms.

What, then, was the totality of the knowledge which had been acquired a few years ago with regard to the chemical nature of the sun's atmosphere taken as a whole—the sun's atmosphere from the upper reaches of the coronal atmosphere down to the region where, doubtless, the spot phenomena are located?

I have two little glass vessels here which ought to point what I wish to say. I have here hydrogen arranged so that I can make it luminous with a minimum of agitation. If we examined it with the spectroscope, we should find it would give the F line alone, there is nothing red about it. Now there is a region around the sun which gives us something very like that in colour, and something absolutely like it, so far as the result of spectroscopic observation is concerned. Now we have in this other little tube hydrogen in a condition to be considerably agitated, because instead of allowing it to occupy a globe, it is arranged so that the electric current has to pass through a fine capillary space in which the gas is inclosed. That is a condition which is supposed to give us the effect of high temperature. This really does give us something like what we see in the next lower solar region. This is exactly the same gas as we have in the globe, but it is treated differently, and the effect is widely different. As we pass from few encounters of molecules to many it is very much more luminous, and it is red. The level which gives such a spectrum as is got from the capillary tube is considerably lower than the one which gives us the F line alone (Fig. 21).

Is this all? By no means; going further down, as was pointed out at an early stage of the work, we get some lines seen in the spectrum of magnesium all round the sun at certain periods of the solar activity. Underneath this again we get a layer in which lines seen in the case of sodium are almost as constantly seen. Still a lower depth—practically there is no end of them—in which we get the lines of iron and other substances. There are many lower variable layers depending upon local disturbance. Tacchini, an eminent Italian observer, has studied these very carefully. We have by these observations a means of determining the fact that the solar atmosphere consists of what may be very conveniently and justly called a very considerable number of layers; and what happens with these layers is this. If the sun is quiet, or if we observe any particular part of the sun at any particular time at which it is not agitated, the layers

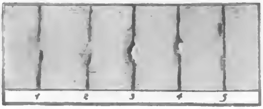


FIG. 19.—Contortions of F line on disk. 1 and 2, rapid downrush and increasing temperature; 3 and 4, uprush of bright hydrogen and downrush of cool hydrogen; 5, local downrushes associated with hydrogen at rest.

visible at that time, few in number, are nearly concentric (Fig. 21), but the moment there is any agitation in any part of the sun the lower layer shoots up into the next layer above it; the next shoots up into the one next above that; and so on (Fig. 22). How far into the very confines of the solar atmosphere this sort of action goes we do not know, because it wants more time to observe than is afforded by an eclipse, but it is certainly known that from the very lowest layer to the upper hydrogen one the layers are made to obey this same sort of rhythmic movement, and extend over like so many shells, so many domes on every part of the sun, which is being most violently agitated at the time.

So far then we have so many shells, so to speak, so many thinnings out.

Tacchini's work shows well that observers have gone into considerable detail. I give one of his drawings (Fig. 23).

The figure shows two separate portions of the chromosphere, and below each portion is shown the height above the photosphere

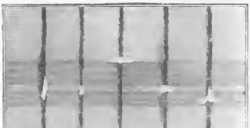


FIG. 20.—Contortions of F line on disk, in connection with spots and uprushes of bright hydrogen.

to which the various substances indicated by the lines given extend. Thus it will be seen that the magnesium stratum reaches the greatest elevation, next the so-called 1474 stuff, then an undetermined substance giving a line at wave-length 4923, another giving a line at 5017, then sodium, then a substance giving a line between B and C, another with a line between B and A, and finally one with a line at 5369. The two last layers were not observed in the second portion shown. It will be observed that most of the lines seen in these small prominences belong to substances with which we are totally unacquainted on this earth.

So much for the first results obtained in localising the solar chemistry. We pass from a general theory, saying that the absorption is above the sun, and that the sun consists of such and such chemical substances; we go to a very much more complete picture, in which we say that the solar absorption is built up by vapours of so and so, and so and so, corresponding to different heights, changing their forms, changing their shapes, changing their quantities at different times, some of them being more particularly visible in the bright ejections from the interior called prominences, and others again being brought to our ken in those down-currents called spots.

Attention must next be drawn to another method of observation, or rather to the same method extended to a different line of work.

Kirchhoff, when he examined the sun as a whole, compared it with the light of a light source as a whole. So far we

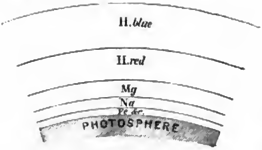


FIG. 21.—Stratification of the solar atmosphere.

have seen the difference in the results obtained when we pass from the question of observing the sun as a whole to that other more detailed question of observing every little bit of the sun that we can get at.

Now is it worth while to do this with the light source?—that is the question. Take the case of the volatilisation of iron in an electric arc. It is obvious that light from every part of a light source placed in front of a slit must enter every part of it; and if there are any differences between the light proceeding from the upper pole or the lower pole, or from the globule of iron which is being melted, and exists in a liquid form, or from the vapours of iron which surround that liquid globule—if there are any differences in these, those differences must be absolutely lost, for the reason that light from all these parts of

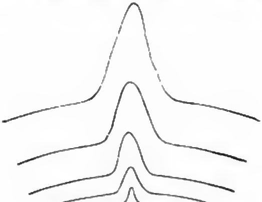


FIG. 22.—Stratification of solar atmosphere, showing the upheaval of a prominence.

the very compound phenomenon we are observing will pass to every part of the slit. But if we introduce a lens between the light source and the slit of the spectroscope, if as we throw an image of the sun on the slit, so we throw an image of the light source on the slit, we ought really to bring about a very considerable difference. For instance, we ought to be able to focus the light on the slit in such a way, that if there are any differences we should see them. It is difficult for us on a small scale to see whether there are any such differences, but if in an electric lamp we so volatilise a piece of iron, and throw the image on a screen, we readily see that there are very considerable optical differences in the various parts of the image of the light

source. We have the upper and lower pole, the globule of iron volatilising, and the vapour, both in the arc, properly so-called, and the accompanying flame. It is obvious that if we throw the image of the arc on the slit we can examine the vapour without getting any light from the pole. It is also obvious that if we arrange the slit horizontally while the current is passing in a vertical direction from one pole to the other, we shall be able,

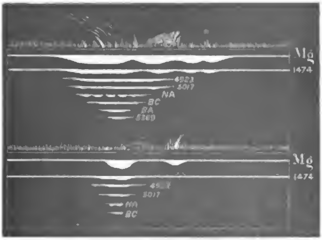


FIG. 23.—Chromosphere with jets (Tacchini).

by moving the slit upwards, to see if there are any differences observable in the vapour, first in the region where we have intense boiling and volatilisation going on, and in the necessarily cooler region where the arc is in contact with the outer air. Photographs taken in this manner show what is really observed in the case of iron under these circumstances. Whether we use the artifice of a horizontal arc with a vertical slit, or a vertical arc with a horizontal slit, does not matter, provided we keep the slit immersed in the light of the arc, and thus reflect the light from the poles, and at the same time arrange the slit so that we can compare the light in the interior portion of the light source with the light nearer its boundaries—if we take all these precautions we shall then get in the case of every substance such a result as here exhibited (Fig. 25). We have in the centre a complete spectrum, its intensity being gradually toned down, and some of the lines being left behind as we look up and down towards the boundary where we have the spectrum of that portion of the arc which was the last to retain its luminosity in consequence of its cooling. If we take horizons from the central portion of the diagram to the point furthest distant from that central axis, we find at last the light becomes absolutely monochromatic. The iron vapour at this distance from the central axis really was only radiating to us the vibration rendered visible to us by that one line. As we get nearer and nearer the centre of agitation the spectrum becomes more complex, until at length when very near the central axis we get a great many short lines introduced,

FIG. 24.—Arrangement for obtaining long and short lines. Image of the horizontal arc on slit plate of spectroscope.

so that the spectrum at that point is most complex. This I am anxious to draw attention to with some insistence, because we shall understand at once the terms long and short lines from this diagram, and about those long and short lines there will be a great deal in the sequel.

The figure shows the much more simple spectrum of sodium.

In all cases under the conditions mentioned it is quite easy to obtain photographs of longs and shorts; the longest line in the middle is D, that to the left the line is the green, and we find that one line excels all the others, and reaches a greater distance from the central axis of the photograph.

An electric lamp can be arranged to show the long and short lines of sodium on a screen; the arrangement is rather a delicate one, but the point is that we have not, as in the case of the other electric lamps, vertical poles, but horizontal ones, and we have a vertical slit close to the horizontal poles in the very middle of the lamp, so that if the experiment is carried far enough we can then prove the accuracy of the statement that the line is an image of the slit, because the slit generally melts, and we see the shape of the lines varying on the screen as the melting goes on. The lines are of different lengths: the yellow is longer than the green, the green longer than the red, and so on.

Results obtained by this method have a very important bearing upon every question connected with solar spectroscopy. When these spectra were observed—the spectra of the longs and shorts, of course we had a perfectly new set of phenomena to deal with. In all preceding spectra all the lines had been practically shown of the same length, or else the lengths had represented their intensities. But here we had, in the case of each chemical substance, to deal with the remarkable fact that when that chemical substance was examined in this way, some of the lines were long and some of them were short, and the question naturally arose,

FIG. 25.—Spectrum of sodium, showing the long and short lines.

how is it that some of the lines are long and some of them short? That question was an exceedingly difficult one to answer: I do not know that it has been thoroughly answered yet; but while researches were being made for the answer to this question certain general statements became possible which are of very considerable importance to us in our inquiry. Such a general statement as this, for instance, that if we take, say, some iron, observe its spectrum, and then mix some manganese with it, and observe the spectrum of the mixture: if the quantity of manganese is very small, we shall only get the longest line of manganese; if the quantity of manganese is increased, the next longest line will come in; and so on. So that if the spectrum of any specimen of iron was photographed, it was at once easy to see whether there was an impurity of manganese in that iron. If you make the admission that the spectra of iron and manganese, and so on, were the spectra of bodies not decomposable at the temperature which you were employing—if, for instance, there was a great quantity of manganese existing as impurity in the iron—you got a great many lines, and of course with the quantity of admixture the number of lines would go on increasing until you had 50 per cent. of each, when you would have the greatest number of lines of iron and the greatest number of lines of manganese you could ever get together, but in no case then would you get *all* the lines of iron, or *all* the lines of manganese.

The great importance of this result was, that it enabled any spectroscopist, or any chemist who chose to take the trouble and devote the time to it, to examine as to the existence of impurities in different substances: not to determine the absolute amount of impurity, but enabling him to say that in specimen A

there is a greater impurity of X than there is in specimen B, or there is a greater impurity in specimen Y of article A than there is in specimen Z, and so on. The statements were not absolute, but they were relative, and being relative they were certainly a very great advance on anything which had been done before, because until this question of longs and shorts was introduced it was almost impossible to see how to eliminate impurities.

There was another matter: it was easy to determine the behaviour of compound bodies under the action of heat by such a method. For instance, if we took the salts of calcium, or of strontium, salts which have as perfect and as complete spectra of their own as iron itself—if we heated them properly, that is to say, if we did not employ too high a temperature, and did not give them a chance of oxidising, it was exceedingly easy to see how these would behave when the heat was gradually increased, and it was then found that the longest line of the metal was always the one which showed itself first. In fact the metal always behaved as an impurity, and brought out this longest line first, in exactly the way that the smallest quantity of impurity would do. Those are small examples of the work which was done, in the one case by working at a constant temperature, and in the other case by working at varying temperatures; and you see it was possible in this way to prepare maps in which all the various impurities of one substance in another may be eliminated. A diagram will explain the way in which this new knowledge could be utilised. We have, for instance, a great number of photographs of iron, cerium, vanadium, and a great number of other chemical elements. We have compared the spectrum of each of the chemical elements with all the others, compared the lines of iron with cerium, titanium, and so on. The question now is, Given these photographs bristling with impurities—for if there were no impurities present in these photographs we should not know that our photograph was a good one—how are we to produce a map which shall be absolutely purified, in which none of these impurities shall have any effect? This diagram (Fig. 26) will show the process which was rendered possible by this long and short series of observations. We have there mapped three spectra, with their long and short lines. We have compared A with B, and we find that in the photograph which gives us A compared with B we have so many lines of the two substances. Now we say if B exists in A as an impurity, the longest line of B will be there. We look for the longest line of B, and we find it, and we put a minus sign over that line in A to show it is most probably due to an impurity of B. We then ask if there is any more B in A, and we naturally look for the next longest line of B; we find that, and we put a minus sign over that, and then we look for the next longest line, and mark that; then we look for the next one—it is not there—then there is no more of B in A. In that way, if we knew everything, we should years ago have been able to determine a spectrum of a substance A, from which all traces of the spectroscopic effects due to the presence of a substance B, had been eliminated, and we might go on with substance C, and so on, and in that way eliminate the effects of C as well as B from the substance A.

I am the more anxious to insist on this work because I shall

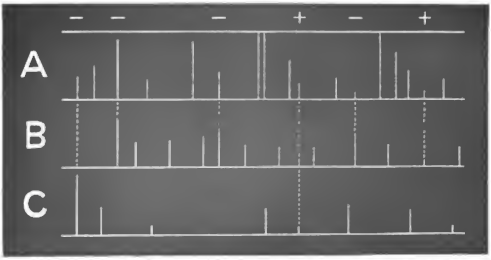


FIG. 26.—Diagram showing the process by which impurities are eliminated from spectra. The lines marked — are due to impurities of one substance in another; those marked + are common or basic lines.

have to show subsequently that it took a very long time to execute it; that the work is of a very rigid nature; and that, so far as I know, no other suggestion has been made with regard to obtaining pure spectra; and of course, if we wish to study the physics of the sun—especially the chemical physics of the sun—the first desideratum, as Kirchhoff saw, and as Angström saw, and as we all see now, is to have a series of maps absolutely and completely beyond all suspicion.

There is one other question to be referred to. Was the way perfectly clear, taking the work as it stood, four or five years ago? Did our chemical theories then explain all the facts which had been gathered by many men in many lands touching this localisation of the solar chemistry? The localisation had depended on using existing maps, whether tainted with impurities or not, observing the lines in all prominences and spots. Was everything, I say, quite clear, let us say, five years ago? I shall have to show that things were by no means at all clear; that any one who took the trouble to bring together all the results which had been obtained up to that time would have found not only that there was a rift in the lute, but that there was a very big one, and that the discord which grew upon one as one went into detail either with regard to the spectrum of the spots or with regard to the spectrum of the prominences, or with regard to the general localisation of the solar layers, was really very much more remarkable than the accord, and that although, of course, an immense deal had been done towards elaborating a view of solar chemistry a great part of which would stand, still there was a great deal which required a considerable amount of attention and a great deal more which suggested that there was still a higher light to be got before we could really face the magnificent problem with which we are attempting to grapple.

J. NORMAN LOCKYER

(*To be continued.*)

ANCHOR ICE

IN an address recently delivered at the Annual Convention of the American Society of Civil Engineers in Montreal, Mr. James B. Francis, the President, gave, *inter alia*, the results of his observations, during forty years, of anchor ice. The following is the passage in question:—

A frequent inconvenience in the use of water-power in cold climates is that peculiar form of ice called anchor or ground ice. It adheres to stones, gravel, wood, and other substances forming the beds of streams, the channels of conduits, and orifices through which water is drawn; sometimes raising the level of water-courses many feet by its accumulation on the bed, and entirely closing small orifices through which water is drawn for industrial purposes. I have been for many years in a position to observe its effects and the conditions under which it is formed.

The essential conditions are, that the temperature of the water is at its freezing-point, and that of the air below that point; the surface of the water must be exposed to the air, and there must be a current in the water.

The ice is formed in small needles on the surface, which

would remain there and form a sheet if the surface were not too much agitated, except for a current or movement in the body of water sufficient to maintain it in a constant state of intermixture. Even when flowing in a regular channel there is a continued interchange of position of the different parts of a stream, the retardation of the bed caused variations in the velocity which produce whirls and eddies and a general instability in the movement of the water in different parts of the section. The result being that the water at the bottom soon finds its way to the surface, and the reverse. I found by experiments on straight canals in earth and masonry that coloured water discharged at the bottom reached the surface at distances varying from ten to thirty times the depth.[1]

In natural watercourses, in which the beds are always more or less irregular, the disturbance would be much greater. The result is that the water at the surface of a running stream does not remain there, and when it leaves the surface it carries with it the needles of ice, the specific gravity of which differs but little from that of the water, which combined with their small size, allows them to be carried by the currents of water in any direction. The converse effect takes place in muddy streams. The mud is apparently held in suspension, but is only prevented from subsiding by the constant intermixture of the different parts of the stream; when the current ceases the mud sinks to the bottom; the earthy particles composing it, being heavier than water, would sink in still water in times inversely proportional to their size and specific gravity. This, I think, is a satisfactory explanation of the manner in which the ice formed at the surface finds its way to the bottom; its adherence to the bottom, I think, is explained by the phenomenon of *regelation* first observed by Faraday; he found that when the wetted surfaces of two pieces of ice were pressed together they froze together, and that this took place under water even when above the freezing point. Prof. James D. Forbes found that the same thing occurred by mere contact without pressure, and that ice would become attached to other substances in a similar manner. Regelation was observed by these philosophers in carefully arranged experiments with prepared surfaces fitting together accurately and kept in contact sufficiently long to allow the freezing together to take place. In nature these favourable conditions would seldom occur in the masses of ice commonly observed; but we must admit, on the evidence of the recorded experiments, that under particular circumstances pieces of ice will freeze together or adhere to other substances in situations where there can be no abstraction of heat.

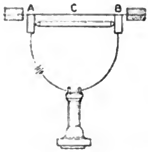
When a piece of ice of considerable size comes in contact under water with ice or other substance it would usually touch in an area very small in proportion to its mass, and other forces acting upon it and tending to move it would usually exceed the freezing force, and regelation would not take place. In the minute needles formed at the surface of the water the tendency to adhere would be much the same as in larger masses touching at points only, while the external forces acting upon them would be extremely small in proportion, and regelation would often occur, and of the immense number of the needles of ice formed at the surface enough would adhere to produce the effect which we observe and call anchor-ice. The adherence of the ice to the bed of the stream or other objects is always down stream from the place where they are formed; in large streams it is frequently many miles below; a large part of them do not become fixed, but as they come in contact with each other, regelate and form spongy masses, often of considerable size, which drift along with the current and are often troublesome impediments to the use of water-power.

Water-powers supplied directly from ponds or rivers or canals frozen over for a long distance immediately above the places from which the water is drawn, are not usually troubled with anchor-ice, which, as I have stated, requires open water up stream for its formation.

UPON A MODIFICATION OF WHEATSTONE'S MICROPHONE AND ITS APPLICABILITY TO RADIOPHONIC RESEARCHES[2]

IN August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm (Amer. Assoc. for Advancement of Science, August 27, 1880). Shortly afterwards Lord Rayleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating (NATURE, vol. xxiii. p. 274). This explanation has recently been called in question by Mr. Preece (Royal Society, March 10, 1881), who has expressed the opinion that although

Fig 1.

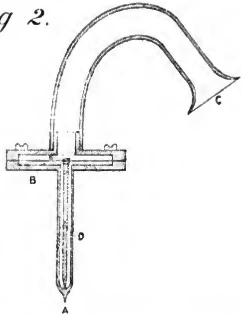


A B, Carbon supports; C, Diaphragm.

vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him the aërial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm, every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Rayleigh on account of the failure of experiments undertaken to test the theory.

He was thus forced—by the supposed insufficiency of the explanation—to seek in some other direction the cause of the

Fig 2.



A, Stiff wire; B, Diaphragm; C, Hearing tube; D, Perforated handle.

phenomenon observed, and as a consequence he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read before the National Academy of Science, April 21, 1881, that audible sounds result from the expansion and contraction of the material exposed to the beam, and that a real to-and-fro

[1] Paper clx. in the *Transactions* of the Society, 1878. Vol. vii., pages 109, 168.

[2] A paper read before the Philosophical Society of Washington, D.C., June 11, 1881, by Prof. Alex. Graham Bell.

vibration of the diaphragm occurs capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone shown in Fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the supports (A B) of the microphone might touch portions of the diaphragm which were practically at rest. It would of course be interesting to ascertain whether any such localisation of the vibration as that supposed really occurred, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated (see Fig. 2).

The instrument is a modification of the form of microphone devised in 1827 by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire (A), one end of which is rigidly attached to the centre of a metallic diaphragm (B). In Wheatstone's original arrangement the diaphragm was placed directly against the ear, and the free extremity of the wire was rested against some sounding body, like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are conveyed to the ear through a rubber hearing-tube (C). The wire passes through the perforated handle (D), and is exposed only at the extremity. When the point (A) was rested against the centre of a diaphragm upon which was focussed an intermittent beam of sunlight, a clear musical tone was perceived by applying the ear to the hearing-tube (C). The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area and in the corresponding area on the other side of the diaphragm. Outside of this area, on both sides of the diaphragm, the sounds became weaker and weaker, until at a certain distance from the centre they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes microphone (see Fig. 1) no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may therefore be reconciled with the positive results obtained in America by Mr. Painter and myself. A still more curious demonstration of localisation of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram.) and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area and for a short distance outside, but not in other parts.

In this experiment, as in the case of the thin diaphragm, absolute contact between the point of the microphone and the surface explored was necessary in order to obtain audible effects. Now I do not mean to deny that sound-waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Rayleigh actually occurs, and that it is sufficient to account for the audible effects observed.

EXPERIMENTAL DETERMINATION OF THE VELOCITY OF WHITE AND COLOURED LIGHT[1]

THE method employed in this research to measure the velocity of light resembled the method of M. Fizeau, subsequently employed by M. Cornu. A revolving toothed wheel is employed in the same way to alter the intensity of the light reflected from a distance. In the present method, however, there are two distant reflectors instead of only one. They are separated by a distance of a quarter of a mile. The observing telescope and the two reflectors are almost in the same line. The observer sees two stars of light, which go through their phases with different periods as the toothed wheel is revolved at increasing speeds. One star is increasing, while the other is diminishing, in intensity, with increase of speed of the toothed wheel. The speed required to produce equality of the light is determined by means of a chronograph.

By choosing such a speed as gives a maximum of one star at the same speed as a minimum of the other, a pair of observations eliminates all cause of doubt arising from varying brightness in the stars, and ratio of the width of a tooth to the width of a space. The distances were observed by triangulation with the Ordnance Survey 18-inch theodolite, using as a base line a side of one of the Ordnance Survey triangles. The source of light was an electric lamp. The velocities (uncorrected for rate of clock, and reduction to a vacuum) measured are as follows :—

	187,707
	188,405
	187,676
	186,457
	185,788
	186,495
	187,003
	186,190
	186,830
	187,266
	188,110
	188,079
Mean	187,167 miles a second.

The correction to vacuum is + 54 miles a second. The correction for rate of clock to a mean solar time is + 52 miles a second.

The final results for the velocity of the light from an electric lamp *in vacuo* is 187,273 miles a second, or 301,382 kilometres a second.

Using Struve's constant of aberration 20·445", we obtain for the solar parallax the value 8·77", and for the mean distance of the sun 93,223,000 miles.

On February 11, 1881, the reflected stars were seen to be coloured, one reddish, the other bluish. The particular colour of a particular star depended upon the speed of rotation of the toothed wheel. That star which was increasing with increase of speed of the toothed wheel was reddish, that one that was diminishing with increase of speed was bluish. This seems to be caused by the fact that blue rays travel quicker than red rays.

A number of tests were made to judge of the accuracy of this conclusion, and they confirmed it. In the final arrangements, the electric light was acted upon by a bisulphide of carbon prism, and part of a pure spectrum was used. Differential measurements were then made to find the difference in velocity of rotation of the toothed wheel, required to produce equality of red and of blue lights. The most convenient method was to use a driving weight slightly in excess of that required to produce equality of the light, then to fix to the pulley carrying the weights one end of a piece of stout india-rubber tubing, the other end being fixed to a point above. This gradually diminished the effective driving weight. The equality of red lights was first noted, the colour of the light was changed, and the interval of time until the blue lights were equal was measured. The rate at which the india-rubber diminished the speed was afterwards measured by the aid of the chronograph, and thus the difference of speed determined. The mean of thirty-seven determinations in this and other ways gave the result that the difference in velocity between red and blue lights is about 1·8 per cent. of the whole velocity, blue travelling most rapidly.

The general conclusion seems to be supported by a comparison of the velocity of light measured by M. Cornu and Mr. Michelson, where the source of light usually employed is taken into consideration. These are the only accurate measurements of the velocity of light hitherto published. They give us the following results :—

	Usual Source of Light.	Velocity in kilos. a Second.
Michelson's research ...	The sun near horizon ...	299,940
Cornu's ,, ...	Lime light	300,400
The present ,, ...	Electric light...	301,382

Classifying the sources of light used by Cornu, we get the following approximate relative velocities :—

Source of Light.	No. of Observations.	Approximate Relative Velocity.
Petroleum	20	298,776 kilos.
Sun near horizon ...	77	300,242 ,,
Lime light...	449	300,290 ,,

All these results seem to support the view that the more refrangible the source of light, the greater is the velocity. Bu the evidence of the present observations, indicating an excess of

[1] Abstract of a paper by Dr. J. Young, F.R.S., and Prof. G. Forbes, read before the Royal Society, March 19.

velocity for blue over red light, seeming to exceed 1 per cent. of the whole, must rest upon the merit of the present observations themselves.

SCIENTIFIC SERIALS

Journal of the Royal Microscopical Society, June, 1881, contains: On the diatoms of the London Clay, by W. H. Shrubsole, with a list of species and remarks by F. Kitton (Plate V. Fig. 1).—On the estimation of aperture in the microscope, by Prof. E. Abbe (woodcuts).—On a new species of Hydrosera (Wallich), by Dr. H. Stolterforth (*H. tricornata*), Plate V. Figs. 2, 3.—Summary of current researches relating to zoology and botany (principally Invertebrata and Cryptogamia), microscopy, &c., including original communications from Fellows and others.—Proceedings of the Society.

The Scottish Naturalist, July, 1881, contains under Phytology—Dr. Stirton, on the genus Usnea and a new genus allied to it.—Rev. J. Stevenson, Mycologia Scotica (continued).—J. Cameron, the Gaelic names of plants (continued).—Dr. F. B. White, preliminary list of the flowering plants and ferns of Perthshire.

SOCIETIES AND ACADEMIES

VIENNA

Imperial Academy of Sciences, July 7.—L. T. Fitzinger in the chair.—Dr. T. Holetschek and T. v. Hepperger, determination of the elements and ephemeris of the comet of 1881*b*.—E. Rathay, on the spermagonia of the *Aecidio mycetes*.—F. Exner, on galvanic couples consisting only of chemical elements, and on the electromotive force of bromine and iodine.—C. Block, a sealed packet.—A. Brezina, on new and little-known meteors (third report).—A. Schlosser and Z. H. Skraup, synthetical experiments on the chinolin series.—R. Brix, on the constituents of copahu (Maracaibo) and on commercial copaibic and metacopaibic acid.—H. Weidel, on dichinolins.—A. Spina, inquiry into the mechanics of intestinal and cutaneous resorption.—Th. Openchowsky, on the pressure of the pulmonary circulation.

July 14.—L. Fitzinger in the chair.—T. Glax and R. Klemensiewicz, contributions to the theory of inflammation (1st part).—E. Seherks, on the action of metals on α-bromopropionic ethyl ether.—H. Leitgeb, on *Completoria compleus*, Lohde, a fungus-parasite on fern-prothallia.—N. v. Lorenz, on the action of lead-metal on aqueous solutions of nitrate of lead.—A. Adamniewiez, preliminary note on the microscopical vessels of human cord.—A. W. Meisels, studies on the zoöid and oekoid of different vertebrates.—C. Etti, contributions to the knowledge of catechin.—T. Kachler, on the action of nitric acid on some fatty bodies made by ustion.—S. Exner, to the knowledge of the cortical motor area.

PARIS

Academy of Sciences, July 18.—M. Wurtz in the chair.—With regard to a telegram from Gabès about a recent earthquake there, and detonations preceding the shocks, M. Boussingault remembered having heard detonations at intervals during an earthquake in South America in 1827.—Observations of comet *b* 1881 at Paris Observatory, by MM. Tisserand and Bigourdan.—Theory of the plane flexion of solids, &c. (continued), by M. Villarceau.—On the reduction of quadratic forms, by M. Jordan.—Researches on glycolic ether, and on oxides of ethylene, by M. Berthelot.—On the trajectory of cyclones, and on the announcements transmitted by telegraphic cables, by M. Faye. Commandant Bridet has lately shown that if Mauritius and Réunion (Bourbon) were connected by means of a cable, the latter might be informed eighteen or twenty four hours in advance of the arrival and direction of storms. M. Bridet is trying to get this project realised.—On the integration of a linear differential equation of the second order on which evection depends, by M. Gyldén.—Effects produced by sulphide of carbon on vines of Beaujolais, by M. Henneguy.—Ephemerides of the planet (103) Hera for the opposition of 1881, by M. Callandreau.—On the tails of comets, by M. Flammarion. He replies to M. Faye, and supports M. Berthelot's theory of electric illumination.—On the vision of stars through comets, by M. André. The enlargement of the image is probably a simple effect of diffraction indicating the presence of solid or liquid nuclei in the mass of matter.—On a function similar to modular functions, by M. Poincaré.—Distribution of energy in the normal spectrum, by Prof. Langley. He gives two curves obtained from observations with his new instrument for a diffraction spectrum after and before zenithal absorption by our atmosphere. The curve of light coincides almost exactly with that of heat. There is enormous absorption by the atmosphere in the blue.—On a method enabling us to amplify the displacements of the plane of polarisation of light, by M. H. Becquerel. When monochromatic luminous rays, polarised rectilinearly, traverse a half-wave crystalline plate, the emergent rays are polarised rectilinearly in a plane which, relatively to the axis of the plate, is symmetrical with the plane of polarisation of the incident waves. This known property is utilised for the purpose indicated.—On the velocities of propagation of the inflammation in explosive gaseous mixtures, by MM. Mallard and Le Chateller. In one form of apparatus each end of the tube has a lateral orifice communicating through a caoutchouc tube with a small chamber closed with an elastic membrane, which, being pressed outwards at the moment of explosion, affects an inscribing style. The propagation in the larger tube in not of normal velocity, unless the part not yet inflamed remains as rest during the whole phenomenon. In a tube closed at one end the velocity is much greater if the gas be fired from the closed end. Even in the other case violent movements often occur in the unburnt mass, and there are various irregularities.—On the decomposition and enlargement of bands of the rainbow, by M. Ritter. Near the observer (to a distance of about 1·50m.) the two systems of cones, with parallel axes from the eyes, by which the rainbow is defined, are quite separate; thus if the drops are within that distance one should see two distinct arcs or rings. Illustrations of this deduction and others are given.—On the extraordinary temperature of July, 1881, by M. Renou. The temperature of 37°·8 in the Park of Saint-Maux, on July 15, is undoubtedly the highest ever experienced in Paris or the environs.—On hydrosulphurous acid, by M. Schutzenberger.—Action of sulphur on various metallic solutions by MM. Filhol and Senderens. It decomposes them (in heat), producing more or less complex reactions.—Separation and determination of alumina and oxides of iron and chromium, by M. Carnot.—Industry of magnesia, by M. Schloesing. This is preliminary to an account of new ways of extracting magnesia from the water of salt marshes, and even from sea-water.—On injury done in Greece by anthracnose and *Peronospora viticola*, by M. Gennadius.—On the origin of trunks of fossil trees perpendicular to the strata of the coal formation, by M. Fayol.—On some points relative to anthracic immunity, by M. Toussaint.—On a new malady of domestic geese observed in the Commune of Viviers-les-Montagnes (Tarn), by M. Caravin-Cachin.—Experiments on yellow-fever patients with phenic acid, phenate of ammonia, &c., by M. de Lacaille.—On the Cretaceous system of the Northern Sahara, by M. Rolland.

CONTENTS

THURSDAY, AUGUST 4, 1881

FOSSIL CRINOIDS

Mémoires de la Société Paléontologique Suisse. Monographie des Crinoïdes Fossiles de la Suisse. Par P. de Loriol. (Genève: Imprimerie Charles Schuchardt, 1877–1879.)

Iconographia Crinoideorum in Stratis Sueciæ Siluricis Fossilium. Auctore N. P. Angelin, Opus postumum edendum curavit Regia Academia Scientiarum Suecica. Cum Tabulis XXIX. (Holmiæ: Samson et Wallin, 1878.)

PROF. P. DE LORIOL of Geneva, who is so well known for his researches on the fossil sea-urchins, has been occupying himself for some time past with the study of the fossil Crinoids. A handsome volume, consisting of 300 pages of text and twenty-one somewhat crowded quarto plates, contains the results of his work on those discovered in the stratified rocks of Switzerland. It originally appeared in three parts, which formed portions of the volumes issued by the Palæontological Society of Switzerland for the years 1877-79.

The total number of species described by Prof. de Loriol amounts to 125, of which thirty-nine are new to science. The series commences with the well-known "Lily-Encrinite" from the Muschelkalk, and ends with a species of D'Orbigny's doubtful genus "*Conocrinus*" from the Nummulitic Eocene of Wesen. Palæozoic Crinoids are, of course, conspicuous by their absence; so that Prof. de Loriol was not hampered by having to deal with any obsolete system of classification. For the primary divisions of the class he adopts Dujardin's modification of Pictet's system. This throws such very diverse forms as *Encrinus*, *Apiocrinus*, and *Pentacrinus* into one family, the *Pycnocrinides*, which is especially characterised by the thickness of the plates of the calyx.

Each of these genera, however, is best regarded as the type of a separate family. In fact, Pictet's "family" of *Pycnocrinides* includes nearly all the non-palæozoic Crinoids or Neocrinoidea except the *Comatulæ*, and is far more comprehensive than an ordinary zoological "family."

Encrinus and *Apiocrinus* are fairly well represented in the Jurassic rocks of Switzerland. Two species of the former genus are described by Prof. de Loriol, one of which is new; and there are four species of *Apiocrinus*, one of which is new, though founded only on the characters of the stem. *Millericrinus* and *Pentacrinus*, however, are considerably more abundant. Thirty-three species of the former are described, two being Liassic and three Cretaceous; while there are no less than forty-three *Pentacrinus* species, six of which are Cretaceous, and one from the Infra-Lias (Rhætic).

Most of the species are necessarily founded only on the characters of isolated joints and fragments of stems, and are therefore only of provisional value; for two or more joints, the markings on which differ considerably, may really belong to different parts of the same stem. Nevertheless, after making allowance for these possibilities, Prof. de Loriol finds a considerable number of different types of stem which are confined to particular horizons. They thus acquire some stratigraphical value, and it is convenient to name them, but the names can only acquire a permanent value (or otherwise) when we are acquainted with the calices associated with the stem-joints in question. This is, unfortunately, but far too rarely possible.

The genus "*Pentacrinus*" is a large one, and it is almost necessary to separate off some of the best marked varieties as distinct generic types, just as has been done with *Apiocrinus*. Prof. de Loriol has attempted this subdivision in two cases, in one of which he seems to us to be fully justified, though we cannot say the same for the other. He attempts to re-establish the genus *Cainocrinus* of Edward Forbes, to include those species of *Pentacrinus* in which the basals form a complete ring and cut off the radials entirely from the top stem-joint. The characters of the stem and of the faces of its component joints are identical with those of the ordinary *Pentacrinus* type; and there is so much variation in the development of the basals among the different *Pentacrinus* species, both recent and fossil, that it is hardly worth while to separate off one of the extreme terms of the series as a distinct genus. Besides the fossil species mentioned by Prof. de Loriol *Cainocrinus* would include the recent *Pentacrinus Mülleri*, Oersted, from the Caribbean Sea, *P. Wyville-Thomsoni* from the North Atlantic, and *P. Maclearanus* of the *Challenger* dredgings.

The genus *Balanocrinus* was established by the late Prof. Louis Agassiz for a crinoidal fragment that he believed to be a calyx with an attached stem-joint; and he described the terminal face of the latter as resembling those of the stem-joints of *Pentacrinus subteres*. Prof. de Loriol, however, finds this fragment to be merely an abnormally swollen piece of stem, with the borings of some parasitic mollusc. But the stem-joints of *P. subteres* have rather different terminal faces from those of the ordinary *Pentacrinus* species; and Prof. de Loriol therefore proposes to retain the name *Balanocrinus* for this and similar forms, in which only the rim of each joint-face is crenulated, and not the central ends of its petaloid divisions as in the ordinary *Pentacrinidæ*. No calyx has ever been found associated with stem-joints of this nature except perhaps that of *P. Fisheri*. This name was given by Edward Forbes to a specimen from the Oxford clay of Weymouth that was described by Baily, who did not, however, say much about the stem-joints. Prof. de Loriol directs the attention of English palæontologists to this subject, in the hope of finding out whether Baily's species is a *Balanocrinus*. If it be so, the original specimen would acquire additional value from its being the only one with the calyx preserved.

The well-known genus *Eugeniacrinus*, which is made the type of a new family by Prof. de Loriol, is represented in the Swiss rocks by nine species, ranging from the "Oxfordien" to the "Néocomien." The curious form *Phyllocrinus* with its deeply incised radials was described by d'Orbigny as a Neocomian Blastoid allied to *Pentremites*; but it has become less interesting since Prof. Zittel showed it to be a near ally of *Eugeniacrinus*. It is represented in Switzerland by nine well-marked species, which range from the Lower Oolites to the Neocomian deposits.

Comatulæ are also abundant in the Swiss rocks, twelve species being described by Prof. de Loriol, eleven of

which are new. These are equally distributed through the Jurassic and Cretaceous series; but there are none as old as our own *Actinometra Cheltonensis* from the Inferior Oolite of Gloucestershire, nor as young as various species from the Margate chalk. One of the Neocomian species belongs to the sub-genus *Ophiocrinus* of Semper, which is characterised by the presence of five undivided rays. There are only three recent species referable to this type, all of them inhabiting different portions of the Pacific Ocean. With the *Comatulæ* must be included two species of the curious genus *Thiollicricrinus*, recently mentioned in these columns (vol. xxiii. p. 377) as being a permanent larval form.

Prof. de Loriol's Monograph with its abundant illustrations forms an excellent supplement to the fourth volume of Quenstedt's wonderful "Petrefactenkunde Deutschlands," which deals with the *Encriniden*. Taken together, the two works give us a very complete account of the Mezozoic Crinoids of Central Europe. We understand that Prof. de Loriol is now working out the French Crinoids in the same way as he has treated the Swiss ones, and we hope that he will be enabled to complete this somewhat extensive task with an equally satisfactory result. This will render a similar work on the British Crinoids more than ever necessary, and we trust that it may be accomplished within a reasonable time.

The second book mentioned at the head of this article is the late Prof. Angelin's "Iconographia of the Silurian Crinoids of Sweden." It has been published as a posthumous work by the Swedish Academy, and is unquestionably the finest work on Crinoids that has ever appeared. It consists of twenty-nine beautifully-printed folio plates, which illustrate the marvellous wealth of Crinoids and Cystids in the Silurian rocks of Sweden. Some of the figures, such as those of *Crotalocrinus*, are excessively intricate, and they are all admirably clear and well-arranged. The lamented death of the eminent Swedish palæontologist has unfortunately prevented these figures from being as useful to his successors as they would have been, had he lived to describe them. They have been edited by two of his colleagues, Professors Lovén and Lindström, who have classified the genera and species according to the system which they found sketched out in Prof. Angelin's notes and manuscripts. Unfortunately, however, the classification is an entirely unnatural one, depending upon the number of basal plates in the calyx. Wachsmuth, the chief authority in America on the Palæocrinoids, has already pointed out that while it brings together very distinct types such as *Rhodocrinus* and *Poteriocrinus*, genera which are very intimately related, such as *Platycrinus* and *Dichocrinus*, are widely separated. Among the true Crinoids forty genera are figured, comprising 176 species, many of which are new. They are arranged into twenty-three families, but as these are not defined we are unable to learn the principles upon which they were established.

There are also figures of twenty-three Cystidean species, arranged into nine genera, including one new one, which fall into three sections, the *Apora*, *Gemellipora*, and *Rhombifera*. So far as can be judged from the species referred to each section, Angelin's classification is something more than an introduction of new names for the three divisions of the group which were sketched out by Müller. Neither of the three genera included in the *Apora*, Angelin, are ordinarily referred to the *Aporitidæ*; but *Echinosphærites aurantium* and *Caryocystites*, von Buch, were placed by Müller among the *Rhombiferi* or "Cystideen mit Porenrauten"; while the third genus, *Megacystites*, Hall, is ordinarily referred to the *Diploporitidæ*, which is a parallel group to the *Gemellipora*, Angelin.

As in the case of the true Crinoids, we are unable to learn the principle of Angelin's classification of the Cystidea. It is not likely therefore to be adopted, at any rate for the present. Possibly, however, it may stand the test of future discoveries better than the Müllerian system, though we do not think this contingency a very probable one.

In spite of the inconsistencies which we have mentioned, the "Iconographia" must be indispensable to every student of the Palæocrinoidea. A glance through its pages makes one long to see some really good illustrations of our British species. There are many specimens of the utmost beauty and novelty, both in our public museums and in private collections, which we hope will some day be properly described in a "Monograph of the Fossil Crinoids of the British Isles."

OUR BOOK SHELF

The Countries of the World. By Robert Brown. Vol. vi. (London: Cassell and Co.)

WE are surprised that, after so many volumes of this work have been devoted to the description of America and Asia, the whole of Europe and of Africa are disposed of in a single volume, a considerable part of it being devoted, moreover, to the Turkish Empire. This last is allotted 58 pages, whilst the whole of Europe is dealt with in 104 pages, and the whole of Africa in other 104 pages. Moreover, why should Turkey have the favour of receiving thrice as much space as Russia, which is actually dismissed in only eight pages, whilst France, Germany, Italy, and Spain have only four pages each. Does the Russian Empire, or Spain, with their variety of climate, of soil, and of population, afford less interest for the general reader than Asiatic Turkey, and Italy less than Senegambia or Liberia?

It is obvious that such a distribution of space must affect the entire value of the work. Certainly when reading Mr. Brown's book we have admired in many instances the talent with which he succeeds in condensing in to very few pages a good description of a country; but the book being intended to afford more interest to the general reader than a simple text-book of geography, the author has been compelled to enter into generalisations which cannot but give a false idea of the subject. Is it possible that the reader can have a true conception of the climate of France when he learns from Mr. R. Brown's book that "the climate is one of the finest in Europe—mild, equable, and healthy, in spite of the hot winds from Africa, which sometimes impinge on the southern districts, and the chilly 'mistral' which sweeps down from the Alps in the north"? Or, what an idea will be impressed upon his mind of Paris, when he learns only that "in Paris centres the most polished society of the world. From Paris are sent forth *the books, the bonnets, the pictures*, and possibly even the vices which are so largely aped by the rest of the civilised world. It is the city of pleasure. But, contrary to the general impression, the morals of Paris, if not high, are not superlatively low; for though these are depraved enough, they are infinitely superior in many respects to those of Vienna, Naples, Bucharest, and even Berlin, which is more cir-

cumspect and prudish." All this is quite right, but is it a description worthy of the great capital of the Continent? The same might be said of all the other countries touched by Mr. R. Brown in this volume. All that he says is quite correct, and we do not find such blunders as are too often found in geographical works. But the necessity of giving the reader a generalisation for the purpose of rendering the book more interesting often leads the author to make such generalisations as give to the reader a most untrue conception of the subject. We must regret that Mr. Brown has been compelled to condense his work in this way, and thus seriously diminish the value of what promised to be a useful and trustworthy compilation.

Phonetik. Zur vergleichenden Physiologie der Stimme und Sprache. By Dr. F. Techmer. (Leipzig: Engelmann, 1880.)

THE excellent work on the Physiology of Language published by Dr. Techmer under the above title forms the first volume of an Introduction to the Science of Language, the rest of which is hereafter to appear. We have little hesitation in saying that it is the best *résumé* that exists at present of what is known about the nature and formation of the sounds we utter.

Dr. Techmer has been well prepared for the task he has undertaken. In the first instance a student of natural science, he next devoted himself to the acquisition of modern European languages, then of languages so remote from ours as Chinese and Sanskrit, and finally to the study of comparative philology. Naturally, however, his earlier studies had inclined him rather to the investigation of the material of speech than to the antiquarian researches of the Indo-Germanists or the psychological inquiries of the school of Steinthal. He brought to the investigation a well-trained mind, an intimate acquaintance with physics, acoustics, and physiology, a wide range of reading, and keen observation. What he has to say, therefore, is well worthy of attention.

The ground he covers is so extensive that in order to bring his work within manageable compass he can do little more than indicate the chief facts, methods of investigation and results which have been arrived at by previous phoneticians, along with copious references and notes. These will enable the reader to follow each particular point into special detail, if he so wish. At the same time Dr. Techmer has not been content with being merely a passive reproducer of the opinions of others. He has carefully tested them wherever it has been possible, and made independent experiments of his own, the results of which he lays before us. Hence his judgments and criticisms are always of value, while the numerous and carefully-drawn illustrations and diagrams which accompany his work leave little to be desired.

He has done well in not forgetting the comparative method in his treatment of phonetics. Properly to understand the physiology of human speech it is necessary to compare our vocal organs with those of reptiles, mammals, and more especially birds. Jäger has already been struck by the curious relationship that seems to exist between the power of speech and walking on two feet, and has endeavoured to explain it, though not very successfully.

Perhaps the fact that is most brought home to our minds by a study of Dr. Techmer's book is the uncertainty and obscurity that still hang over a large part of phonetics. Experts still differ radically on some of the most fundamental details of the science. This is more especially the case with that side of the science which has to do with acoustics; on the physiological side it lends itself more readily to observation and experiment, and the physiological conditions requisite for the production of particular sounds are consequently much better known. Hence it is that the nature of the consonants is far more accurately determined than that of the vowels, and that it will be long before all the difficulties connected with the formation of the latter are satisfactorily removed. The best means of overcoming them will be a succession of works like this of Dr. Techmer's, at once clear, precise, and thorough.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Medusæ

IN Mr. H. N. Moseley's "Notes by a Naturalist," on the *Challenger*, p. 404, a curious habit of Medusæ in the Island of Santa Cruz Major, is mentioned, viz. their lying on the tops of their umbrellas, its tentacles directed upwards. I think your readers may be interested to learn that I have frequently noticed Medusæ in a similar position in the West Indies. A few years ago I was quartered for some time at Port Royal, Jamaica, and in the channels between the mangroves I observed what I at first thought were Actineæ of large size on the muddy bottom, in about eight feet of water. They were very numerous. I stirred one up with the boat-hook, and was surprised to find it was a Medusa turned upside down. On being disturbed, it lazily contracted its umbrella in the usual manner and settled down again in the mud as before. The species was about a foot in diameter of umbrella, and dirty white in colour. I never saw them swimming in the mangrove creeks, though I was frequently out in a boat, and they were at all times common on the bottom, lying as described. Some time afterwards I saw what seemed to be the same species at St. George's Bay, a small island about ten miles from Belize, Honduras. It was lying in the same position on the mud amongst the mangroves, in about four feet of water. I poked several up with a stick, and they slowly swam for a short distance, and again settled down on their umbrellas. I believe it to be really the habit of the species to lie on its back, as it were, and it is interesting to find another kind in the east acting similarly. Mangrove swamps are extensive in the vicinity of Singapore, but I have not noticed any Medusæ here in that position, possibly because there is a considerable tide which leaves the mud bare at low water.

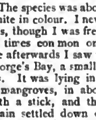
I think I have seen the habit noticed in some book, but cannot recollect where. H. ARCHER

Fort Canning, Singapore, June 28

Two Kinds of Stamens with Different Functions in the same Flower

THE following extract from a letter lately received from my brother Fritz Müller (of Blumenau, Prov. St. Catharina, Brazil) contains so new and curious an observation that it will probably interest the botanical readers of this journal.

"A species of Heeria (Melastomaceæ), which is not indigenous here, begins in my garden now to open its beautiful red flowers,



Flower of Heeria spec., longitudinally dissected. *s*, sepals; *p*, petals; a^1, one of the conspicuous yellow anthers which attract the insects; a^2 one of the inconspicuous red anthers, which powder the insects with pollen; *c*, connective of this anther; *f*, fork of this connective; *st*, stigma.

remarkable for having two kinds of differently coloured anthers. The four petals spread in a perpendicular plane; the yellow anthers (a^1) of the four shorter filaments, closely pressed together, project from the middle of the flower; their bright yellow strikingly contrasts with the violet-shining light red of the corolla; the longer anthers (a^2) are red, like the filaments, and the very long connective (*c*), which is lengthened beyond the point of insertion

into a fork (f), with two yellowish points; these points stand close beneath the yellow anthers, whilst the apical apertures of the red anthers (a^2) are placed far below them near the stigma; also the style and the stigma (st) are coloured so very like the corolla, that from some distance neither they nor the longer stamens can be seen at all. Any large bee (like Xylocopa, Centris, or Bombus), when working on the smaller anthers in order to collect pollen, would, by moving the connective fork of the larger ones, press the apertures of the latter against the ventral side of its abdomen and powder it with pollen. Until now I have only seen a little fly (Syrphidæ) and *Trigona ruficrus* visiting this flower, both too small to fertilise it. The fly takes only notice of the yellow anthers; the Trigonas, too, always sit down first on these; but most of them (the more experienced specimens?) turn then round, and go to the larger anthers, which offer a more copious pollen-store, and work on them with their mandibles or eat them up entirely. Even if larger bees acted in the same manner as *Trigona ruficrus*, they would have powdered the ventral side of their abdomen before going to plunder the latter. The pollen of both kinds of anthers is white."

HERMANN MÜLLER

Palæolithic Implements in the Thames Valley at and near London. Their Comparative Numbers

IN my former letters, NATURE, vol. xxiii. p. 604, vol. xxiv. p. 29, I cited instances of the occurrence of these objects at great heights, indicating great antiquity, at the north and south of London. After the positions of the implements on the different old river terraces are considered, their numbers, as compared with the amount of material excavated, is a subject of considerable interest, as these numbers indicate in a broad way the amount of human population.

Before I give the results of my own experience I may say here that I have had these implements in view for about twenty years. I have not searched for them myself during all this time, although at first I commonly looked over pits and roads for implements and flakes with little or no result.

I had four reasons for beginning a thorough examination of the London gravels:—1. I had long taken a great interest in the subject. 2. I had particularly noticed the implement found in Gray's Inn Lane now in the British Museum, I had looked over Col. A. Lane Fox's collection from Acton and Ealing, and I knew of two implements from the gravels excavated near my own house. 3. I felt disappointed at not meeting with Thames valley implements myself. 4. I had been unwell through overwork, and my doctor told me I should not be well again till I regularly took a four-miles daily walk.

In the early spring of 1878 I determined to walk over the London gravels and note the constituent stones—not walk over the roads and pits once or twice, but ten, twenty, or if need be fifty times, so as to thoroughly acquaint myself with the stratification and materials.

I began in May, 1878, to examine the excavated gravel at Clapton, N.E. London, in the valley of the Lea. Here, after considerable searching, I found an implement and several flakes. I then mapped out the gravels for twenty-seven miles in a line east and west of North London, and wherever the gravel has been exposed in these twenty-seven miles I have been over it a great number of times. In three years—from May, 1878, to May, 1881, I found exactly one hundred implements, mostly lingulate examples (a few ovate), and thirteen trimmed flakes, *i.e.* genuine implements, but worked on one face only. This is equal to one hundred and thirteen perfect specimens. I also found twenty-one butt-ends and six points, some broken in Palæolithic times, others showing modern fractures; side-scrapers, six; flakes about one thousand four hundred; broken fossil bones, teeth, and tusks, chiefly mammoth and horse, not uncommon. Hammer-stones of quartzite, with abraded ends, none. An unabraded quartzite pebble, such as the pebble mentioned by Mr. Perceval, teaches nothing. Even if one end is abraded off, it might have been rubbed off by other pebbles passing over it whilst naturally fixed in the bed of a stream. When both ends of a quartzite pebble are abraded *quite away*, and the abraded parts are of a *distinctly different colour* from the rest of the pebble, such a stone is probably a hammer-stone. I have several genuine examples of these of Palæolithic age, but not from the Thames valley.

On reading these notes some persons may be inclined to exclaim, What a large number of implements! How common these objects must be! My reply is they are by no means common, but as a rule *extremely rare* and *most difficult to find*. One seldom sees a first-class implement resting flat and clean in the middle of a road or pit, inviting the passer-by to pick it up. They are usually half-buried, with only part of the point, edge, or butt visible, and that part frequently covered with clay or dirt, so that it requires a sharp and trained eye to distinguish the implements and flakes from the ballast with which they are incorporated.

My first attempts were to find how many implements occurred in a hundred tons of London gravel, but I found it impossible to determine this with certainty; I however could accurately find how many miles of the actual drift I had walked over, and my experience is that I walked in three years over four thousand five hundred miles of gravel to find one hundred and thirteen implements, equal to a walk of about forty miles for one implement.

Of course the implements may be more frequent in some places, as at Milford Hill, Salisbury, and Warren Hill, Mildenhall, and much less frequent in others, but the above statement is my personal experience in the twenty-seven miles of river-gravel to the north of the Thames at London. The men working in the roads and pits often questioned me, and I set all the men to look for the implements during my absence: the whole of the men together in three years produced twenty-two extra implements, ovate or lingulate, and worked on both sides.

The mere accumulation of implements was by no means my object. I felt from the first that to entirely depend upon workmen was a great mistake, as all ill-defined instruments must be lost. I therefore personally looked out for genuine new things, and especially wished to ascertain, if possible, what the implements themselves had to teach of the *men who made them*, how the implements were deposited, and if possible to calculate their age in years. With these objects in view I have kept a manuscript book, giving the exact circumstances of finding of every implement in my collection, not only in reference to the implements belonging to the Thames Valley, but to nearly all the implementiferous river-valleys of this country. With equal care I have kept a list of non-implementiferous positions, and my experience is, the lower gravels of the Thames as at Hammersmith and Battersea are barren. As soon however as a seventy or eighty feet terrace is reached, the implements and flakes crop up. Two implements have been found in the Thames at Hammersmith and Battersea, as recorded by Mr. Evans ("Stone Implements," p. 528), but these, of course, were washed out of a higher bed. I have found several flakes and an implement at Clapham Common and Battersea Rise, but here the heights are seventy to ninety feet. The most persistent searching at Lower Battersea and Hammersmith has produced with me absolutely nothing. With your permission I will give further results in a future letter.

WORTHINGTON G. SMITH

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THE COMET

WE have received the following further communications on the lately-visible comet:—

THE appearance of a large comet has afforded an opportunity of adding to our knowledge of these bodies by applying to it a new means of research. Owing to the recent progress in photography it was to be hoped that photographs of the comet and even of its spectrum might be obtained and peculiarities invisible to the eye detected. For such experiments my observatory was prepared, because for many years its resources have been directed to the more delicate branches of celestial photography and spectroscopy, such as photography of stellar spectra and of the nebulæ. More than a hundred photographs of spectra of stars have been taken, and in the nebula of Orion details equal in faintness to stars of the 14·7 magnitude have been photographed.

It was obvious that if the comet could be photographed by less than an hour's exposure there would be a chance of obtaining a photograph of the spectrum of the coma, especially as it was probable that its ultra-violet region consisted of but few lines. In examining my photographs of the spectrum of the voltaic arc, a strong band or group of lines was found above H, and on the hypothesis that the incandescent vapour of a carbon compound exists in

comets, this band might be photographed in their spectrum.

Accordingly at the first attempt a photograph of the nucleus and part of the envelopes was obtained in seventeen minutes, on the night of June 24, through breaks in the clouds. On succeeding occasions, when an exposure of 162 minutes was given, the tail impressed itself to an extent of nearly ten degrees in length.

I next tried by interposing a direct-vision prism between the sensitive plate and the object-glass to secure a photograph which would show the continuous spectrum of the nucleus and the banded spectrum of the coma. After an exposure of eighty-three minutes a strong picture of the spectrum of the nucleus, coma, and part of the tail was obtained, but the banded spectrum was overpowered by the continuous spectrum.

I then applied the two-prism spectroscope used for stellar spectrum photography, anticipating that, although the diminution of light would be serious after passing through the slit, two prisms, and two object-glasses, yet the advantage of being able to have a juxtaposed comparison-spectrum would make the attempt desirable, and, moreover, the continuous spectrum being more weakened than the banded by the increased dispersion, the latter would become more distinct.

Three photographs of the comet's spectrum have been taken with this arrangement with exposures of 180 minutes, 196 minutes, and 228 minutes, and with a comparison spectrum on each. The continuous spectrum of the nucleus was plainly seen while the photography was in progress. It will take some time to reduce and discuss these photographs and prepare the auxiliary photographs which will be necessary for their interpretation. For the present it suffices to say that the most striking feature is a heavy band above H which is divisible into lines, and in addition two faint bands, one between G and *h*, and another between *h* and H. I was very careful to stop the exposures before dawn, fearing that the spectrum of daylight might become superposed on the cometary spectrum.

It would seem that these photographs strengthen the hypothesis of the presence of carbon in comets, but a series of comparisons will be necessary, and it is not improbable that a part of the spectrum may be due to other elements. HENRY DRAPER

271, Madison Avenue, New York

MY first view of the comet was on June 25, when it appeared through a momentary opening in the clouds, with a nucleus that, in size and brightness, seemed to equal Venus at her best. The tail, immediately at its commencement, was exceedingly bright also, but I could see no more of it then, nor at a second view, also momentary, when little more than the head was visible. Notwithstanding the immense development of tail shown by the great comet of 1861 it could not compare for an instant in brightness with the nucleus of the present one as I saw it on June 25.

On June 27 I again observed it wonderfully waned in light, with a tail plainly traceable for ten degrees, and pointing directly to the Pole. The tail was slightly curved to the right; that is, it was convex to the apparent east, or on the preceding side, and was brightest and best developed on that side. There was no time for observation with the telescope when the clouds shut up the skies for the remainder of the night.

On the next night, the 28th, I got a view with the telescope, and by an observation, which was much interfered with by clouds, I made out the position to be, in Right Ascension, 5h. 58m. 44s., and in Declination 63° 12′ 53″, at 12h. 15m. Greenwich mean time. The comet was progressively waning, but the tail seemed still about 10° long, and pointed to the Polar star. The nucleus, though growing smaller, was still exceedingly brilliant, and as large as a star of the first magnitude. The direction of the tail did not differ very much from the comet's apparent course, and seemed *concave* to it, contrary to what is usually observed with other comets. With a power of 126 on a 4½-inch achromatic I saw a curve of light extending like wings on each side of the head, and outside, with a dark space between them, appeared a large enveloping curve of inferior brightness. I fancied at times that I could discern a very faint third envelope outside all. These curves extended farther in the direction of the tail on the *following* side than on the other, though it was on the latter or preceding side that the tail showed the best definition.

On July 8 the comet seemed much diminished in light and magnitude, though the nucleus was still brightly stellar. I could now see only the inner light-curve extending on both sides of the comet's head, and through it on the following side I distinctly observed a small star shining, as it would appear, with undiminished brightness. The preceding side of the tail was still brighter and better defined than the following. I made the comet's position at 14h. G.M.T. = Right Ascension 7h. 49m. 38s., and Declination 79° 27′ 21″.

On July 11 it was still further diminished, and in the bright moonlight there was no longer any light-curve traceable in my telescope. The nucleus however continued remarkably bright and star-like, and there was an extensive nebulosity round it. The tail might be traced by the naked eye three or four degrees. I had on this night a very satisfactory micrometrical observation, but I have not as yet been able to perfectly identify the star of comparison. The calculated light of the comet was scarce more than a tenth of what it was on June 22. It is now fast receding out of naked-eye view, and of little interest except for marking its position. This on July 20, at 12h. 20m. G.M.T., I made to be 11h. 17m. 47s. in R.A., and 82° 9′ 2″ in Declination.

Schæberle's comet is now well in view, and would be visible to the naked eye only for the brightness of the sky where it is moving. The weakest binocular is sufficient to show it, and it is rapidly gaining in brightness. On the 27th inst. it appeared to me with a stellar nucleus and a tail visible for about half a degree. I made its position at 14h. 15m. G.M.T. = 6h. 14m. 41s. R. Ascension, and 43° 59′ 10″ in Declination. The observation was difficult owing to the brightness of the sky and to clouds.

Millbrook, Tuam, July 28 J. BIRMINGHAM

A POPULAR ACCOUNT OF CHAMÆLEONS[1]

THE animal to which I propose especially to direct your attention to-day is one which has been the subject of many observations, and has inspired great interest from the most ancient times.

Its name "chamæleon" is derived from two Greek words signifying "Ground-lion," a name singularly inappropriate, since it is one of those creatures which are specially fitted by their organisation to live on trees, and which are comparatively ill at ease when on the surface of the earth.

It is by no means surprising however that this creature should have attracted the attention it has attracted, such is the singularity of its appearance and the peculiarities of its habits and properties. Neither is it surprising that it should have occasioned many errors and superstitions when we consider the erroneous beliefs current amongst ourselves with respect to our own toads and slow-worms, efts, &c.

Aristotle was acquainted (as was to be expected of him) with the singular motions of its eyes, but even he fell into some curious mistakes respecting it, and he tells us that

[1] Lecture delivered at the Zoological Gardens on July 28, 1881, by St George Mivart, F.R.S.

it has no spleen and no blood except in the vicinity of its head and eyes.[1]

Pliny is careful to restate these errors, and further tells us that it lives without eating or drinking, and though generally an inoffensive animal, becomes terrible in the dog-days. He also adds,[2] on the authority of Democritus,[3] that it has the power of attracting to the earth birds of prey, so that they become in turn the prey of other animals, and that its head and neck, when burnt on oak charcoal, will cause thunder and rain to occur simultaneously. On the other hand, he rejects as fabulous the Grecian belief that its right leg cooked with a certain herb has the power of making a person invisible; that the thigh of its left leg mixed with sow's milk will induce gout if the foot be rubbed with the compound, and that a man may be made to incur the hatred of all his fellow-citizens by having his gate-posts anointed with a mixture of chamæleon's intestine and the renal secretion of an ape.

Aldrovandus informs us[4] (on the authority of older writers) that if a viper passes beneath a tree in the branches of which a chamæleon is perched, the latter will let fall some of its saliva upon the viper, which is thereby killed; and he further tells that elephants sometimes unwittingly eat chamæleons amongst the leaves of the trees on which they feed, and that the meal is a fatal one unless the elephants have recourse to the wild olive as an antidote. Gesner in his History of Animals[5] has carefully collected all these fables.

But though more accurate knowledge has dissipated many errors and destroyed many superstitions with respect to the chamæleon, yet such knowledge, far from detracting from the interest of our subject, has made it more than ever an object of scientific wonder and intelligent admiration.

My duty to-day, as I understand it, is to enable you to give a rational answer to the question, "What is a chamæleon?" and therefore to give you an accurate general notion of what the creature is in itself, and in what relations it stands to the world about it. Let us first look at the animal itself. It has a wonderfully lean and hungry look, and is in fact a hungry animal, and keen in pursuit of its insect prey at the present warm season. Its trunk is often greatly flattened from side to side, though sometimes swollen and inflated. It is never flattened from above downwards (as in so many lizards), but deep and raised up from the ground by the animal's long legs. Its head is large, and, in shape, somewhat triangular when seen in profile, and its upper surface is bounded on each side by a prominent ridge extending from the muzzle to the hinder part of the head or occiput. There is hardly any neck externally distinguishable. The limbs (of which there are two pairs) are long, uniformly slender members, each terminated by a paw in the form of a pair of pincers. There is a very long tail, also slender and curled towards its extremity, so as to be able to grasp firmly any object about which it may be rolled. The skin is rather soft and distensible. It is similar all over the body—not scaly (as in most lizards), but beset with small horny tubercles which become more close-set and flattened along the mid-line of the back and of the belly (where the tubercles project in a serrated manner), and also on the head. The mouth of the animal is very wide, but its lips meet so exactly that when closed the situation of the mouth is not readily distinguishable. The nostrils are small and open, one on each side of the muzzle, a little behind its apex. The eyes are very large, but the prominent eyeballs are covered by skin like that of the body, except at a minute central point where there is a small opening like an external pupil. Thus, instead of two eyelids, as in ourselves, there is one, formed as it were by the almost complete junction of two such as ours.

What is however much more remarkable than the form of the eyes is the manner in which they can be used. When we look at any object our eyes always move simultaneously, and are directed as much as possible towards the same object. We can thus make them converge, and we can restore the axes of our eyes to a parallel position, but we cannot make them diverge or direct one eye upwards and the other downwards at the same time. This limited power of motion in our eyes is with us innate and natural. Indeed such is the tendency to simultaneous action in our own eyes, that the very eyelids of our two eyes naturally move together, and it is only by repeated efforts that we obtain the power of moving them separately. The art of winking, then, is not an original gift but an acquired accomplishment, and this is especially the case with that refined winking which consists of a scarcely perceptible motion of the upper eyelid only.

In the chamæleon the motion of the eyes is not thus limited. It can move them with complete independence, and can simultaneously direct one eye upwards and forwards while the other gazes downwards and backwards. As far as I am aware, the chamæleon is the only animal which possesses this power.

But the chamæleon's eye has a very noteworthy internal structure. In ourselves the special organ of sight is an exceedingly delicate membrane called the "*retina*," which is spread out over the back of the inside of the eyeball. This membrane is composed for a part of its thickness of certain most minute structures termed "*rods* and *cones*," which are placed side by side, one end of each being directed outwards, and its other end towards the interior of the eyeball. At that part of the human retina which is directly opposite the pupil of the eye, is what is called "the yellow spot," which is the seat of our most acute sense of vision. In this yellow spot of ours there are many cones but few rods,[1] and the centre of it is formed of cones only. The cones of the yellow spot, moreover, are longer than those found in any other part of the retina.

According to Heinrich Müller, who has most carefully investigated the structure of this animal's eye,[2] the retina of the chamæleon has cones only, but no rods (like the centre of our yellow spot), while its cones are longer even absolutely (and therefore greatly longer relatively) than are our own. Finally the yellow spot itself is larger in the chamæleon than it is in us. Thus in all these respects the perfection of the human eye is exceeded by that of this very singular reptile.

That the chamæleon is able to gaze simultaneously at two distinct objects placed wide apart is not wonderful, because there are so many animals with eyes placed so completely on opposite sides of the head that many objects within the range of one of their eyes cannot possibly be seen simultaneously by the other. But even we are able to direct our attention simultaneously to two objects which lie towards opposite margins of our field of vision, while we neglect the sense impressions produced by all the various intermediate objects.

There is no external sign whatever of an ear in the chamæleon. Not only is there no projection, but there is no external aperture on the surface of the head, or any indication of the drum of the ear—an indication very commonly found in animals nearly allied to it. Nevertheless the chamæleon has a pair of ears substantially like those within our own skull, and these ears each communicate with the exterior by an aperture at the back of the mouth, as do ours also. It is this communication between the internal ear and the mouth which causes a man to open his mouth when he is intently listening.

[1] See his "History of Animals," book ii. chapter vii.
[2] See his book viii. Panckoncke's edition, Paris, 1830, vol. v. p. 318.
[3] See his book xxviii. chapter xxix.
[4] See his "De Quadrupedibus Digitatis viviparis," 1645. book ii. p. 668.
[5] See his "Historiæ Animalium," book ii. p. 2. The work was published in 1554.

[1] In the rest of the human retina the rods are much more numerous than are the cones.
[2] See *Würzburg naturwiss. Zeitschr.*, iii. 1861, pp. 10-42.

The chamæleon's internal ear however is not an exceptionally perfect organ like its eye. On the contrary, an important part, resembling a snail's shell in form, called the cochlea, which is largely developed in us, and which exists in a rudimentary manner in lizards generally, is absolutely and entirely wanting in the chamæleon.[1]

The tongue of this animal is the most wonderful of all its organs, and the chamæleon's entire organisation may be said to have been formed with reference to this most remarkable tongue.

If the animal's mouth be opened, its tongue will be seen as a thick fleshy mass lying between the two sides of the lower jaw. At the front end of this tongue is a cup-like depression with a prominence specially developed above and below it like an upper and lower lip. But this thick portion of the tongue thus at first visible is but a part of the entire structure. At its hinder end it suddenly narrows into another very long and cylindrical part, which is arranged and bent in transverse folds behind and beneath the thick part first described. This narrow part or, as it is called, "worm," finally bends to the front end of the lower jaw, where it becomes continuous with a third firmer part, which is rigid, because it contains a solid body within. This third part reaches from the front of the lower jaw to the back of the floor of the mouth, where it enters a sort of funnel-like depression, to the bottom of which it is firmly attached by flesh and membrane. The cavity of the mouth is very deep, as is necessary for the reception within it of this very voluminous tongue. When the tongue is elongated it may be extended six or seven inches. The action of the tongue will be spoken of in connection with what I have to say as to the other actions of the chamæleon.

The structure of the chamæleon's feet is very noteworthy. Each foot is (as has been said) practically a pair of pincers, but each branch of each pincer is made up either of two or of three toes bound together by the skin down to the very roots of the claws.

There are five toes or (as they are technically termed in anatomy) "digits" to each foot, and these five digits correspond with our own thumb and four fingers and our own five toes respectively.

In the fore-paw or hand of the chamæleon the digits which answer to our thumb, index, and middle digits are bound together in one bundle, while the digits answering to our ring and little fingers form the other bundle.

In the hind-paw or foot of the chamæleon the arrangement is different. There the digits which answer to our great and second toes are bound together in one bundle, and are opposed to another bundle formed of the third, fourth, and fifth toes.

Moreover while the three united digits of the fore-limb are directed inwards, the three united digits of the hind-limb are directed outwards.

There is yet another noteworthy point as to the structure of the paws. In ourselves the small bones which form our "wrist" and our "ankle" respectively are (as they are in almost all beasts) distinct and separate from those long, more or less slender bones which are in the palm of the hand and the sole of the foot, and which are called "metacarpal bones" in the hand and "metatarsal bones" in the foot. In the chamæleon however each metacarpal and each metatarsal absolutely unites with the wrist or ankle bone which is adjacent to it, so that they together form but one bone.

As to the internal organs of the chamæleon, I will only speak of the lungs. These organs are practically a pair of bags—air-bags—but each bag is furnished with seven or eight tubular prolongations, which seem each to end in a point. These ends however really open into certain sacs within the cavity of the body, which sacs can thus be inflated and the whole body much blown out.

[1] See Prof. Parker's paper in the *Transactions* of the Zool. Soc., vol. xi. p. 102.

The last structure I shall notice is the skin, so remarkable for the very conspicuous changes of colour it undergoes. The chamæleon's skin, like the skin of other animals, is furnished with very minute bags containing pigment. It is the presence of very many such bags containing a dark pigment which makes the negro's skin black. These pigment-bags are called "chromatophores," and the chromatophores of the chamæleon, unlike those of the negro, are contractile, and it is by the alternate contraction and expansion of chromatophores containing different coloured pigments that the changes of colour which take place in the chamæleon's skin appear to be effected.

The chamæleon does not make at all a bad pet. It is not only perfectly inoffensive, but most gentle and not at all wild, while it forms an object very interesting to contemplate. It needs to be kept warm and supplied with flies, mealworms, or other insects, and also with water, and with some branching shrub on which it may perch and climb. It is better to inclose the shrub in a glass case or cage, to prevent such accidents as happened to one of mine, which, being left alone and free, wandered to the fire-place, where it got beneath the grate, and so scorched its paws that it could no longer climb, and soon died.

Wonderful is the slowness with which the chamæleon ordinarily moves. When at rest it clings to the branches by its four paws and prehensile tail. When it wishes to advance it only moves one limb at a time. Let us say it begins by moving the right fore-limb. It first, of course, unhooks that paw, and then, bending the elbow, slowly raises it and holds it suspended a certain time, moving it right and left, forwards and backwards, till it finds a suitable foothold. Then its pincer-like fingers slowly and firmly grasp the new point of support, after which the left hind-limb performs a similar series of movements; then follows the left fore-limb, afterwards the right hind-limb, and finally the tail is unrolled, and then readjusted round some new sustaining object.

This is its ordinary mode of progress, but it can sustain itself by its tail only, and when thus hanging may seek for fresh foothold by stretching in various directions all its four limbs.

The chamæleon is probably the most thoroughly arboreal animal which exists. Many creatures of different kinds which live in trees are furnished with a prehensile tail. This is the case, for example, with the most arboreal monkeys, such as the spider- and howling-monkeys. It is also the case with that most arboreal member of the raccoon family, the kinkajou, and with the most arboreal members of the porcupine family and of the opossum order.

Arboreal animals may have their feet especially organised for climbing, as is the case with monkeys and opossums, but they are not such perfectly and exclusively climbing organs as are the chamæleon's feet. The sloths are animals the whole organisation of which is planned for tree-life, and their paws are modified to serve almost exclusively for climbing, and their digits are also bound together by skin to the roots of the claws. Moreover, in the sloths the wrist and ankle bones may more or less coalesce with the metacarpals and metatarsals, as in the chamæleon; nevertheless the sloth's digits are not pincers, but hooks only, all the digits of each foot being bound together in a single bundle. Moreover, admirable as is generally the arboreal organisation of the sloth, that animal is nevertheless devoid of a prehensile tail.

In birds the ankle-bones coalesce with the metatarsals, and there is a certain resemblance between the feet of the climbing arboreal parrots and those of the chamæleon, for though the parrot's toes are not bound together to the claws, they yet form a pair of pincers, two of them being turned in one direction and opposed to the other two Yet the mode in which they are grouped is different. for

in the parrot it is the first and fourth toes which are opposed to the second and third, instead of the first and second to the others.

In remarkable contrast with the slowness of its limb-movements is the quickness with which it can move its eyes, and above all its tongue. The chamæleon lives largely upon flies, and at first sight it would seem impossible that so apparently torpid and sluggish an animal should be able to reach and seize creatures not only active in their movements, but possessing the power of flight. At this season, when the chamæleon's appetite is keen, it may often be observed when a fly has been introduced into its cage to move about with comparative celerity, attentively watching the fly's movements, now with one and now with the other eye. It sooner or later happens that the fly settles for a few seconds somewhere within half a foot's distance of the chamæleon's head. Then the chamæleon's mouth may be observed to open and the apex of the tongue to protrude. In an instant it has shut again and the fly has disappeared. In fact the chamæleon has spit out, as it were, its enormously extensible tongue upon the insect, secured it by the viscid secretion with which the tongue is coated, and again withdrawn that organ together with the prey, but the whole has been effected with such amazing rapidity that the observer's eye cannot follow the movements of the reptile's tongue. It is projected and withdrawn without the slightest noise, but in the twinkling of an eye.

As I have said, it is this tongue which is as it were the centre of the chamæleon's organisation, and this tongue-movement is the very essence of its existence, and is its whole *raison d'être*. Without it the animal's life would be impossible, while the very slowness and deliberation of its other movements are a gain, since they enable the chamæleon to advance upon its prey within shooting distance without alarming it.

(*To be continued.*)

THE UNEXPLORED PARTS OF EUROPE AND ASIA

UNDER this title M. Venoukoff has just published an interesting paper on those parts of Europe and Asia which remain yet unexplored. It is not to be wondered at that the name of Europe should be among incompletely explored parts of the world, as there are even in Europe considerable spaces, especially in the Balkan peninsula and in North-Eastern Russia, which await scientific exploration. The war of 1877-78 certainly afforded occasion for surveying and mapping wide spaces in Bulgaria and Eastern Roumelia, but the geography of Macedonia, Epirus, and even of Thessaly is far from being exact. In Russia all the northern provinces, from the Norwegian frontier to the Ural Mountains are only known superficially; we know here only the coast and the three principal rivers—the Onega, the Dwina, and the Petchora. The great Samoyede *tundra* remains quite unexplored. Notwithstanding several journeys in the Northern Ural, this country is little known, and the interior of the great double island of Novaya Zemlya remains quite unknown, both affording, however, a very great interest, especially for geologists. As to the hydrographical exploration of the Kara Sea and of the Arctic Ocean north of Siberia, M. Venoukoff does not give them much of importance, notwithstanding what he terms the pompous newspaper writing about the trade with Northern Siberia, and he thinks that there are on the Asiatic continent several places far more interesting for explorers. For instance, Chekanovsky's and Nordenskjöld's explorations have quite changed our ideas on the geography of that land, twice as wide as France, which belongs to the basins of the Khatanga and of the Anabara. It would be a rich field of exploration for a bold traveller. The lands east from the Lena remain quite unknown, and the northern slopes of the Stanovoï Mountains are still a *tabula rasa;* the sources of the Indighirka, Kolyma, Omolon, Aniouy, and Ghijiga rivers were never visited by Europeans, and Wrangel mapped them only from hearsay. The land of the Chukchis is better known, thanks to the work of the explorers of the last century, to the recent Russian expeditions, and to Nordenskjöld's information; but all our knowledge of this country is far from being exact, and Europeans have never penetrated to the interior of the peninsula which separates the Arctic Ocean from the Pacific, and which promises to have a future as a meeting-point for the whalers, as well as for the trade in mammoth bones. The land of the Koriaks is less attractive, except for a naturalist. As to Kamchatka, certainly it is passably well known, but what a mass of work remains to be done in mapping the west coast, preparing a map of the interior, studying the most interesting geology, botany, and ethnography of the peninsula! Further south we see that the northern part of Sakhalin remains quite unexplored; the Sikhota-alin Mountains are all but unknown; and the regions between the Ussuri and Sungari Rivers, the sources of the Nonni and Argoun Rivers promise very much to the naturalist and to the geographer who would study them. The interesting peninsula of Corea will certainly be explored as soon as access to it is not forbidden to Europeans. In the Chinese Empire there are spaces as wide as England which remain unexplored. As to Eastern and Northern Thibet we are not yet sure as to what is the true source of the Brahmaputra and of the Irawaddi, and what is the importance in the orography of this land of the Kuen-Lun range. The inaccessible deserts of Eastern Turkestan are as deserving of exploration as Thibet, and the reaching of the sources of the Hoang-ho is one of the *desiderata* of geographical science. The great desert of Gobi is passably well explored, but still there remains an important problem: Does there exist, under the 42° and 43° N. lat., a chain of mountains which crosses the desert and unites the eastern Thian-Shan with the In-Shan Mountains? In northern Mongolia there still remain unknown the highlands at the upper parts of the Selenga River. In China proper there is certainly no room for geographical discoveries, but there remains very much to do as to astronomical determinations, and the substitution of a true picture of nature for the hypothetical chains of mountains which cover our maps. Useless to speak of what might be done with regard to the ethnography of Western and South-Western China. A most attractive exploration would be certainly that of Indo-China in all directions, but it is to be feared that such an exploration will remain for a long time a simple dream, because of the political institutions of this terrestrial paradise. But the exploration of Siam and Annam is one of the most necessary geographical *desiderata.* Without speaking of the Asiatic islands, where so much remains to do, M. Venoukoff points out that British India is certainly one of the best explored countries in the world, and that several parts of Europe are far behind India as to our geographical knowledge of them; but it is not the case as to those countries which are situated to the north-west of India. Afghanistan and Beluchistan await explorers, especially for certain, perhaps the most important, parts of them, as well as Southern Turkestan and the land of the Turkomans, where so much remains to do. Khorassan and Western Persia are quite well known, but Iran remains unknown; of course the exploration of these deserts, as well as of those of the interior of Arabia, would afford very great difficulties and give comparatively few scientific results. But a thorough geographical exploration of Armenia and of Asia Minor is most desirable; and, to finish with Turkey, M. Venoukoff asks if the Straits of the Hellespont and Bosphorus will

be seriously explored as to the most important question of the existence of an undercurrent in these straits?

As seen from this short sketch, there remains plenty of work for geographers and naturalists on our continent, and we may only express the wish that M. Venoukoff's idea of publishing a sketch of the "Unknown Lands," with a summary of the most important questions with regard to them, were executed on a larger scale, and that such a compendium were put into the hands of every young geographer.

NOTES

We heartily commend to the attention of our readers the announcement of the Rolleston Memorial Committee, to be found in our advertising pages.

The Scottish Zoological Laboratory, which last year supplied Mr. G. J. Romanes and Prof. Ewart with the material for their researches on Echinodermata, published in the Croonian Lecture, is this year to be placed at Oban. Those who intend to avail themselves of the advantages held out by this institution for the purposes of original work, are requested to communicate with Prof. Ewart, the University, Aberdeen.

The accommodation for anatomical work at the Prosector's rooms in the Zoological Society's Gardens, which has hitherto been somewhat limited, has lately been increased by the erection of three new working-rooms, intended for the use of students. These rooms are now finished, and are at present tenanted by a small Long-Vacation class from Cambridge, who, under the superintendence of Mr. T. T. Lister, the Demonstrator of Comparative Anatomy in that University, are studying practically the anatomy of the *Mammalia* on the abundant material in that group provided by the Society's Menagerie. The class, it may be remarked, includes two lady students from Newnham College. It is to be hoped that when this class concludes its labours at the end of the month, other students may be found disposed to profit by the new facilities for work afforded them by the Zoological Society, and that thus the expense incurred in the erection of these new rooms may be fully justified by the increased scientific results reaped in the Regent's Park from the superabundant material at the disposal of Mr. Forbes.

The honour of Knighthood has been conferred upon Mr. F. J. Bramwell, C.E., F.R.S., for his services to technical education.

The German Emperor has conferred on Dr. Schliemann the Prussian Order of the Crown of the Second Class.

The death is announced, at the age of seventy-seven, of Mr. Hewett Cottrell Watson, the well known English botanist.

Dr. Ferdinand Keller, the well-known Swiss archæologist, died at Zurich on July 21, in his eighty-first year. Dr. Keller had a decided taste for science in his youth, and shortly after leaving the University went to Paris, where he spent some time in the study of natural history. In 1832 he discovered a number of Celtic grave-mounds on the Burghölzli, a circumstance that led to the formation of the national Swiss Antiquarian Society. Of this Keller was named president, a position that he occupied for many years. The first work of the society was the exploration of the Burghölzli; and other similar researches, which threw much light on the primeval history of Switzerland, were undertaken. In 1837 began the publication of the Society's *Communications*, thirty volumes of which were wholly illustrated, and almost wholly written, by the president. In 1853 he opened the series of researches into the origin of Swiss lake-dwellings which have made his name so widely known, and revealed the way of life, in its minutest details, of a race of men whose mere existence had hardly before been suspected. Between 1860 and 1864 Dr. Keller gave to the world the results of his investigation of the Roman antiquities of eastern Switzerland. He wrote or edited further a history of the Abbey of Zurich, of the arms of Zurich, and sundry miscellaneous papers relating to life and culture in the middle ages. Until past his eighty-first year Dr. Keller continued to be an active and energetic member of the society which he had founded.

The thirty-fourth summer meeting of the Institution of Mechanical Engineers was opened on Tuesday in Newcastle-on-Tyne, this being the third visit of the Institution to that town. The chair was occupied by the president, Mr. Edward A. Cowper. The president, after some remarks on foreign competition, gave a statement of the result of the progress of mechanical engineering during the past twelve years, and expressed the hope that the latter part of the present century would be marked not only by small improvements, but by many substantial inventions for the good of mankind. Mr. I. Lowthian Bell read an exhaustive paper on the Tyne as connected with the history of engineering, and a paper by Mr. F. C. Marshall of Newcastle, on the progress and development of the marine engine, was also read.

The *Sydney Mail* brings us the welcome news that the Biological Station has been fairly established at Watson's Bay, under the direction of the well-known Russian naturalist, Dr. Miklucho-Maclay. During the last two years Mr. Maclay has been endeavouring to establish a zoological station in the neighbourhood of Sydney. Being seconded in his efforts by the Royal and Linnean Societies of Victoria and by the Royal Society of New South Wales, he has obtained from the Government an eligible site at Watson's Bay, most appropriate for the purpose. The station is situated on the shallow basin of Port Jackson, and close to the deep water of the Pacific, with large freshwater swamps and lagoons in the immediate neighbourhood, and a vast tract of wild forest country to the north, which probably will remain for a long time to come in its primitive wildness. The communication between Watson's Bay and Sydney by steamer being frequent and rapid (half an hour), the scientific work at the station will be greatly facilitated by the museums, gardens, and libraries of Sydney. The expenses for the building are estimated at 600*l*., and will be covered by the sum of 300*l*. already allowed by Government and by the subscriptions, whilst the yearly expenses will probably be covered by annual grants of the Linnean and Royal Societies of Victoria and of New South Wales. The building, which stands on a slight eminence overlooking Camp Cove, with lovely views from the balconies on all the four sides, has been constructed to suit the requirements of a biological laboratory. It has five work-rooms, two bed-rooms, a bath-room, and a room for stores, plenty of light being provided for the work-rooms, which are fifteen feet by twelve feet each and twelve feet high. The partitions between each set of rooms are constructed of studding and double-lined, the space being filled with sawdust for the prevention of noise. The trustees are quite satisfied with the building; even upon so humble a scale as the present, it promises to give very good results. It may be hoped that the institution will become a bond of union between all those in Australia who are interested in biological research. As to the expected results we can but repeat M. Maclay's words: "Next after the tropics (which are the richest in animal life) the widest field offered to the investigator of nature, and consequently the most suitable region for the establishment of zoological stations, is Australia, with a fauna so interesting, so important, and so far from being sufficiently known, especially as regards anatomy and embryology."

Prof. C. V. Riley, chief of the U.S. Entomological Commission, has accepted the position of Entomologist to the

Department of Agriculture, which has been tendered him by the new Commissioner, Dr. Geo. B. Loring. The appointment is to take effect on August 1. It will be remembered that, owing to differences with the retiring Commissioner of Agriculture, Prof. Riley resigned this same position two years ago, and we understand that he accepts it again at a salary less than that which the Members of the Entomological Commission get, because his reinstatement has been so generally demanded by scientific and agricultural associations, and because there is a near prospect, under the new administration, of the department being enlarged in scope and usefulness. Prof. J. H. Comstock, the retiring entomologist, was formerly an assistant under Prof. Riley, and will continue his connection with the department, pursuing the special work on the Coccidæ, which he has more particularly been engaged in. The reappointment of Prof. Riley will meet with approval not only in America, but here, where he has many riends and his work is well known and appreciated.

THE opening of the Exhibition of Electricity at Paris has been postponed as we anticipated. The ceremony will take place on August 11, and numerous speeches will be delivered by the public authorities. The electrical railway is being constructed from the Place de la Concorde to the Porte de l'Est of the Palais de l'Industrie. The posts required for supporting the copper wire required are attracting considerable public notice.

THE steamer *Travailleur* of the French Navy is now engaged in the Mediterranean Sea for dredging purposes, with a regular staff of scientific workers on board.

ONE of the results in Paris of the advent of two comets has been to infuse life into the popular Trocadero Observatory, which is visited by a large number of members, and where a course of lectures on several astronomical subjects is going on.

THE seventh annual conference of the Cryptogamic Society of Scotland will commence at Salen, Island of Mull, on Tuesday, August 30, 1881.

THE arrangements connected with the unveiling of the statue of Harvey at Folkestone on Saturday next, August 6, by Prof. Owen, are now nearly completed. It is expected that there will be a large concourse of doctors and others on the occasion. A small bust of the much-admired head of the statue is now on view in the western gallery of the Sanitary Exhibition at South Kensington, and can be had either in terra-cotta or imitation bronze. A reduction of the whole statue is also contemplated by the sculptor, Mr. A. B. Joy, and will be completed if a sufficient number of subscribers should order it.

IN a *Gazette* covering 250 pages have been published the new Statutes which have been promulgated by the University of Oxford Commissioners.

WE have received *Anthony's Photographic Bulletin* (New York) for June, containing an enlarged copy of Dr. Henry Draper's photograph of the nebula in Orion. The enlargement shows we have very little to hope for in this direction, still the result is a *tour de force* which reflects credit on Dr. Draper. The same number contains an excellent photograph of the Doctor himself.

WE regret to learn that the Committee, formed more than twelve months ago, to raise and present to Dr. William Farr, C.B., F.R.S., a testimonial on his retirement from the public service, have only succeeded in obtaining 930*l*. This sum has been temporarily invested in the names of trustees; and, disappointed as the Committee feel at the comparatively small success of their efforts, they have decided to close the fund so soon as they are able to obtain the small balance now required to raise the amount of the testimonial to 1000*l*. We trust there will at least be no difficulty in obtaining the small sum still required to complete the testimonial.

PHILOLOGISTS will be glad to learn that Prof. G. Beltrame's valuable papers on the Denka language have at last been published in full. They occupy the whole of the current volume of the Italian Geographical Society's *Memoirs*, and consist of three parts, a very complete grammar, an Italian-Denka and a Denka-Italian vocabulary, the former of nearly 4000, the latter of 2000 words. The grammar had already appeared in previous bulletins of the society; but these are now mostly out of print, and in any case the directors rightly considered that students would find it convenient to have all the documents collected in one volume. The Denka is one of the most widespread as well as one of the most interesting of all the Negro tongues current in the White Nile region, being spoken with great uniformity by all the tribes between 5°–12° N. along the main stream and its tributaries, who are collectively known to the Arabs as the Denka nation, but who call themselves *Jen*, a derivative form of *Jan* = race, people. They lie mainly between the Nuêr and Shillûks on the north, and the powerful Bari nation on the south, stretching westwards as far as Dar-Fertit, and south-westwards to the Nyam-Nyam country. There are altogether twenty-five chief tribes, but the common speech presents scarcely any dialectic variety except amongst the Shîr in the extreme south, and amongst the Abuyo and others in the Sobat valley. The language itself is quite distinct from any of the other Upper Nilotic idioms, and is characterised by remarkable regularity and clearness in its structure. It is entirely destitute of grammatical endings, and most of the words are monosyllabic. Prof. Beltrame belongs to a somewhat obsolete school of philologists; hence still speaks of six cases, moods, and other verbal forms. But it is sufficiently evident from his otherwise lucid exposition, and especially from his copious examples, that there are neither cases, moods, tenses, nor, strictly speaking, verbs at all in the language. It need scarcely be remarked that the Denka has nothing in common either with Galla, Ki-Ganda, or other members of the surrounding Hamitic and Bantu linguistic families. It forms one of the numerous independent groups that have been developed during the course of ages amongst the true negro tribes of Sudan and the Upper Nile Valley. Prof. Beltrame's papers must be regarded as a valuable addition to our knowledge of African forms of speech, and will prove of permanent value when the time comes for a more exhaustive study of this ethnical domain.

THE International Pharmaceutical Conference is holding its meetings in London during the present week; one of the questions engaging its attention is an International Pharmacopœia, the desirability of which is generally admitted.

A SLIGHT shock of earthquake was felt at Bangor, Maine, U.S., on July 31.

AT the last meeting of the Natural History Class of the University of Edinburgh, held on Monday, July 25, an illuminated address was presented to Prof. Alleyne Nicholson, of St. Andrews, who has been lecturing on behalf of Sir Wyville Thompson for the past three sessions. The address was signed by about 500 of the students who have attended Prof. Nicholson's lectures during the past three years.

SHORTLY before midnight on July 20 a splendid meteor was observed at Munich. It resembled a fiery ball of 30 centimetres diameter, and it passed slowly from south to north in an almost horizontal direction.

AT the distance of twenty-seven miles north-east from Padang (on the western coast of Sumatra) and some fifteen miles east from Lake Singkarah, we find a high land very similar to the Saxon highlands, and reaching a height of 2400 feet above

the sea-level, the hills of which are formed of sandstones which contain immense coal-fields. According to a description of them, just published by D. D. Veth in the *Deutsche Geographische Blätter*, these coal-fields may contain altogether no less than 300 millions of tons of good coal. The northern, or Parambahan part of them contains two main beds of coal, having an average thickness of thirty-three feet and occupying a surface of about three square kilometres, that is, about 20 millions of tons of good coal; but the rocks are rather disturbed, and therefore the extraction of coal would be difficult. The middle, or Singalut part, situated on the right bank of the Ombilin River, contains about 80 millions of tons of coal, and consists of seven thin beds of coal, which have altogether an average thickness of 16 feet, But the best coal-field is the southern, or Sungei-Durian part. situated on the left bank of the Ombilin River, which contains about 200 millions of tons of good coal. The beds of coal are three, having a thickness of 20, 7, and 7 feet, separated from one another by sheets of sandstone 50 to 70 feet thick. As to the quality of the coal, thirteen tons having been extracted and brought to Padang, it was found that as fuel for steam-engines this coal is not below that of Cardiff or Newcastle, but that it would not be as good as these two in the production of lighting gas or for iron furnaces. As to the transport of this coal to the sea-coast, it would necessitate the construction of a railway 65 or even 100 miles long.

THE Danzig *Naturforschende Gesellschaft*, which numbers now no less than 398 members, has just issued a new volume of its *Proceedings* (new series, vol. v., fascicules 1 and 2). It contains, besides the minutes of meetings of the Anthropological, Physical, Chemical, and Medical Sections, much valuable information, especially as to the botany and zoology of Prussia. The *pièce de resistance* of this volume is an essay at a topographical flora of West Prussia, by H. von Klinggraeff, being a *résumé* of the author's own researches and of what is known on the flora of this province. The author finds that there are in this province no less than 1218 species of Phanerogams, 44 species of cellular Cryptogams, 363 species of mosses, 18 of Characeæ, and 276 species of lichens, and he takes into account only the true inhabitants of the province. As to the lower Cryptogams, the figures are but provisional ones, as the algæ and mushrooms of the province are but incompletely known. We notice also in this volume papers on the freshwater molluscs of the neighbourhoods of Danzig, by E. Schumann; on the Ichneumonids of Western and Eastern Prussia, by C. Brischke; the *Reports* on the third meeting of the Botanical and Zoological Society of Western Prussia, containing a series of catalogues of plants found during botanical excursions; an interesting paper by C. Brischke, which deals with a rather neglected question, namely, with the Phytophags which the author has observed and cultivated in the neighbourhood of Danzig; a paper on the bronze-basin of Steinwage, by Dr. Fröling; and on the Cenoman fossils which are found in the diluvium near Danzig, by Dr. Kiezow.

THE St. Petersburg Naturalist's Society intend to offer various prizes for botanical papers, and to couple with them the name of the late Dr. Schleiden, who was a member of the St. Petersburg Academy and Russian State counsellor.

ON July 21 the meeting of Polish Naturalists and Physicians took place at Cracow. Some 500 members attended the meeting.

ACCORDING to the latest investigations the *Phylloxera vastatrix* has spread enormously upon the peninsula of Istria, particularly in the neighbourhood of Pirano. The plague threatens to infect the vineyards of the Karst, of Friaul, and of Carniola.

WE learn from a circular, issued by the Director of the St. Petersburg Central Physical Observatory, that all the Arctic meteorological stations will soon be opened, and that about the autumn of 1882 we will have observations from these stations for a whole year. The following, we may remind our readers, are the stations to be established:—At Upernivik, by Denmark; in Northern Finnmarken, by Norway; on the Jan Mayen Island, and, if possible, on the western coast of Grönland, by Austria-Hungary; on Spitzbergen, by Sweden; on Novaya-Zemlya (already opened a year ago) and at the mouth of Lena River, by Russia; on Point Barrow and in Lady Franklin's Bay, by the United States. Sites have already been taken by the United States and Norway to open new stations. It is to be hoped that meteorological stations will be opened, according to the wish of the International Conference at Bern, also in Antarctic regions, namely, on South Georgia, by Germany, and at Cape Horn, by France; whilst the Netherlands expect to establish a station further in the Arctic region, namely, at Dickson Haven in Siberia. The International Conference which will be opened at St. Petersburg will establish the method of observation to be adopted at all these stations.

AN International Exhibition is planned for 1883 at Shanghai.

THE additions to the Zoological Society's Gardens during the past week include two Common Marmosets (*Hapale jacchus*) from South-East Brazil, presented by the Lord W. G. Cecil; two Common Squirrels (*Sciurus vulgaris*), British, presented by Mr. C. B. Barber; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Douglas; two Common Jays (*Garrulus glandarius*), British, presented by Mr. Arthur F. Astlay; a Common Cuckoo (*Cuculus canorus*), British, presented by Mr. Harry Morrisson; a Surucucu Snake (*Lachesis mutus*) from Pernambuco, presented by Mr. C. A. Craven; two Common Boas (*Boa constrictor*) from South America, presented by Mr. G. H. Hawtayne; a Common Adder (*Vipera berus*), British, presented by Mr. J. Snow; two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, four Common Widgeons (*Mareca penelope*), an Osprey (*Pandion haliatus*), European, purchased; a Guinea Baboon (*Cynocephalus sphinx*) from West Africa, received in exchange. Amongst the additions to the Insectarium during the same time are imagos of *Antheræa yama-mai*, bred from eggs, and larvæ of the Lobster Moth (*Stauropus fagi*), Pebble and Swallow Prominent Moths (*Notodonta ziczac* and *dictæa*) and Purple Thorn Moth (*Selenia illustraria*). Numerous Ant-Lions (*Myrmeleo formicarius*) are also now emerging in the perfect state from their burrows in the sand.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

WHAT then are those precise difficulties to which reference has been made?

The number of them is considerable, and they have arisen from careful study extending over many different fields of work.

1. We most conveniently begin by noticing those suggested in the work of comparing the lines of the different elementary bodies with the Fraunhoferian lines; work done chiefly by Kirchhoff, Ångström, Thalèn and others. Kirchhoff was not long before he found that to say that each substance had a spectrum entirely and specially belonging to that particular substance was not true. He says,[2] "If we compare the spectra of the different metals with each other, several of the bright lines appear to coincide." Now Kirchhoff was working with Bunsen as his collaborateur, and therefore this was not said lightly, as we may imagine. Similarly Ångström, who was working with the assistance of the Professor of Chemistry at Upsala, was driven to exactly the same conclusion. He says[3]—

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 301.
[2] "Researches on the Solar Spectrum." Roscoe's translation. Part I. p. 10.
[3] "Recherches sur le Spectre Solaire," p. 36.

I translate his words—"Of all the bodies iron has certainly produced the greater number of lines in the solar spectrum. Some of these seem to be common with those of calcium." Thalén carried on this work, and if one compares the magnificent tables, which we owe to his untiring skill and industry, one is perfectly astonished to find the number of coincidences which he has so carefully tabulated.

2. There was another kind of work, a newer kind of work, going on. Observers began to give particular attention to the bright lines of flames, and the lines thickened in spots. And here I may limit myself to the general statement that the divergence between the spectra of the different substances as observed in the sun and in our laboratories was very much intensified as facts were accumulated. Very many of the lines observed in flames were lines with no terrestrial equivalents, and the spot-spectrum often contained lines much thickened, which were either not represented at all, or only feebly among the Fraunhofer lines.

3. Next, among all the metalloids known to chemists only one of them—or one substance classed as such, hydrogen—was present in the solar atmosphere, and that in overwhelming quantity: whereas the efforts of Ångström, Kirchhoff, and others could not trace such substances as oxygen, chlorine, silicon and other common metalloidal constituents of the earth's crust.

4. Then again, the layer which was produced by what was taken to be gaseous magnesium round the sun, a layer indicated by the brightest member of the *b* group, was always higher—always gave us longer lines—than that other layer which was brought under our ken by the bright line D seen in the spectrum of sodium.

Here was a distinct inversion of the chemical order. The atomic weight of sodium being 23, and of magnesium being 24, the sodium ought to have been higher than the magnesium; but the contrary was the fact, and that fact still remains after twelve years of observation.

5. As the work of tabulating the lines went on, and the more complex outpourings of vapours from the sun's interior were studied, it was found that the lines of iron, calcium, and so forth revealed to us were by no means the brightest lines—by no means the most important, or most prominent lines, but lines which really we had very great difficulty in recognising as characteristic of any particular spectrum. There they certainly were, however, mapped as very fine lines by the most industrious observers. Similarly with the spots, there was an absolute inversion of the thickness of the lines of any one substance in the spot. Surely there was a great screw loose here.

6. Closely allied to these observations we had another extraordinary fact. We could quite understand why in a spot the change of refrangibility of the magnesium lines when there was a storm going on in the sun should be different from the change of refrangibility of, say, the iron lines. The natural explanation was, of course, this: you have the magnesium gas going at one rate, the iron gas going at another rate, and that is all there is to be said about it. But it was soon found that the differences which could be sharply seen between the spectrum of a particular mass of magnesium vapour and a particular mass of iron vapour extended to the iron vapour itself. There were just as many variations in the refrangibility of the lines of iron itself, for instance, as there were between the lines of iron and other substances: that is to say, we had in the one case magnesium going at one rate and iron going at another rate; but when we came to deal with the iron lines alone we found one

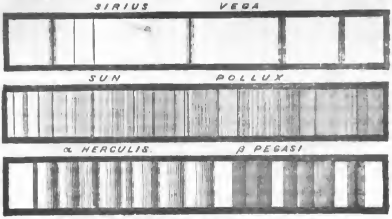


FIG. 27.—Three chief types of stellar spectra.

iron line told us the iron vapour was going at one rate, and another iron line told us that same iron vapour was going at another rate. It will be seen at once that there was a great difficulty in that.

7. Further. The lines on which these observations of the relative motions of the vapour depended were found to go in sets. In a spot, for instance, we would generally see movement indicated by one set of iron lines, whereas in a prominence we would always see a different set—a set in a different part of the spectrum altogether—registering this movement for us. Here again was considerable food for thought.

That was stated very roundly a good many years ago—in 1869. I will read what was then written on this subject:[1] "Alterations of wave-length have been detected in the sodium, magnesium, and iron lines of the spot's spectrum. In the case of the last substance the lines in which the alteration was detected were not those observed when iron, if we accept them to be due to iron alone, is ejected into the chromosphere."

That caveat with regard to iron arose from the fact that of the 460 lines recorded by Kirchhoff in 1869 only three lines of iron had been seen bright in the solar prominences.

8. Then came a point which has been very slightly alluded to already. How came it that the total chemical composition of this atmosphere of the sun, which we were taught to look upon as the exemplar of what must have once happened to our own planet, varied so enormously from the composition of the crust of our earth? No oxygen in it, no silicon, no fluorine; whereas we get abundance of titanium, nickel, and so on. It was difficult to imagine a stronger difference to exist between any two masses of matter than the chemical constitution of the incandescent sun, and of the earth, which is now cooling.

9. There was still another point of view very soon forced upon solar observers by the magnificent success which had attended the labours of Dr. Huggins, Secchi, and other observers in recording the spectra of stars. It was a most interesting inquiry naturally to see whether the stars gave spectra quite like each other, and if it should happen that they did not give spectra like each other, then the points of difference would be sure to give us some excellent working suggestions.

Now what are the facts? Here are three typical stellar spectra (Fig. 27), which show us at once that there is a very considerable difference in the phenomena. In the upper part of this diagram we have a star remarkable for the fewness of lines in its spectrum. From one end of the spectrum to the other there are not above half-a-dozen prominent lines. In the next part however we have a star which is remarkably like our own sun, both as regards the number of lines and their arrangement. In the lower part of the diagram, on the other hand, we have a star in which we get flutings instead of lines; so that we get not only a difference of degree, but a fundamental spectroscopic difference of kind. Now there is a circumstance connected with that first star with the simple spectrum very striking to any one in the habit of observing the sun, and it is this: those lines visible in the star, which, be it remembered, had been independently determined to be hotter than our sun, are precisely those lines, and none other, which we see bright on the disk of the sun itself. I have emphasised the fact that

[1] *Proc.* Roy. Soc., vol. xviii. p. 74.

we have independent evidence that the star with very few lines is hotter than our sun. It is also clear that the other star with the fluted spectrum is a star much cooler than our sun, because it was one of those red stars, the light of which is exceedingly feeble, which, on grounds independent altogether of spectroscopic evidence, are supposed to be stars in the last stage of visible cooling.

So much then for some of the earlier observations on the coincidence of metallic lines in the sun, with observations on the lines themselves in different portions of the sun's atmosphere.

10. We now come to another part of the work where we also find difficulties. Ångström, in that exceedingly important memoir which accompanies his Atlas, states:[1] "In increasing successively the temperature I have found that the lines of the spectra vary in intensity in an exceedingly complicated way, and consequently new lines even may present themselves if the temperature is raised sufficiently high." Kirchhoff, on his part, had seen phenomena very similar to those thus touched upon by Ångström, but his explanation was a different one. He did not agree that the temperature upon which Ångström laid such strong stress was really the cause at work. He attributed those changes rather to the mass and the thickness of the vapours experimented upon—nay, he went further: at a time when scarcely any facts were at his command he broached a famous theorem which went

FIG. 28.—The blue end of the spectrum of calcium under different conditions. 1. Calcium combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen. 2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current. 3. The spectrum of metallic calcium in the electric arc with a small number of cells. 4. The same when the number of cells is increased. 5. The spectrum when a coil and small jar are employed. 6. The spectrum when a large coil and large jar are used. 7. The absorption of the calcium vapour in the sun.

to prove this; and yet what had Kirchhoff himself done? how had he traversed his own theory? He states that his observations were made by means of a coil using iron poles one millimetre in thickness. Now the thickness of a short spark taken from iron poles one millimetre in thickness would probably be two millimetres. Next Kirchhoff allocated the region where the absorption which produces the reversal of the iron lines took place at a considerable height in the atmosphere of the sun, and he expected the atmosphere of the sun to be an enormous mass represented by the old drawings of coronas, so that on Kirchhoff's view the thickness of the iron vapour which reversed the iron spectrum must have been, at a moderate estimate, 10,000 miles, and yet he said that the spectrum of that, and of the light given by the coil were absolutely identical; that is to say, that the *fact* was that the variation of thickness from two millimetres to 10,000 miles made no difference. That was on the one hand; on the other hand he gave us his *theorem*, showing that a slight variation of thickness would produce all the changes which Ångström and others had observed up to that time, and which we have observed since in much greater number.

A diagram (Fig. 28) will show the sort of changes to which Ångström referred, changes which have been observed by every

FIG 29.—Fluted spectrum of iodine.

new worker who has taken up the subject. It represents the variations which take place in the spectrum of calcium in the photographic region. At a particular temperature we get a spectrum of calcium which contains no lines whatever in the blue, but when we increase that temperature—the temperature of a Bunsen burner is sometimes sufficient to produce it—we get a line in the blue. When we pass from a Bunsen burner to an electric lamp we get this blue line intensified and reversed, and at the same time we get two new lines in the violet. Using a still higher temperature in the arc, we thin the blue line, and at the expense of that line, so to speak, we thicken the two in the violet, so that the latter equal the blue line in thickness and intensity. Passing to a large induction coil with a small jar we make the violet lines very much more prominent, and using a larger induction coil and the largest jar we can get, we practically abolish the blue line and get the violet lines alone. Now we have simply produced these effects by varying the temperature, and this diagram enables me to point out one of the things to which reference will have to be made subsequently. The thicknesses of the calcium lines in the spectrum of the sun are also given. The two lines in the violet are really H and K. The

[1] "Recherches sur le Spectre Solaire," pp. 38, 39.

other line—the all-important one at low temperatures—is feeble and unimportant. So that both on the solar evidence and on the evidence of all these spectra, whatever the explanation may be, there is the undoubted fact that fundamental changes of intensity in the lines are produced by some cause or other, and if Kirchhoff's statement about the matching of lines is true for one temperature it is false for all the others.

11. In my reference to stellar spectra I mentioned the word "fluted" spectrum. Before Kirchhoff had published his first paper two very eminent Germans—Plücker and Hittorf—were working at spectrum analysis at Bonn, and they found that in the case of a great many simple substances what are called fluted spectra were to be observed as well as line spectra.

The accompanying diagram (Fig. 29) of the fluted spectrum of iodine will show the difference between these fluted spectra and the line spectra, on which we have been exclusively occupied up to the present.

We observe that the chief novelty is an absolute rhythm in the spectrum; instead of lines irregularly distributed over the spectrum, we have groups which are beautifully rhythmic in their structure. The next diagram (Fig. 30) shows us the radiation spectrum of a particular molecular grouping of carbon vapour, that also is beautifully rhythmic; the rhythm of each of the elementary flutings exactly resembling that of the iodine.

These observations were among the first to suggest the idea that the same chemical element could have two completely distinct spectra. They were eminently suggestive, for if two, why not many?

In my reference to the "long and short" method of observation I stated that it enabled us to note what happens when a known compound body is decomposed. With ordinary compounds, such as chloride of calcium and so on, one can watch the precise moment at which the compound is broken up—when the calcium begins to come out; and we can then determine the relative amount of dissociation by the number and thickness of the lines of calcium which are produced. Similarly with regard to these flutings we can take iodine vapour, which gives us this fluted spectrum, and we can then increase the temperature suddenly, so that we no longer get the fluted spectrum at all, or we may increase it so gently that the lines of iodine come out one by one in exactly the same way that the lines of calcium came out from the chloride of calcium. We end by destroying the compound of calcium in the one case, and by destroying the fluted spectrum in the other, leaving, as the result in both cases, the bright lines of the constituents—in the one case calcium and chlorine; in the other case iodine itself. I have by no means exhausted the list of difficulties which were gradually presented to us when we considered that both in the sun and in our laboratories spectrum analysis brought before us the results of unique, absolutely similar "chemical atoms." Not only were there differences, but the differences worked in different ways, whether we passed from low to high temperatures in laboratory work, or from the general spectrum or the flame spectrum in the sun.

But I have said enough for my present purpose; details on the points I have referred to and on others must be gone into afterwards.

How then was one to attempt to grapple with these difficulties? Was it the time to found new theories? or to rest and be thankful? Was it not better to appeal to what was known—to proceed in accordance with Newton's laws of philosophising, and start no new principle unless one were absolutely bound to do so: to appeal in fact to the law of continuity, and to suppose that the explanation of a very large part at all events, of this new matter, lay in the fact that, all unconsciously, spectroscopists had been working under more transcendental conditions as regards temperature than had ever been employed before, and that the natural result was that this higher temperature had done for the matter on which they had experimented exactly what all lower temperatures had been found to do. That is to say, that they had been broken up. In other words, it lent great probability to the view that when we subjected, say iron—because it is a good thing to keep to one specific substance—to one of these transcendental temperatures, we were no longer dealing with the spectrum of iron, but with the spectrum of the constituents of iron revealed to us by a temperature at which no experiments had been made before.

And one was the more struck by the probability of this being at all events an approximation to the truth by those stellar spectra to which I have referred, and by the knowledge we possessed, that in the case of a star of the simplest spectrum we were dealing with the highest possible temperature. So the idea was thrown out that these stars were really simpler in their structure; that their immense temperature had not allowed a complex evolution of higher complex forms of chemical matter to take place; and that we had there the primordial germs of matter, so to speak, or at all events something nearer to the beginning of things than anything that we had in this cool planet of ours, or anything that we were likely to find easily here, in consequence of the various difficulties which harass every kind of experimentation. It was imagined that we might picture to ourselves a sort of celestial dissociation in the heavenly bodies which would place those stars, the spectra of which have been seen, in a different order; that the first star with lines should be a star of the simplest spectrum, the next star with lines should be that which mostly resembled our sun, and that the last in order should be that one in which the lined spectrum had utterly disappeared in favour of the fluted spectrum. If this were granted for the stars, why not attach all this to the sun? Because, as has already been mentioned, all these lines which were seen in the spectra of the hottest stars were precisely those lines which were seen most intense in the hottest parts of the sun; and it did really seem as if in that way we could eventually sooner or later—most likely later, for Art is very long—get some light on the subject.

I at once say that this idea which was thrown out in the year 1873 on spectroscopic evidence had been anticipated by the foremost philosopher amongst English chemists of his time; I mean the late Sir Benjamin Brodie.[1] From considerations of a perfectly different kind he had come to the conclusion that our chemical philosophy was not anything like so firmly based as was generally imagined, and that, given a higher temperature, the elementary bodies would cease to be elementary—that the adjective "elementary" applied to them was merely the measure of our inability to dissociate them; and to watch the progress of dissociation when we got them at a temperature at our command. By a stroke of genius he, before anything was known about the chemistry of the sun, went to the sun for that transcendental temperature he was in search of; thus showing that he had an absolutely pure and accurate conception of the whole thing as I believe it to be—but that is anticipating matters. He suggested that the constituents of our elementary bodies might be found in the hottest parts of the solar atmosphere existing as independent forms. The whole merit of that conception therefore is due to Sir Benjamin Brodie, and dates from the year 1867.

Now we can easily understand, seeing that much of the spectroscopic work which had been done up to 1874 had had for its object the connecting—intermingling, so to speak—of solar, stellar, and terrestrial chemistry, that it was not a pleasant thing to find that the path seemed about to be such a very rugged one—that we seemed after all not to be in the light, but in the dark, and the very practical question was, what was to be done? Would it have been wise to have considered, *then*, the whole question of the dissociation of elementary bodies? I think it would not have been wise; the data were insufficient. The true thing to be done was, I think, to endeavour to accumulate a vast number of new facts and then to see what would happen when a sufficiently long base of facts had been obtained. What did we want? We chiefly wanted to settle those questions of the variations of spectra seen in our laboratories, and the variations observed when we passed from the spectrum, say of iron on the earth, to the spectrum of iron in solar spots and storms. The coincidence of lines of different bodies which had been referred to by Ångström and Kirchhoff also required investigation. What more ready means of doing that—what more perfect means were there than those placed at our disposal by photography? Photography has no personal equation, it has no inducement to cook a result either in one direction or the other, and it moreover has this excellent thing about it, that the results can be multiplied a thousandfold and can be recorded in an absolutely easy and safe manner. There were other reasons why photography should be introduced. We see at once that it it was quite easy to introduce the process of purification of the spectra to which I have already drawn attention, by merely comparing a series of photographs; the A, B, C of my diagram (Fig. 26) being represented, say, by iron, cobalt, and nickel, or any other substances. Again, it was quite possible by the use of the electric lamp to very considerably increase the

[1] "Ideal Chemistry." Lecture delivered to the Chemical Society in 1867, republished 1880. (Macmillan).

dispersion which Angström had employed; so that, if impurities had been suggested, there was now a method which has not yet been challenged of getting rid of them. If the dispersion was then insufficient there was nothing to prevent it being made very much more considerable, because a perfect photograph will bear a very considerable amount of magnification.

The diagram (Fig. 31) will show the method of photography that was adopted in this work, and by which the various photographs thrown on the screen were taken. The object was to compare the light of the sun with the light of the vapour in the electric arc of any particular substance that we wished to observe. By means of a heliostat and lens an image of the sun was thrown exactly between the poles of an electric lamp, and the rays diverging from it were collected by a second lens and again brought to a focus, this time on the slit of the spectroscope. The slit was provided with two slides, by means of which either its upper or lower half could be exposed, while the other half was covered. If we wished to take the solar spectrum first, the

Ultra-violet fluting. Blue fluting.

FIG. 30.—Carbon flutings, contrasted with the line-spectra of calcium, iron, aluminium, and other impurities of the poles.

poles were separated so that they might not obstruct the sunlight; the image of the sun was allowed to fall on one-half of the slit, and the plate was exposed. That half of the slit was then covered up and the other half opened (the sunlight being cut off), and the substance volatilised in the electric arc so that its image fell on the open part of the slit. The plate was again exposed, and so the two spectra were obtained, one above the other. In this way then we had, first of all, a spectrum of the sun compared with the spectrum of the particular substance we wished to map. After that we had the long and short lines in the same substance photographed on another plate. After that we had all the substances which might exist as impurities in the first substance—that is to say, all the chemical elements photographed with their lines—their long and short lines, in precisely the same manner; and finally we had a comparison of the substances we wished to photograph, say iron, with a spectrum of every other substance which might contain these impurities. It will be seen therefore that an enormous number of photographs had to be taken. As

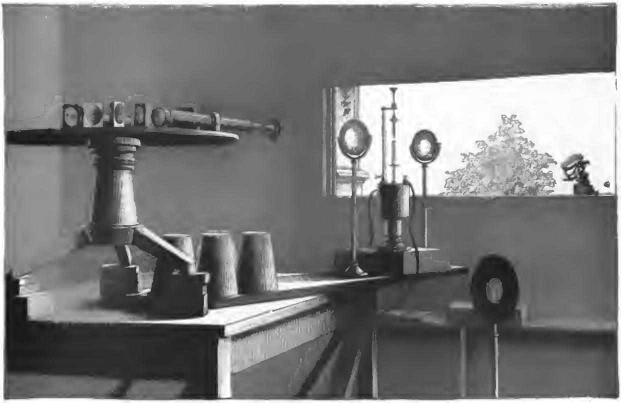


FIG. 31.—Arrangement for photographically determining the coincidence of solar and metallic lines.

a matter of fact three or four thousand photographs have been taken, and a very considerable amount of time (about four years) was consumed in that way.

But it may be said, "Surely if you are going to limit yourself to photography, you will only be dealing with a very small part of the spectrum." My reply to that is that already in the year 1875, when a part of this work had been carried on, other laboratory work had given us reason to believe that what was then being done in photography at the blue end of the spectrum would be done by photography in every other portion, for in fact a spectroscopic study of the behaviour of bodies at low temperature, to which I hope I shall have time to refer, had led several to believe—at all events had led me to believe—that what one got in the text-books about actinism and so on was but a very rough approximation to the truth. We had been taking as the functions of light what were really the functions of the

FINAL REDUCTION—IRON.

Intensity in Sun.	Wave length and length of line.		Coincidences with Short Lines.
1	39 0600	2	U 3, Zr 5, Yt 4
3	0622	4	Va 4
2	0920	3	Va 2, Ba 3
3	1010	4	Va 4, Pt 3
2	1648	2	Co 3
2	1755	3	Mn 3, Ce 4
2	1835	4	Os 2
1	2700	1	Va 2
1	2950	1	Mo 3
3	3023	4	Ce 4
5	3435	4	U 2
3	3475	2	Ba 2, Rh 2
3	3628	3	Ta 3
2	3975	2	Co 3
3	4026	3	Va 5
3	4422	4	Mo 3
3	4720	2	Yt 5, Th 3
2	5012	2	Di 2
2	5160	2	Ce 3, Ru 3
2	5210	3	W 3
2	5423	4	U 3, Mo 3, W 4
3	6215	3	Yt 5, Ce 3, Di 2
2	6571	2	Zr 2
3	6662	2	Th 1
1	7555	3	Os 2, Ta 4, Cr 2
3	7578	4	Di 2
2	7685	2	Va 4
2	8083	2	Ti 1
1	8320	1	Cr 3
3	9520	3	Ru 3
2	9750	2	Mo 3

bodies which received it, and it was therefore quite easy to imagine, and one was justified in hoping that as the work went on we should find, that what one particular kind of substance would do for the blue rays another particular kind of substance would do for the red rays and for the green rays, and so on. Capt. Abney in his lectures will show you that the spectro-

scope was no bad guide in that matter, and, thanks to his valuable researches, we are now able to photograph as well, if not better, at the extreme red end of the spectrum than we did at that time—years ago now—in the blue.

Well, then, four years were consumed in the accumulation of these facts. I do not now intend to call attention to the whole of them, but I will take some instances, directing special attention to what happened with regard to the spectrum of iron. This[1] is the final map produced up to a certain point. We have first the solar spectrum; below this are mapped all the lines of iron observed on one of the photographs which we obtained, including of course all impurities; and then follow the spectra of manganese, cobalt, nickel, chromium, uranium, cerium, and so on through the whole story. When that work had been completed in that manner we had to get rid of the impurities by the process which I have already explained, and at last we got what is called a purified spectrum, in which, along the horizon labelled iron we had only those lines left which we could not by any application of the principle which has been explained be shown to be due to the admixture of any other substance whatever. What then was the total result? The accompanying table (p. 320) will show the sort of corner in which we found ourselves after all this work had been accomplished. It gives the list of the iron lines which, after making every allowance for the existence of impurities, were found to coincide with lines in other substances.

It will be seen, for instance, that the two short lines 390600 and 395423 coincided, the first with short lines in uranium, zirconium, and yttrium, the second with short lines in uranium, molybdenum, and tungsten. Similarly there are two short-line coincidences with zirconium, and no less than six with vanadium, and so on. The total gives the coincidence of the lines of all the elements under the conditions that I have drawn attention to. So that the sum total of this really very laborious inquiry with regard to iron was that in the region between 39 and 40, the region including H and K on that map, where, before the introduction of photography, scarcely any iron lines had been seen, and where only five solar lines I think had been mapped, photography gave us a total of nearly 300 lines in the solar spectrum, and it gave us sixty-two lines of iron.

Of those sixty-two lines of iron only eighteen went straight; by which I mean that the remainder had short-line coincidences with the lines of other substances. So that the idea first thrown out by Kirchhoff, Ångström, and Thalèn of the possibility of the coincidence of lines among the metallic elements was enormously intensified. It will be seen that the thing is absolutely reversed in the case of iron, and it might be the case also in other substances. The fact of a line not being coincident with a line in another substance was the exception, and not the rule. The ratio in the case of iron being as 44 to 18.

It is amusing in the light of recent criticisms to go back to the old observations and to see with what pertinacity for the first two years we stuck to the possibility that the solar line or the iron line we were dealing with was a double line, and then, after we had to give that idea up, as the coincidences became of three, four, five, and sixfold complexity, we came to the conclusion that we were dealing with a common impurity. That of course was a point we could not settle until we had gone through all the chemical elements which were known to us, and it was going through so many substances which took up so much time.

But there was another question which became striking, in this excessively minute anatomy of even a very small portion of the solar spectrum, for I should say that the small range of the spectrum represented here forms a portion of a map which, when completed, will be the sixteenth of a mile long, so that after all we were dealing with an excessively small portion of the total work which had to be done. Having there mapped that small region, where without photography it would have been difficult to see any lines at all, we got in almost twenty cases from one end to the other, instances in which there was absolutely no relationship at all between the brightness of the iron line on our photographs and the darkness of the corresponding solar line.

These were carefully noted as "anomalous reversals," a term we coined in the laboratory at the time, and which we still use, although the word anomalous always suggests a very large amount of ignorance.

In more ways than one, then, this work landed us in rather worse confusion than we were in before. What we had to face was (1) the variation in intensity as we passed from earth to sun, a variation so great that in some cases terrestrial lines were missing in the sun, and in others feeble terrestrial lines were greatly intensified; and (2) the coincidence of lines in several spectra. That is, here and there along the spectrum we found the lines massed as it were even if the coincidence was but apparent, and it really did

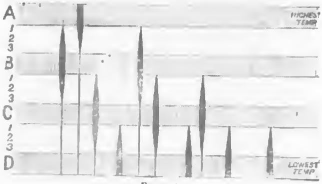


FIG. 32.[1]

seem time to consider what the effect would be, supposing that a dissociation was really going on under our eyes without our knowing or imagining anything about it. Why, it may be said, did you pitch on dissociation? For the reason that the startling results really after all contained nothing that was new—nothing that was novel about them the least in the world, if we regarded them with an absolutely unbiased and receptive mind. Dissociation would undoubtedly account for all the variations of intensity observed on passing from one temperature to another, as already exemplified in the case of the calcium lines, and moreover the short common lines, should they turn out to be truly common, which we were getting in the case of all substances, might be simply the equivalents of those short common lines of calcium which for years past we had watched coming out of the salts of calcium when decomposition was taking place. No new theory was necessary. The appeal to the law of continuity, as I said before, was really open to us, and it seemed to be our duty to appeal to it, and it was also easy to see, before really one has inquired into the matter, that if nature had built up the inorganic world in the way we now know she has built up the organic world, that precisely these facts and none other would be those she would present to us.

"Let us assume a series of furnaces A–D, of which A is the hottest (Fig. 32).

"Let us further assume that in A there exists a substance α, by itself competent to form a compound body β by union with itself, or with something else when the temperature is lowered.

"Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be

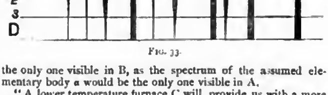


FIG. 33.

the only one visible in B, as the spectrum of the assumed elementary body α would be the only one visible in A.

"A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

[1] This map is too large and detailed to reproduce here.

[1] The figures between the hypothetical spectra point to the gradual change in the interstices of the lines as the spectrum is observed near the temperature of each of the furnaces.

"Now if into the furnace A we throw some of this doubly compounded body γ, we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β; finally α will exist alone, and the spectrum will be reduced to one of the utmost simplicity.

"This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ, and δ all higher, that is, more compound forms of α, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

"The same reasoning therefore which shows how variation in intensity can most naturally explain the short line coincidences—lines which I have termed basic, for the line of α strong in A is *basic* in B, C, and D, the lines of β strong in B are *basic* in C and D, and so on.

"I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into α, is far below that stage, although higher than B.

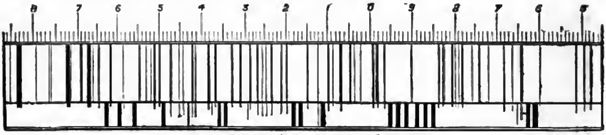


FIG. 34.—Spectrum of sun-spot observed at Greenwich.

"It will be seen from this diagram (Fig. 33) that then the only difference in the spectra of the bodies existing in the four furnaces would consist in the relative thicknesses of the lines. The spectrum of the substances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; *each line would in turn be basic in the whole series of furnaces* instead of in one or two only."

We are therefore completely justified in asking whether these are not the differences in intensities of lines to which Kirchhoff and Ångström have referred, and it is quite easy to see that if we change the temperature of the furnaces in such a manner as to produce the strongest lines, owing to the greatest quantity of the vapour given off at any temperature, that the long lines produced at these different temperatures would vary, and the longest line produced in furnace D would not be the same therefore as the longest line produced in furnace A, so that in that way we can imagine a transcendental temperature giving a very long line to a particular substance, and that substance may exist highly compounded in another substance, and yet at a lower temperature it may only appear as an exceedingly short feeble line.

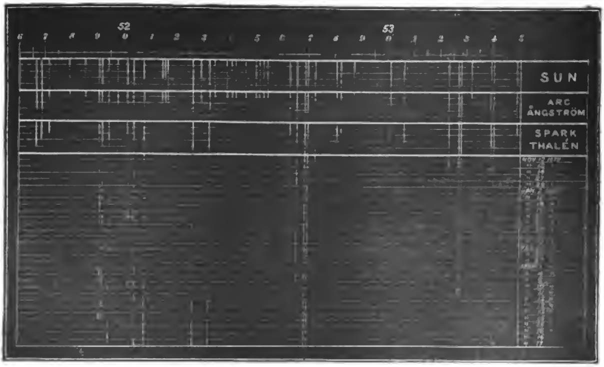


FIG. 35.—Portion of a large map showing the lines most affected in 100 sun-spots observed at South Kensington.

The result of this reasoning was, in short, to explain at once variations of intensity of the short feeble lines which were common to so many of the so-called elementary bodies.

I am particularly anxious to point out that there is absolutely nothing new in these views. We have simply taken as our exemplar the behaviour of a known compound body, and then pushed the reasoning three or four stages further. We have gone just the safest possible way, by the easiest possible stages, from the known to the unknown.

I have now to refer, one by one, to the various tests which have been applied to these considerations, and I should now like to bring the first considerable test under notice. I shall show on a subsequent occasion the various laboratory methods that we possess of determining whether short lines are really the product of high temperature. I shall at once draw your attention to the fact that the short lines may be due, not merely to the work of high temperature, being thus truly produced by the temperature which we are employing, but they may be also the

indications of excessively complex groupings which are just dying at the temperature we are using at the time. So that if it may be permitted to coin terms I should like to call some of the short lines hot-short lines, and others cold-short lines. We shall see the reason by and by.

Now if this order of things is in any way as I have stated it, the first test that we have to employ is one of excessive simplicity. The differences between terrestrial and solar spectra indicate that if the view be correct differences should be seen in the spectra of the same substances observed in different parts of the sun.

We should now have a very distinct notion of the enormous difference of temperature between the highest and lowest reaches of the solar atmosphere. The lowest region of the solar atmosphere that we can get at must be far hotter than the highest part we can get at, at all events in times of eclipses; the lines that we should see therefore in the hottest region of the sun should bring us very near to the effects of this transcendental

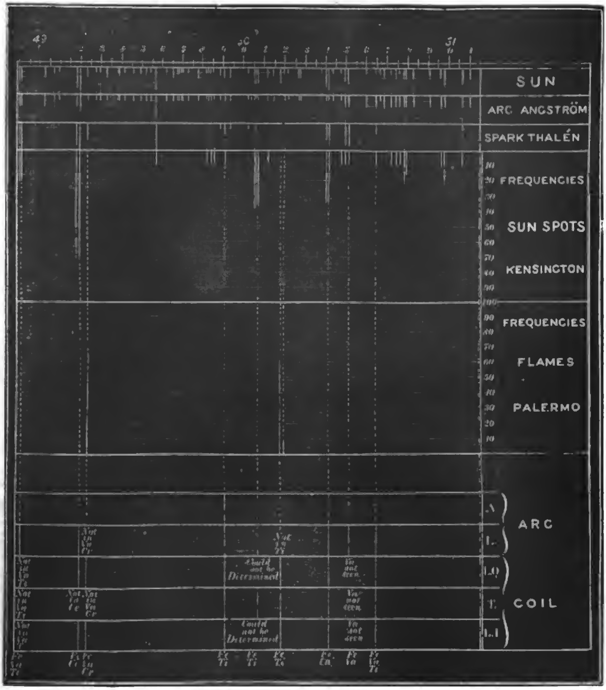


FIG. 36.

temperature to which I have referred, and the spectrum of iron seen in this way should bring us in presence of the result of the highest temperature.

Let us take then the flames as giving us the spectrum of the hottest part of the sun. Where are we to find the record of the coolest part? Now to get to this point we have had naturally to dismiss all the observations which have been made of the lines visible in solar prominences, of the lines thickened in solar spots and the like, because we know that in these prominences and spots we really are dealing with phenomena local to particular and highly heated regions.

Dealing with the whole solar spectrum we know that we are dealing with the whole of the solar atmosphere, however great, however high that atmosphere must be. Therefore we know that the solar atmospheric spectrum, the Fraunhofer spectrum, cannot by any possibility give us what is going on in any particular region—it must naturally be the summation of what is going on in every region where any absorption of any kind

whatever is visible. Therefore as the spectra of prominences and of storms may be stated to be the spectra of the hottest regions of the sun that we can get at in our inquiries. The lines in the solar spectrum affected neither in spots nor flames give us an approach to the cool spectrum we are in search of. We might expect if differences were observable that we should get something like this—

Lines special to prominences	= hottest.
Lines special to spots	= medium.
Lines affected neither in spots nor storms..	= coolest.

How have these views been tested. The first attempt made to get light out of this inquiry was one which simply dealt with a long catalogue of lines observed by Prof. Young in the memorable expedition of his to Mount Sherman, where, at the height of between 8000 and 9000 feet, with perfect weather and admirable instrumental appliances, about a month was employed in getting such a catalogue of lines as had never been got before. But it was found that, although the result of this inquiry was absolutely in harmony with these views, still after all one wanted more facts. Therefore we have endeavoured to get some of the facts here. And the way in which they have been collected is as follows:—During the last two years the spectra of 100 sun-spots have been observed in the observatory here—observed in a new fashion, and for a good reason I think. In this changeable climate it does not do to do as we began by doing—to attempt to observe all the lines acted upon in a solar spot. The excessive complication, and the intense variation of a spot-spectrum from the ordinary solar spectrum, cannot be better shown than by throwing on the screen the spectrum of one of the sun-spots lately observed at Greenwich.

The figure (Fig. 34) shows a limited part of the solar spectrum, and the lines thickened in the spot-spectrum. It will be seen therefore that to tabulate the existence and thickness and intensities of these lines over the whole of the solar spectrum would be a work which it would be difficult to accomplish in a single day, even if the day were absolutely fine. So that was given up in favour of a limited inquiry over a small part of the solar spectrum; limited further by this, that we only get the twelve lines most affected in each spot on each day. In this way we insure a considerable number of absolutely comparable observations, and we can more easily compare the spot results with those which had been obtained in the observation of the brightest lines in prominences, because when we begin to observe lines in the solar prominences one naturally begins by observing the brightest lines first. So that by observing the darkest lines first in the case of spots, one has a fairer comparison.

A diagram (Fig. 35) will show the result of our observations of 100 spots over a very limited part of the solar spectrum. We will begin by the individual observations. We have at the top the iron lines recorded among the Fraunhofer lines; below we have the iron lines recorded as iron lines by Ångström, who used an electric arc. Lower down we have the iron lines recorded by Thalèn, who used the electric spark. It will be seen that there is a very considerable difference in the spectrum of iron as viewed by means of the spark and by means of the arc, and that there is an equal difference between the spectrum of iron in the sun, that is to say, in the whole sun, determined by the Fraunhofer lines, and the spectrum of either the arc or the spark. It is also to be noted that the solar spectrum is more like the spectrum of the arc than the spectrum of the spark.

Since the relative intensities in all these cases are represented by the length of the lines, we have here an opportunity of observing and discussing the accuracy of Kirchhoff's statement that the iron lines in the sun correspond absolutely in intensity with the lines of iron seen in a light source here. It is necessary first of all to see which light source he fixes on, whether the arc or the spark. When this has been done it is found that the statement is really true with regard to neither.

That however is a digression; to proceed with the diagram, descending from this general spectrum of iron which we get by the absorption of the whole atmosphere of the sun independently of the hottest region and the coldest region—descending from the general to the particular—and taking that particular part of the solar atmosphere where the spots produce their phenomena, let us see what are the results in the case of the spots? We have in the vertical lines a record of the lines which are affected in each spot, and each of the spaces included between the horizontal lines represents a particular spot, the date being given on the right hand side; and these 100 lines which we have here represent the phenomena produced by 100 spots. The diagram is a small portion of the larger map. Now the wonderful thing that one is at once struck with is the absolute and complete irregularity of the whole result. There is no continuity among any of these lines. A careful inspection of the diagram shows us that, speaking in a general way, each of these lines is seen in one spot or another absolutely without the other. We have an *inversion* in the intensities of the lines when passing from spot to spot. Whenever we get a line intensified by Thalèn, we miss it in the spots, and, as a rule, what happens is that the spectrum of the spot is not only simpler than the spectrum of the arc, but simpler than the spectrum of the spark.

Now the importance of these statements depends on other statements which we can bring to confront with them. The next diagram shows the observations of 100 prominences observed between the years 1872 and 1876. (The diagram was thrown on the screen.) Prominences exist in a region of the solar atmosphere not very far from that occupied by the spots, but we have already seen that whereas the spots are produced by a downrush of cool material, prominences are produced by an uprush of hot material. Let us see therefore if any change is produced in the phenomena; whether we shall have exactly the same lines from the flames, or the prominences, as we have from the spots; whether we shall get the same information or no.

Here are the facts with respect to Tacchini's observations:—We begin as before with the whole absorption of the sun, Ångström's map, and Thalèn's map. I think you will see a very considerable change; the iron lines (for we are only dealing with iron) most prominent in the prominences are vastly different from the iron lines most thickened in the spots. The difference is shown in the annexed diagram (Fig. 36), which represents those individual observations both of spots and flames treated in a certain way with reference to the discussion. I will at once explain to you what that certain way is. We have, as before, the three data to begin with, and we have treated the sun-spot observations so that the lengths of the lines will represent the number of times they have been seen in 100 sun-spots; the line at wave-length 4919·5, for instance, has been seen seventy-two times; *that* line, in fact, has been seen more than any other; the one at 5005·0 some forty times, and so on; very many lines having been seen less than ten times. In another part of the same diagram we have summarised the individual results obtained from Tacchini's observation of prominences in exactly the same way. The line 5017·5 was seen in 66 prominences out of 100. But why I am particularly anxious to show this diagram is this, that it brings out the perfectly natural fact—for it is the natural fact—that over this region of the spectrum, at all events, no iron lines affected in the spots are visible in the prominences. If we assume that the region occupied by prominences is hotter than the region occupied by spots, that higher region ought to do this work, and it ought to be a work of simplification. Therefore I say it is a perfectly natural result, and not one to be wondered at, that in the spectra of the flames there is no line coincident with any of the lines seen frequently widened in the spots.

Now we have these three solar spectra here which we can compare one with the other. First of all we have the iron spectrum of the sun taken as a whole. Then we have next the spectrum of spots, which we know to be hotter than the sun taken as a whole. Then we have the spectrum of flames, which we know to be hotter than the spots. It will be seen that the story, as it runs from the top of the diagram downwards, is a story of greater simplicity, as it ought to be, and it was explained in the diagram which I exhibited before I began to show these results of absolute hard facts. It will be seen that the simplicity brought about by the reduction of lines actually seen as to number, is accompanied by the appearance of new lines (produced by the transcendental temperatures) in these regions. This first discussion of a large number of spectra and of spots, as compared with storms, is, I submit, in absolute harmony with the view of the dissociation of the elementary bodies by the solar temperature suggested by Sir Benjamin Brodie in 1867, and therefore I may further add that to me, at all events, it is absolutely inexplicable on any other view.

J. Norman Lockyer

(*To be continued.*)

INTERNATIONAL MEDICAL CONGRESS

THIS Congress, which opened by an informal reception at the College of Physicians on Tuesday, has so far been a real success. It has brought together something

like 2500 medical men, no less than 1000 being from abroad, and 500 from the provinces. Indeed, the attendance is more than double that of any previous Congress. Among the distinguished foreigners who attend the Congress are the following:—Dr. Fordyce Barker, New York; Dr. Billings, Washington; Dr. Bigelow, Boston; Professors Brown-Séquard, Paris; Chauveau, Lyons; Donders, Utrecht; Professors Holmgren, Upsala; His, Leipsic; Kölliker, Wurzburg; Klebs, Prague; Loven, Stockholm; Pasteur, Paris; Pflüger, Bonn; Panteleoni, Rome; Von Slawjansky, St. Petersburg; Stokvis, Amsterdam; Virchow, Berlin. A very large concourse of members thronged the rooms of the College on Tuesday, and crowded St. James's Hall yesterday morning, when Sir James Paget delivered the presidential address. The sectional meetings are being held in the rooms of the various scientific societies in the Burlington House region, and there are fifteen of them altogether. Prof. Virchow gave an address last night on "The Value of Pathological Experiments." To-day Prof. Maurice Raynaud gives a general address on "Scepticism in Medicine"; to-morrow Dr. Billings of Washington gives an address on "Our Medical Literature"; and to-morrow night the Lord Mayor and Corporation receive the members in the Guildhall at a *conversazione*. On Saturday there will be several excursions, and Sir Joseph Hooker will hold a reception at Kew in the afternoon. On Monday at a general meeting Prof. Volkmann of Halle will lecture on "Modern Surgery"; and on Tuesday Prof. Huxley will lecture on "The Connection of the Biological Sciences with Medicine." We this week give the opening address of Sir James Paget:—

As I look round this hall my admiration is moved not only by the number and total power of the minds which are here, but by their diversity, a diversity in which I believe they fairly represent the whole of those who are engaged in the cultivation of our science. For here are minds representing the distinctive characters of all the most gifted and most educated nations; characters still distinctly national, in spite of the constantly increasing intercourse of the nations. And from many of these nations we have both elder and younger men; thoughtful men and practical; men of fact and men of imagination; some confident, some sceptic; various, also, in education, in purpose and mode of study, in disposition, and in power. And scarcely less various are the places and all the circumstances in which those who are here have collected and have been using their knowledge. For I think that our calling is pre-eminent in its range of opportunities for scientific study. It is not only that the pure science of human life may match with the largest of the natural sciences in the complexity of its subject-matter; not only that the living human body is, in both its material and its indwelling forces, the most complex thing yet known, but that in our practical duties this most complex thing is presented to us in an almost infinite multiformity. For in practice we are occupied, not with a type and pattern of the human nature, but with all its varieties in all classes of men, of every age and every occupation, and all climates and all social states; we have to study men singly and in multitudes, in poverty and in wealth, in wise and unwise living, in health and all the varieties of disease; and we have to learn, or at least try to learn, the results of all these conditions of life while, in successive generations and in the mingling of families, they are heaped together, confused, and always changing. In every one of all these conditions man, in mind and body, must be studied by us; and every one of them offers some different problems for inquiry and solution. Wherever our duty or our scientific curiosity, or, in happy combination, both, may lead us, there are the materials and there the opportunities for separate original research.

Now, from these various opportunities of study, men are here in Congress. Surely, whatever a multitude and diversity of minds can in a few days do for the promotion of knowledge, may be done here.

But it is not proposed to leave the work of the Congress to what would seem like chances and disorder, good as the result might be; nor yet to the personal influences by which we may all be made fitter for work, though these may be very potent. In the stir and controversy of meetings such as we shall have, there cannot fail to be useful emulation; by the examples that will appear of success in research, many will be moved to more enthusiasm, many to more keen study of the truth; our range of work will be made wider, and we shall gain that greater interest in each other's views and that clearer apprehension of them which are always attained by personal acquaintance and by memories of association in pleasure as well as in work. But as it will not be left to chance, so neither will sentiment have to fulfil the chief duties of the Congress.

Following the good example of our predecessors, certain subjects have been selected which will be chiefly, though not exclusively, discussed, and the discussions are to be in the sections into which we shall soon divide.

Of these subjects it would not be for me to speak even if I were competent to do so; unless I may say that they are so numerous and complete that—together with the opening addresses of the Presidents of Sections—they leave me nothing but such generalities as may seem commonplace. They have been selected, after the custom of former meetings, from the most stirring and practical questions of the day; they are those which must occupy men's minds, and on which there is at this time most reason to expect progress, or even a just decision, from very wide discussion. They will be discussed by those most learned in them, and in many instances by those who have spent months or years in studying them, and who now offer their work for criticism and judgment.

I will only observe that the subjects selected in every section involve questions in the solution of which all the varieties of mind and knowledge of which I have spoken may find their use. For there are questions, not only on many subjects, but in all stages of progress towards settlement. In some the chief need seems to be the collection of facts well observed by many persons. I say by many, not only because many facts are wanted, but because in all difficult research it is well that each apparent fact should be observed by many; for things are not what they appear to each one mind. In that which each man believes that he observes, there is something of himself; and for certainty, even on matters of fact, we often need the agreement of many minds, that the personal element of each may be counteracted. And much more is this necessary in the consideration of the many questions which are to be decided by discussing the several values of admitted facts and of probabilities, and of the conclusions drawn from them. For, on questions such as these minds of all kinds may be well employed. Here there will be occasion even for those which are not unconditionally praiseworthy, such as those that habitually doubt, and those to whom the invention of arguments is more pleasing than the mere search for truth. Nay, we may be able to observe the utility even of error. We may not indeed wish for a prevalence of errors; they are not more desirable than are the crime and misery which evoke charity. And yet in a congress we may palliate them, for we may see how, as we may often read in history, errors, like doubts and contrary pleadings, serve to bring out the truth, to make it express itself in clearest terms and show its whole strength and value. Adversity is an excellent school for truth as well as for virtue.

But that which I would chiefly note, in relation to the great variety of minds which are here, is that it is characteristic of that mental pliancy and readiness for variation which is essential to all scientific progress, and which a great international congress may illustrate and promote. In all the subjects for discussion we look for the attainment of some novelty and change in knowledge or belief; and after every such change there must ensue a change in some of the conditions of thinking and of working. Now, for all these changes minds need to be pliant and quick to adjust themselves. For all progressive science there must be minds that are young whatever may be their age.

Just as the discovery of auscultation brought to us the necessity for a refined cultivation of the sense of hearing, which was before of only the same use in medicine as in the common business of life; or, as the employment of the numerical method in estimating the value of facts required that minds should be able to record and think in ways previously unused; or, as the acceptance of the doctrine of evolution has changed the course of thinking in whole departments of science: so is it, in less measure, in every less advance of knowledge. All such advances change the circumstances of the mental life, and minds that cannot or will not adjust themselves become less useful, or must at least modify their manner of utility. They may continue to

be the best defenders of what is true; they may strengthen and expand the truth, and may apply it in practice with all the advantages of experience; they may thus secure the possessions of science and use them well; but they will not increase them.

It is with minds as with living bodies. One of their chief powers is in their self-adjustment to the varying conditions in which they have to live. Generally those species are the strongest and most abiding that can thrive in the widest range of climate and of food. And of all the races of men they are the mightiest and most noble who are, or by self-adjustment can become, most fit for all the new conditions of existence in which by various changes they may be placed. These are they who prosper in great changes of their social state; who, in successive generations, grow stronger by the production of a population so various that some are fitted to each of all the conditions of material and mode of life which they can discover or invent. These are most prosperous in the highest civilisation; these whom nature adapts to the products of their own arts.

Or, among other groups, the mightiest are those who are strong alike on land and sea; who can explore and colonise, and in every climate can replenish the earth and subdue it; and this not by tenacity or mere robustness, but rather by pliancy and the production of varieties fit to abide and increase in all the various conditions of the world around.

Now it is by no distant analogy that we trace the likeness between these in their successful contests with the material conditions of life and those who are to succeed in the intellectual strife with the difficulties of science and of art. There must be minds which in variety may match with all the varieties of the subject-matters and minds which, at once or in swift succession, can be adjusted to all the increasing and changing modes of thought and work.

Such are the minds we need; or rather, such are the minds we have; and these in great meetings prove and augment their worth. Happily the natural increase in the variety of minds in all cultivated races is—whether as cause or as consequence—nearly proportionate to the increasing variety of knowledge. And it has become proverbial, and is nearly true in science and art, as it is in commerce and in national life, that, whatever work is to be done, men are found or soon produced who are exactly fit to do it.

But it need not be denied that, in the possession of this first and chiefest power for the increase of knowledge, there is a source of weakness. In works done by dissimilar and independent minds, dispersed in different fields of study, or only gathered into self-assorted groups, there is apt to be discord and great waste of power. There is therefore need that the workers should from time to time be brought to some consent and unity of purpose; that they should have opportunity for conference and mutual criticism, for mutual help and the tests of free discussion. This it is which, on the largest scale and most effectually, our Congress may achieve; not indeed by striving after a useless and happily impossible uniformity of mind or method, but by diminishing the lesser evil of waste and discord which is attached to the far greater good of diversity and independence. Now as in numbers and variety the Congress may represent the whole multitude of workers everywhere dispersed, so in its gathering and concord it may represent a common consent that, though we may be far apart and different, yet our work is and shall be essentially one; in all its parts mutually dependent, mutually helpful, in no part complete or self-sufficient. We may thus declare that as we who are many are met to be members of one body, so our work for science shall be one, though manifold; that as we, who are of many nations, will for a time forget our nationalities and will even repress our patriotism, unless for the promotion of a friendly rivalry, so will we in our work, whether here and now or everywhere and always, have one end and one design—the promotion of the whole science and whole art of healing.

It may seem to be a denial of this declaration of unity that, after this general meeting, we shall separate into sections more numerous than in any former Congress. Let me speak of these sections to defend them; for some maintain that, even in such a division of studies as these may encourage, there is a mischievous dispersion of forces. The science of medicine, which used to be praised as one and indivisible, is broken-up, they say, among specialists, who work in conflict rather than in concert, and with mutual distrust more than mutual help.

But let it be observed that the sections which we have instituted are only some of those which are already recognised in many countries, in separate societies, each of which has its own place and rules of self-government and its own literature. And the division has taken place naturally in the course of events which could not be hindered. For the partial separation of medicine, first from the other natural sciences, and now into sections of its own, has been due to the increase of knowledge being far greater than the increase of individual mental power.

I do not doubt that the average mental power constantly increases in the successive generations of all well-trained peoples; but it does not increase so fast as knowledge does, and thus in every science, as well as in our own, a small portion of the whole sum of knowledge has become as much as even a large mind can hold and duly cultivate. Many of us must, for practical life, have a fair acquaintance with many parts of our science, but none can hold it all; and for complete knowledge, or for research, or for safely thinking-out beyond what is known, no one can hope for success unless by limiting himself within the few divisions of the science for which, by nature or by education, he is best fitted. Thus, our division into sections is only an instance of that division of labour which, in every prosperous nation, we see in every field of active life and which is always justified by more work better done.

Moreover, it cannot be said that in any of our sections there is not enough for a full strong mind to do. If any one will doubt this let him try his own strength in the discussions of several of them.

In truth, the fault of specialism is not in narrowness, but in the shallowness and the belief in self-sufficiency with which it is apt to be associated. If the field of any specialty in science be narrow, it can be dug deeply. In science, as in mining, a very narrow shaft, if only it be carried deep enough, may reach the richest stores of wealth and find use for all the appliances of scientific art. Not in medicine alone, but in every department of knowledge, some of the grandest results of research and of learning, broad and deep, are to be found in monographs on subjects that, to the common mind, seemed small and trivial.

And study in a Congress such as this may be a useful remedy for self-sufficiency. Here every group may find a rare occasion, not only for an opportune assertion of the supreme excellence of its own range and mode of study, but for the observation of the work of every other. Each section may show that its own facts must be deemed sure, and that by them every suggestion from without must be tested; but each may learn to doubt every inference of its own which is not consistent with the facts or reasonable beliefs of others; each may observe how much there is in the knowledge of others which should be mingled with its own; and the sum of all may be the wholesome conviction of all, that we cannot justly estimate the value of a doctrine in one part of our science till it has been tried in many or in all.

We were taught this in our schools; and many of us have taught that all the parts of medical science are necessary to the education of the complete practitioner. In the independence of later life some of us seem too ready to believe that the parts we severally choose may be self-sufficient, and that what others are learning cannot much concern us. A fair study of the whole work of the Congress may convince us of the fallacy of this belief. We may see that the test of truth in every part must be in the patient and impartial trial of its adjustment with what is true in every other. All perfect organisations bear this test; all parts of the whole body of scientific truth should be tried by it.

Moreover, I would not, from a scientific point of view, admit any estimate of the comparative importance of the several divisions of our science, however widely they may differ in their present utilities. And this I would think right, not only because my office as president binds me to a strict impartiality and to the claim of freedom of research for all, but because we are very imperfect judges of the whole value of any knowledge, or even of single facts. For every fact in science, wherever gathered, has not only a present value, which we may be able to estimate, but a living and germinal power of which none can guess the issue.

It would be difficult to think of anything that seemed less likely to acquire practical utility than those researches of the few naturalists who, from Leeuwenhoeck to Ehrenberg, studied the most minute of living things, the Vibrionidæ. Men boasting themselves as practical might ask, "What good can come of it?" Time and scientific industry have answered, "This good: those researches have given a more true form to one of the most important practical doctrines of organic chemistry; they have introduced a great beneficial change in the most practical part of

surgery; they are leading to one as great in the practice of medicine; they concern the highest interests of agriculture, and their power is not yet exhausted.

And as practical men were, in this instance, incompetent judges of the value of scientific facts, so were men of science at fault when they missed the discovery of anæsthetics. Year after year the influences of laughing-gas and of ether were shown: the one fell to the level of the wonders displayed by itinerant lecturers, students made fun with the other; they were the merest practical men, men looking for nothing but what might be straightway useful, who made the great discovery which has borne fruit not only in the mitigation of suffering, but in a wide range of physiological science.

The history of science has many similar facts, and they may teach that any man will be both wise and dutiful if he will patiently and thoughtfully do the best he can in the field of work in which, whether by choice or chance, his lot is cast. There let him, at least, search for truth, reflect on it, and record it accurately; let him imitate that accuracy and completeness of which I think we may boast that we have, in the descriptions of the human body, the highest instance yet attained in any branch of knowledge. Truth so recorded cannot remain barren.

In thus speaking of the value of careful observation and records of facts, I seem to be in agreement with the officers of all the sections; for, without any intended consent, they have all proposed such subjects for discussion as can be decided only by well-directed facts and fair direct inductions from them. There are no questions on theories or mere doctrines. This, I am sure, may be ascribed, not to any disregard of the value of good reasoning or of reasonable hypotheses, but partly to the just belief that such things are ill-suited for discussion in large meetings, and partly to the fact that we have no great opponent schools, no great parties named after leaders or leading doctrines about which we are in the habit of disputing. In every section the discussions are to be on definite questions, which, even if they be associated with theory or general doctrines, may yet be soon brought to the test of fact; there is to be no use of doctrinal touchstones.

I am speaking of no science but our own. I do not doubt that in others there is advantage in dogma, or in the guidance of a central organising power, or in divisions and conflicting parties. But in the medical sciences I believe that the existence of parties founded on dominant theories has always been injurious; a sign of satisfaction with plausible errors, or with knowledge which was even for the time imperfect. Such parties used to exist, and the personal histories of their leaders are some of the most attractive parts of the history of medicine: but, although in some instances an enthusiasm for the master-mind may have stirred a few men to unusual industry, yet very soon the disciples seem to have been fascinated by the distinctive doctrine, content to bear its name, and to cease from active scientific work. The dominance of doctrine has promoted the habit of inference, and repressed that of careful observation and induction. It has encouraged that fallacy to which we are all too prone, that we have at length reached an elevated sure position on which we may rest, and only think and guide. In this way specialism in doctrine or in method of study has hindered the progress of science more than the specialism which has attached itself to the study of one organ or of one method of practice. This kind of specialism may enslave inferior minds: the specialism of doctrine can enchant into mere dreaming those that should be strong and alert in the work of free research.

I speak the more earnestly of this because it may be said, if our Congress be representative, as it surely is, may we not legislate? May we not declare some general doctrines which may be used as tests and as guides for future study? We had better not.

The best work of our International Congress is in the clearing and strengthening of the knowledge of realities; in bringing, year after year, all its force of numbers and varieties of minds to press forward the demonstration and diffusion of truth as nearly to completion as may from year to year be possible. Thus, chiefly, our Congress may maintain and invigorate the life of our science. And the progress of science must be as that of life. It sounds well to speak of the temple of science, and of building and crowning the edifice. But the body of science is not as any dead thing of human work, however beautiful; it is as something living, capable of development and a better growth in every part. For, as in all life the attainment of the highest condition is only possible through the timely passing-by of the less good, that it may be replaced by the better, so is it in science. As time passes, that which seemed true and was very good becomes relatively imperfect truth, and the truth more nearly perfect takes its place.

We may read the history of the progress of truth in science as a paleontology. Many things which, as we look far back, appear, like errors, monstrous and uncouth creatures, were, in their time, good and useful, as good as possible. They were the lower and less perfect forms of truth which, amid the floods and stifling atmospheres of error, still survived; and just as each successive condition of the organic world was necessary to the evolution of the next following higher state, so from these were slowly evolved the better forms of truth which we now hold.

This thought of the likeness between the progress of scientific truth and the history of organic life may give us all the better courage in a work which we cannot hope to complete, and in which we see continual, and sometimes disheartening, change. It is, at least, full of comfort to those of us who are growing old. We that can read in memory the history of half a century might look back with shame and deep regret at the imperfections of our early knowledge if we might not be sure that we held, and sometimes helped onward, the best things that were, in their time, possible, and that they were necessary steps to the better present, even as the present is to the still better future. Yes—to the far better future; for there is no course of nature more certain than is the upward progress of science. We may seem to move in circles, but they are the circles of a constantly ascending spiral; we may seem to sway from side to side, but it is only as on a steep ascent which must be climbed in zig-zag.

What may be the knowledge of the future none can guess. If we could conceive a limit to the total sum of mental power which will be possessed by future multitudes of well-instructed men, yet could we not conceive a limit to the discovery of the properties of materials which they will bend to their service. We may find the limit of the power of our unaided limbs and senses; but we cannot guess at a limit to the means by which they may be assisted, or to the invention of instruments which will become only a little more separate from our mental selves than are the outer sense-organs with which we are constructed.

In the certainty of this progress the great question for us is what shall we contribute to it? It will not be easy to match the recent past. The advance of medical knowledge within one's memory is amazing, whether reckoned in the wonders of the science not yet applied, or in practical results in the general lengthening of life, or, which is still better, in the prevention and decrease of pain and misery, and in the increase of working power. I cannot count or recount all that in this time has been done; and I suppose there are very few, if any, who can justly tell whether the progress of medicine has been equal to that of any other great branch of knowledge during the same time. I believe it has been; I know that the same rate of progress cannot be maintained without the constant and wise work of thousands of good intellects; and the mere maintenance of the same rate is not enough, for the rate of the progress of science should constantly increase. That in the last fifty years was at least twice as great as that in the previous fifty. What will it be in the next, or, for a more useful question, what shall we contribute to it?

I have no right to prescribe for more than this week. In this let us do heartily the proper work of the Congress, teaching, learning, discussing, looking for new lines for research, planning for mutual help, forming new friendships. It will be hard work if we will do it well; but we have not met for mere amusement or for recreation, though for that I hope you will find fair provision, and enjoy it the better for the work preceding it.

And when we part let us bear away with us, not only much more knowledge than we came with, but some of the lessons for our conduct in the future which we may learn in reflecting the work of our Congress.

In the number and intensity of the questions brought before us, we may see something of our responsibility. If we could gather into thought the amounts of misery or happiness, of helplessness or of power for work, which may depend on the answers to all the questions that will come before us, this might be a measure of our responsibility. But we cannot count it; let us imagine it; we cannot even in imagination exaggerate it. Let us bear it always in our mind, and remind ourselves that our responsibility will constantly increase. For, as men become in the best sense better educated, and the influence of scientific knowledge on their moral and social state

increases, so among all sciences there is none of which the influence, and therefore the responsibility, will increase more than ours, because none more intimately concerns man's happiness and working power.

But, more clearly in the recollections of the Congress, we may be reminded that in our science there may be, or, rather, there really is, a complete community of interest among men of all nations. On all the questions before us we can differ, discuss, dispute, and stand in earnest rivalry; but all consistently with friendship, all with readiness to wait patiently till more knowledge shall decide which is in the right. Let us resolutely hold to this when we are apart: let our internationality be a clear abiding sentiment, to be, as now, declared and celebrated at appointed times, but never to be forgotten; we may, perhaps, help to gain a new honour for science, if we thus suggest that in many more things, if they were as deeply and dispassionately studied, there might be found the same complete identity of international interests as in ours.

And then, let us always remind ourselves of the nobility of our calling. I dare to claim for it, that among all the sciences, ours, in the pursuit and use of truth, offers the most complete and constant union of those three qualities which have the greatest charm for pure and active minds—novelty, utility, and charity. These three, which are sometimes in so lamentable disunion, as in the attractions of novelty without either utility or charity, are in our researches so combined that, unless by force or wilful wrong, they hardly can be put asunder. And each of them is admirable in its kind. For in every search for truth we can not only exercise curiosity, and have the delight—the really elemental happiness—of watching the unveiling of a mystery, but, on the way to truth, if we look well round us, we shall see that we are passing wonders more than the eye or mind can fully apprehend. And as one of the perfections of nature is that in all her works wonder is harmonised with utility, so is it with our science. In every truth attained there is utility either at hand or among the certainties of the future. And this utility is not selfish: it is not in any degree correlative with money-making; it may generally be estimated in the welfare of others better than in our own. Some of us may indeed make money and grow rich; but many of those that minister even to the follies and vices of mankind can make much more money than we. In all things costly and vainglorious they would far surpass us if we would compete with them. We had better not compete where wealth is the highest evidence of success; we can compete with the world in the nobler ambition of being counted among the learned and the good who strive to make the future better and happier than the past. And to this we shall attain if we will remind ourselves that, as in every pursuit of knowledge there is the charm of novelty, and in every attainment of truth utility, so in every use of it there may be charity. I do not mean only the charity which is in hospitals or in the service of the poor, great as is the privilege of our calling in that we may be its chief ministers; but that wider charity which is practised in a constant sympathy and gentleness, in patience and self-devotion. And it is surely fair to hold that, as in every search for knowledge we may strengthen our intellectual power, so in every practical employment of it we may, if we will, improve our moral nature; we may obey the whole law of Christian love, we may illustrate the highest induction of scientific philanthropy.

Let us, then, resolve to devote ourselves to the promotion of the whole science, art, and charity of medicine. Let this resolve be to us as a vow of brotherhood; and may God held us in our work.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 25.—M. Wurtz in the chair.—The following papers were read:—On the comet *b* of 1881, by M. Mouchez. The result of M. Oudemans' search among the Dutch Colonial Archives in South Africa is that the comet of 1881 is probably not that of 1807, but seen now for the first time.—Determination of the horizontal and lateral flexure and the flexure of the instrumental axis of the meridian circle of Bischoffsheim, by means of new apparatus, by MM. Lœwy and Perigaud.—On the equivalence of quadratic forms, by M. Jordan.—On chlorhydric ether of glycol, by M. Berthelot.—Anthracic vaccination; *résumé* of experiments made at Lambert, near Chartres, to test the method of M. Pasteur, by M. Bouley. The essence of the test consisted in inoculating vaccinated sheep with natural virus (anthracic blood from a sheep which died of the disease) instead of that prepared by processes of culture. The efficacy of the vaccination was fully demonstrated.—On the irreducible covariants of the binary quantic of the eighth order, by Prof. Sylvester.—Parabolic elements of the comet *b* 1881, by M. Bigourdan.—Observations of Schæberle's comet (*c* 1881) at Paris Observatory, by M. Bigourdan; also by MM. Henry.—Considerations on the forces of nature; inadmissibility of the hypothesis proposed by M. Faye to explain the tails of comets, by M. Picard. Whatever the nature of the repulsive force it can only be proportional to masses, not to surfaces, for *ideal* pressure on surfaces only arises from *effective* action on masses. No interposed matter can weaken or arrest its action, for the etherised medium penetrates all bodies. The action is propagated, not successively but instantaneously, being due not to an undulatory motion, but to shocks of etherised atoms and ponderable molecules, like gravitation; hence on a point in motion it is exerted in the same direction as the attraction exercised by the ponderable mass of the sun.—Remarks on the calculation of relative perturbations, according to M. Gyldén's method, by M. Callandreau.—Hemihedral crystals with inclined faces as constant sources of electricity, by MM. Jacques and Pierre Curie. A plate suitably cut in such a crystal and placed between two sheets of tin forms a condenser which becomes charged when it is compressed. The authors give an absolute measure of the quantities of electricity liberated by tourmaline and quartz for a determinate pressure. It is shown how the instrument may serve in comparison of charges and capacities.—Determination of the angular distance of colours, by M. Rosenstiehl. He shows that three colours previously referred to, viz. orange, the third yellow green, and the third blue, have the characters of a triad (that is, mixed in equal intensity, they produce the sensation of white). All the colours which occupy the angles of an inscribed equilateral triangle have the same properties.—Electric stopcock; transformation, transport, and use of energy, by M. Cabanellas.—On the heat of formation of explosives, by MM. Sarrau and Vieille. When an explosive is decomposed the heat liberated is equal (according to thermodynamics) to the excess of the heat of formation of the products over the heat of formation of the explosive. Hence, knowing, in a given case, the heat liberated by decomposition, and the composition of the products of the reaction, the heat of formation may be arrived at. The authors have applied the method to the principal explosives, and will shortly give the results.—Industry of magnesia (continued), by M. Schlœsing. He treats sewage matter with phosphate of magnesia, obtaining the phosphoric acid from natural phosphates of lime, and the magnesia from sea-water or water of salt marshes (it is precipitated by slaked lime). He produces a sort of vermicelli of lime, which gives a porous magnesia, on which the acid liquid acts easily.—On some reactions of morphine and its congeners, by M. Grimaux.—On a new process of vaccination of chicken cholera, by M. Toussaint. He inoculated fowls with blood of rabbits which had died of septicemia (or with matter cultivated from it), and the effects were those of an attenuated virus, which made the fowls refractory to cholera.—On a volcanic breccia capable of being utilised as an agricultural manure, by M. Carnot. The rock (from l'Herault) contains notable amounts of iron, lime, potash, and phosphoric acid.—Boric acid; its existence in salt lakes of the modern period, and in natural saline waters (second note), by M. Dieulafait.—On the extraordinary temperature of July, 1881, by M. Renou. It rose to 38°·4 on the 19th at the Park Observatory, a degree never experienced in Algiers, the Antilles, and Cayenne.

CONTENTS

THURSDAY, AUGUST 11, 1881

VIVISECTION AND MEDICINE

THE International Medical Congress which has met in London during the past week is the largest that the world has ever seen. Medical men have assembled from every part of the earth, and their meetings seem to have been productive of general satisfaction. The objects of such a Congress are twofold—first, to tell or hear of new discoveries; and, second, to make men personally acquainted who have previously been known to each other only through their works. The latter is perhaps the more important of the two, for it is not only a source of very great pleasure, but of great profit, inasmuch as it enables men to form a juster appreciation of the workers in each department of medicine, and to avoid falling into the error, very common at the present day, of placing the observations and opinions of a mere tyro on a level with those of the scientific veteran. The work of the Congress has been divided into no less than fifteen sections, each of which has taken up some special department of the science or practice of medicine. For medicine is now not merely an art. It is no longer practised by simple rule-of-thumb. It is becoming, to some extent, a science, and exact knowledge is beginning to supplant blind empiricism. The means by which this change has been effected have been admirably illustrated in the addresses of Prof. Virchow, Mr. Simon, and Prof. Fraser.

They are those of experiment

It is by experiment alone that we are able to distinguish between facts and fancies, between the ideas which arise in men's minds and the realities of the external world. It is in proportion as we bring our ideas into accordance with facts, or, in other words, as we *know* instead of *supposing*, that our power increases. Suppositions have been the *bête noire* of medicine. They have constantly misled men as to the causes, the nature, and the treatment of disease, and so long as they were not subjected to the test of experiment one supposition succeeded another, only to be itself replaced by a third, no less fanciful and no less delusive. This is the reason why the progress of medicine was formerly so slow, and it is only of recent years, since the experimental method has been employed, that medical knowledge has begun to acquire any exactitude. As Prof. Virchow points out in his address, the principle of modern medicine is localisation. We localise the causes and seats of a disease, we localise the action of remedies, and thus we are able to act with certainty so far as our knowledge will carry us. If we were able to localise certainly and define accurately the causes and seats of disease and the action of our remedies, we should possess a power to arrest or prevent disease which would render death by old age the usual, instead of as at present the exceptional, termination of human life. The experiments by which exact knowledge is obtained are, as Mr. Simon points out in his address, of two kinds. "On the one hand we have the carefully pre-arranged and comparatively few experiments which are done by us in our pathological laboratories, and for the most part on other animals than man; on the other hand, we have the experiments which accident does for us, and, above all, the incalculably large amount of crude experiment which is popularly done by man on man under our present ordinary conditions of social life, and which gives us its results for our interpretation." As an example of these two kinds of experiment, Mr. Simon quotes the classical experiments to which we habitually refer when we think of guarding against the danger of Asiatic cholera: "On the one side there are the well-known *scientific* infection experiments of Prof. Thiersch, and others following him, performed on a certain number of mice; on the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848-49 and 1853-54, were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies which supplied those districts with water."

Popular experiments on the causes of disease are performed everywhere around us. Even when no epidemic prevails, our hospitals are crowded with the sick and dying, and many, very many, of these are dying from lack of knowledge. Probably the most dreaded scourge of this country is pulmonary consumption, or tubercle, as it is sometimes shortly termed, from a pathological product found in the lungs in this disease. This fearful malady seems often to attack the most beautiful and the most gifted. We have hospitals established especially for its treatment, and these institutions are crowded to the door, applicants having to wait weeks, perhaps months, before they can obtain admission. Hitherto we have been accustomed to regard this dreadful disease as one which we had no power to guard against, and whose attacks were no more to be averted than the stroke of a thunderbolt. But increased knowledge has already shown us how to avoid or prevent to a great extent the danger which we might otherwise incur from the lightning-flash, and increased knowledge is now showing us the causes which may induce consumption, and thus teaching us how to avoid them. By experiment upon animals we are learning the nature of the morbid processes which occur in this disease, and the conditions which give rise to them. We are learning that tuberculosis in cows may be communicated to healthy animals fed upon the milk which they yield, and that tubercular disease may also be induced by tubercular matter inhaled in the air or conveyed into the stomach. In these experiments upon animals we are simply repeating in a scientific way the popular experiments which men daily make in blind ignorance upon men. We communicate to a few animals a disease of which men perish by thousands, and by the sacrifice of a few dogs or rabbits we gain knowledge which may enable us to preserve the lives of thousands of men, and avert the anguish which their untimely death would cause to their relations.

In the out-patient departments of our general hospitals there are probably no cases more trying to the humane physician than the cases of consumption which he sees. Racked by cough and worn to a shadow as they often are, the physician knows that he can do but little for them if they are admitted. The utmost that his art is capable of is somewhat to alleviate their sufferings, and perhaps slightly to prolong a comparatively useless life. For these reasons he is often obliged to sacrifice his own feelings, and to refuse admission to the sufferer,

knowing that such an act of apparent charity would be real cruelty to others. By putting out of sight for a moment the fact that the number of beds in the hospitals is necessarily limited, and admitting such a consumptive patient, he would gratify his own feelings of kindness and benevolence, but would also exclude the young and strong who suffer from such acute diseases as inflammation of the heart, lungs, or kidneys, diseases which by proper care and attention in the hospital might, and very probably would, rapidly run a favourable course, and result in the patient's restoration to his family in health and strength, but which if left to themselves might damage the constitution of the sufferer and make him a burden on society, or quickly carry him off, leaving his wife a widow and his children fatherless. Although the wistful looks and earnest entreaties of the consumptive patient might lead some few morbidly sensitive and unreflecting persons to open the gate of the hospital to him rather than to the strongly-built and apathetic labourer whose life was in hourly peril from acute disease, yet most people would, in all probability, have little difficulty in deciding between the two cases, were they to apply for admission at the same time. But the case is different when the consumptive is refused, not because the other is already there, but because we know that in the ordinary course of events he must needs come. Here we are forced to disregard the promptings of sympathy with the case before us, and to do that which gives us present pain in order that we may achieve a higher though future good.

Now what occurs daily in the treatment of patients in hospitals, occurs also in the investigation of disease. In order to prevent the suffering, misery, and death of human beings, it is necessary that animals should be sacrificed, and that we should not allow ourselves, for the momentary gratification of those sentimental feelings which would lead us to avoid inflicting even slight and transitory pain upon animals, to neglect the acquirement of that knowledge which will be productive of lasting and widespread benefit to mankind. Many of those consumptive patients probably owe their weary days, their sleepless nights, and their shortened lives to popular experiments, experiments which have been made upon them just as they might have been made upon animals in the laboratory; but they have been made for a different purpose, for the purpose of gain—gain of money, and not of knowledge. These patients may have been supplied with milk from tubercular cows, because it was more profitable for the owners of the dairy to continue milking such animals than to destroy them. Such popular experiments may be carried on for many years without leading to any knowledge of their results, because the conditions under which their subjects live are so complex that it is very hard to ascertain which one of them is the cause of disease. And all this time the unfortunate sufferers from such experiments are suffering and dying for lack of the knowledge which might be acquired by a few experiments on animals in a laboratory. For in experiments n the laboratory the conditions are much more simple, and it is by such experiments on a small number of animals, instead of on an enormous number of human beings, that it has been ascertained that the milk of tuberculous animals is dangerous, and that the seeds of tubercle may be sown in the organism by its use. By similar experiments on a small number of animals in the laboratory we are now learning that many diseases are due to minute organisms, which we can cultivate at will under definite conditions, ascertaining their mode of growth and the influences which modify it. By such experiments M. Pasteur and others have found that these organisms may have their virulence so modified that they can be inoculated harmlessly, and that these inoculations will protect the animal against the virulent form, just as vaccination will protect against small-pox. It is only by an accurate knowledge of the causes of disease that we can hope to prevent its occurrence, and it is only by an accurate knowledge of its nature and seat, and of the action of drugs, that we can hope to cure it when it is present. The seat of disease may be determined without experiment upon animals, for, after the death of the patient, a post-mortem examination will show what parts of the body have been affected. But the alterations which we find in the dead body are only the results of disease. They are no more the disease itself than a field strewn with slain is a battle. As Prof. Virchow remarks in his address, disease presupposes life. In the dead body there is no disease; with death, life and disease disappear simultaneously. It is only in the living body that we can investigate the process of disease, and it is by experiments upon living animals that such exact knowledge of disease as we already possess has been acquired. Without the aid of experiment we are able to ascertain even less regarding the action of drugs than regarding disease, for the most powerful drugs will profoundly alter all the functions of life, and may, indeed, kill almost as rapidly as the lightning-flash, without leaving any visible trace behind to guide us to the seat of action. It is only by experiment upon living creatures that we can ascertain the action of a drug. Formerly, physicians were accustomed to make these experiments upon their patients, "pouring," as Voltaire has said, "drugs of which they knew little into bodies of which they knew less." Nor could they do otherwise. They were called upon to render assistance to their patients, and in their ignorance they did what they could; but instead of being guided by the lamp of knowledge, they followed the *ignis fatuus* of their own imaginations. As Prof. Fraser points out in his address before the Section of Pharmacology, fanciful resemblances between medicines and parts of the body, healthy or diseased, were supposed to show the organs which the medicines particularly affected, and the diseases in which they would be useful. For example, the white spots on the leaf of a plant were supposed to indicate that it would be useful in consumption, because in that disease white spots are found in the lungs. The carrot was employed in jaundice, because the plant and the patient were alike—yellow; and fruits were given in diseases of the heart or kidneys for no better reason than that they resembled these organs in shape. We now laugh at the wildness of these fancies, but we are justified in doing so only because they have been proved by experiment to be foolish. The experiments which proved this have mostly been made by giving drugs to large numbers of human beings, patient after patient being treated in the same way, until the inefficacy of the drug became so apparent that its use was finally abandoned. But while physicians

were thus blindly groping after the truth, their patients were suffering or dying. The doctors might *think*, perhaps, that some other treatment would have been more beneficial than the one they adopted, but they did not *know* it, and they were obliged to act according to the best of their belief. They were forced by the circumstances in which they were placed to perform what Mr. Simon terms a "popular" experiment instead of a scientific one, and the complicated conditions under which it was performed rendered it doubtful how much of the result was due to the drug and how much to the disease, so that a conclusion could only be arrived at after an immense number of trials. The method by which pharmacology is now studied is entirely different. Instead of first giving the medicine to a patient labouring under disease, the effect of any new drug is tested upon plants, such as algæ and fungi, and upon the lower animals, such as frogs and rabbits, and its mode of action is then exactly ascertained by means of experiment upon animals, so that before giving it to a human being we not only know what organs and structures in his body will be affected by it, but, to a great extent, *how* they will be affected, and consequently what changes will be produced in the course of the disease for which we administer it. Instead, therefore, of acting blindfold, we are able, almost with certainty, to relieve where we should formerly have been powerless, and to prevent suffering even when we cannot save life. The key-note of the present medical congress, struck by Prof. Virchow in his address, is the absolute necessity of experimentation upon living beings for the progress of medical science. Without experiment we can have no certain knowledge, and without knowledge we have no power to cure and prevent disease and death. Experiment there must be, and the only question is, Upon what living beings are the experiments to be performed, and how are they to be performed? Are they to be popular experiments, such as those to which Mr. Simon alludes, blindly made upon hundreds or thousands of human beings, healthy or diseased, or are they to be made upon a few animals in laboratories? The idea of inflicting pain upon animals is naturally repugnant to every well-regulated mind, and the thought that they are preventing unnecessary suffering is probably one of the greatest pleasures that tender-hearted and sensitive persons can experience. But this pleasure may be purchased too dearly, and by preventing the infliction of a certain amount of suffering upon a few animals a much greater amount of suffering may be caused to thousands of men.

Vivid pictures have been drawn of the suffering of animals in a physiological laboratory, and, misled by these, great numbers of people have been induced to join in the agitation, and consequent legislation, against vivisection, forgetting entirely that the pain inflicted in a vivisection experiment, except in the very rarest instances, is far exceeded, both in intensity and duration, by the sufferings of very many human beings in the course of a mortal disease, and of almost all animals except those slaughtered by man or killed and eaten by other animals. Every winter hundreds and thousands of birds and beasts die of cold and hunger, and hunger and thirst must almost always hasten the death of all wild animals. Sometimes they starve simply because no food is to be obtained; but the result is the same if weakness or disease renders them unable to reach it, although it may be plentiful around them. For while the death-beds of men are usually soothed by the kindness of the friends who moisten the parched lips and administer such nourishment as the sufferer can take, animals dying from old age, weakness, or disease have no such alleviations to their sufferings. The experiments of Chossat on starvation are generally quoted as the most cruel ever performed in a physiological laboratory, and yet they were only repetitions, on an exceedingly small scale, of the experiments which are constantly being performed by the conditions of life on thousands or millions of wild animals throughout the world. The animals on which Chossat experimented did not suffer more pain than those which die in the fields or forests because their death was witnessed by an observer who utilised it to gain knowledge of great importance to man, while the sufferings of their wild companions were unseen by any human eye. Yet many people seem to think that this is the case, and that the mere fact that pain is inflicted for a beneficial purpose renders it much less endurable than if it were simply inflicted thoughtlessly or in sport. More pain is caused by the whip of a London cab-driver in one day than is inflicted in any physiological laboratory in this country in the course of weeks; and the householder who puts down a pot of phosphorous paste to poison the rats which plague him inflicts upon them a more painful death than any they would be likely to suffer at the hands of a vivisectionist. Within the last few years those who experiment upon animals have been frequently and unjustly abused for their endeavours to gain the knowledge necessary to relieve pain and cure disease. They have, however, followed the example of their great master, Harvey, who held that to "return evil-speaking with evil-speaking" was "unworthy in a philosopher and searcher after truth," and have, like him, believed that they "would do better and more advisedly to meet so many indications of ill-breeding with the light of faithful and conclusive observation." They have, indeed, submitted to legislation which was felt to be unjust, inasmuch as it was directed against abuses which were not shown to exist, and which has already been found to hamper greatly the progress of experimental investigation in this country. Confident in their sense of the necessity for experiment, and feeling assured that ere long every one capable of forming a correct opinion and willing to take the trouble of ascertaining the facts for himself would perceive the necessity, they have remained silent, though assailed, like Harvey, with opprobrious epithets. Now, however, when the opponents of vivisection are exerting all their efforts to render legislation, already sufficiently oppressive, entirely prohibitory, the medical profession has spoken out, and with no uncertain voice, and has declared that experiments upon animals are absolutely necessary. Nor could medical men do otherwise. For no man can practise the medical profession without having occasionally to suffer most acutely on account of the imperfection of his knowledge. Often and often is his heart saddened by his patient's asking, with feeble voice and wistful eye, for the relief which he is powerless to give, and again and again has he to avert his face and to shake his head when, with agonised voices, the friends around the dying sufferer cry to him, "Oh, doctor, can nothing more

be done?" He sees his patients dying around him for lack of the knowledge which can only be obtained by experiment, and cannot but demand that the right to perform such experiments should be conceded to those who have qualified themselves for the task. There are those who say that, instead of trying experiments on the lower animals, medical men should experiment upon themselves; but, as Prof. Virchow points out, "Medical men are already more exposed in epidemics of all kinds in the performance of their duties in hospitals, in the country, in their nocturnal visits to the sick, in operations and necropsies, than any other class of the community as a rule; and it requires all the blindness of the animal fanatics to require also of them that they should test on their own bodies the remedial, or poisonous, or indifferent action of unknown substances, or that they should determine the limit of permissible doses by observations made on themselves." Nor is this all. Medical men do make experiments upon themselves, and some have sacrificed their own lives in such experiments. But such a method of observation is open to the objection that the sacrifice is to a great extent useless, as the death of the experimenter deprives him of the opportunity of recording the results of his experiment. Not only has the necessity for experimentation upon animals been clearly pointed out in the addresses delivered at the Congress, but this International Medical Congress itself, the greatest assembly of men qualified to judge in the matter that has ever been held, has expressed its judgment in the resolution passed, without a single dissentient, at its concluding general meeting:—

"That this Congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable for its future progress; and accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion that, alike in the interests of man and of animals, it is not desirable to restrict competent persons in the performance of such experiments."

THE BIBLE AND SCIENCE

The Bible and Science. By T. Lauder Brunton, M.D., D.Sc., F.R.S., &c. (London: Macmillan and Co., 1881.)

THIS work is in the form of seventeen lectures, which appear to have been delivered before an orthodox audience. Their scope is a wide one, ranging from sketches of ancient Egyptian and Israelitish life to the newest results in biological science. The principal object of the book is professedly that of showing how Darwinism is not antagonistic to Christian belief in general, or to the Mosaic account of creation in particular. But although this is the peg, so to speak, on which the course of lectures is made to hang, occasion is taken to devote the main part of the work to rendering in a plain and popular form an epitome of the leading facts of animal and vegetable morphology. This part of the work is admirably done. Indeed we do not know any writings of this nature better calculated to accomplish their object of making science easy to the general reader; and as the spirit is throughout tender, not to say sympathetic, towards traditional beliefs, the book deserves a large circulation among the always increasing class of persons who desire to learn, with a small amount of trouble and without fear of stumbling upon any cloven hoof, what biological science has done, is doing, and is likely to do. In a word, this part of the book, besides being written in a very graceful style, well exemplifies the truth that no writer is so able to serve up to the general public the facts of science in a palatable form as one who is himself a practical worker in the subjects which he expounds. In the interests of scientific education, therefore, we should like to see "The Bible and Science" pass through any number of editions.

Coming now to what is professedly the main object of the work, opinions of course will differ as to the success which has attended Dr. Brunton's efforts. And here it may be observed, first of all, that it is not very clear what the author himself thinks about the deeper topics that underlie his expositions. Apparently addressing an audience of the straitest sect, he judiciously steers clear of all topics save the one immediately before him, *i.e.* showing that the doctrine of evolution is not incompatible with that of the Mosaic cosmology; and although this is perhaps more effectively done than by many previous essayists, there is nothing to show that he is not adopting the method of St. Paul, which he commends, who "graduated his instructions to the people whom he was addressing, first giving them milk, and afterwards strong meat" (p. 358). Of course in this there is nothing to find fault. Because a man sticks to a text which does not happen to contain a confession of faith, we have no reason to object that he does not publish his religious opinions; only we think it well to point out that such is here the case, for any reader who is careless or obtuse might fail to perceive the adroitness with which Dr. Brunton steers his discussion among the rocks of dogma. At every point where we feel inclined to ask what our author himself believes, we virtually fall into a dialogue with him such as that with which is told of another eminent man—"What is your own creed?" "The creed of all sensible men." "And what is that?" "Sensible men never say."

But whatever Dr. Brunton's creed may be, his book everywhere breathes with such a genuine, and indeed we may say pathetic, appreciation of the beauty of the biblical writings and the nobility of religious belief, that if he fails to strike a chord which through all changes and chances is ever ready to vibrate deep down in the bass of human nature, we have only to commiserate the reader who has departed so far from the best and the purest of human emotions. Having travelled through Palestine, and knowing his Bible as thoroughly as his science, Dr. Brunton gives us some beautiful little sketches of Bible scenes, lighted up by numberless interesting suggestions derived from modern science, as well as by the glow of a singularly vivid imagination. Take, for instance, the following:—

"Never in my life do I remember a pleasanter moment than when I sat down on one of these, and looked at the scene before me, for this was the realisation of my childhood's dream; this was the spot where Joseph had lived. Yonder might have been the granaries where he received his brothers; here, in the neighbourhood, stood his house, where he returned, weary of his day's work, and was received by his lovely and loving wife Asenath,

whose gentle care had obliterated from his mind, not only all the sorrows and trials of his early life, the hatred of his brothers, his slavery in Egypt, his temptations in Potiphar's house, and his long imprisonment in the dungeon, but had almost made him forget his dead mother, the kind old father who had loved him so well, and the little brother Benjamin to whom he had been so deeply attached, so that he called the name of his first-born son Manasseh, 'For God,' said he, 'hath made me forget all my toil, and all my father's house.' . . . Let us, in order to form an idea of the country, suppose Joseph at this time of the year to be starting on a tour of inspection, and let us in thought accompany him.

"He has said farewell to his wife and children. His chariot and horses are at the gate, he springs up, and, accompanied by his attendants, drives onward towards the southern point of the Delta, just where it joins the Nile valley. At first he proceeds amongst shady trees, bounded on either side by fertile gardens; but as he rides on, his path lies through a strip of hard sandy desert, in crossing which the hind legs of one of the horses ridden by his attendants suddenly becomes paralysed, the animal sinks upon its haunches, and the horseman falls backwards. The Cerastes, or horned snake, a little viper only about a foot long, lying concealed in the sand, which it resembles in colour, irritated by the passage of the cavalcade, has bitten the horse's heel. Immediately the poison spreads up the leg, paralysing it, and, when it reaches the spinal cord, paralyses it also, thus destroying the power of both hind legs, and causing them to give way under the weight of the animal. Only within the last year or two have we learned the exact manner in which such a poison as this acts upon the body; but centuries ago its general effect was well known, and no more vivid description of it could be given than that of the dying Jacob, who compared his son Dan to 'an adder in the way, a serpent in the path, biting the horse's heels, so that the rider falleth backwards.'"

In a similarly picturesque manner we are carried through sundry scenes of early Egyptian life, of the bondage of the Israelites, their exodus, wanderings, and conquest of Palestine. In the course of this exposition, which only errs from being too short, several interesting suggestions are made as to the possible origin of the accounts of some of the Pentateuch miracles. Thus, speaking of the plagues, he says:—

"Amongst these was one that used to puzzle me not a little, the plague of 'darkness which might be felt.' Why, thought I, did all the people remain in the dwellings? Why could they not take lanterns with them and move out? But a day which I spent at Port Said showed me what was probably the reason. On waking in the morning it seemed to me that everything had been turned into pea-soup. Above, around, and on every side, was a thick yellow mist, darkening the air like a London fog, but differing from it in this respect, that it was a darkness perceptible; a darkness that might be felt, and painfully felt too, for it was caused by a storm of sand, driven by the wind, and every particle stinging the skin like a needle."

Again, regarding the passage of the Jordan, he writes:—

"One of the puzzles of my childhood's days was to imagine the condition of the waters thus cut off, for I fancied to myself the River Jordan like such streams as I had been accustomed to, flowing through a small channel with level meadows stretching on either side. How then, I thought, did the waters stand up as in a heap? I could picture to myself a steep, glassy wall of water running across the channel itself, but was there likewise a level wall along each bank, or did the waters flow over the meadows on either side? On seeing the Jordan, however, I at once discovered the solution of my childhood's difficulty."

Then, after describing the double channel of the river—

"Within this larger or outer channel, confined by its bank on either side, the waters of the river might become filled up as a heap. Here was an answer to one inquiry of childhood. There were no invisible or glassy walls, indeed, at the sides to prevent the waters from running over the surrounding country. Was there, then, one to draw them up in their channel, and thus to cut them off towards the Dead Sea? or was the dam here simply of earth? On standing at the river's brink, the whole scene appeared to pass before me. The country around is highly volcanic. Earthquakes occur with great frequency, and during such convulsions of nature we know that the relations of land and water become greatly altered. . . . Here, I thought, we have a method by which the Israelites were able to pass over dryshod. If the bed of the stream at this place underwent a sudden upheaval at the time of their passage, the consequences would be exactly those which are described in the Book of Joshua. The waters would rise up like a heap, filling the channel far up the valley, and those flowing down to the Dead Sea would be cut off.

"To some this explanation may seem mere fancy, but it appears to be the one accepted by the psalmist, for in the 114th Psalm we find, 'Jordan was driven back. The mountains skipped like rams, and the little hills like lambs. What ailed thee, O thou sea, that thou fleddest? thou Jordan, that thou wast driven back? Ye mountains, that ye skipped like rams; and ye little hills, like lambs? Tremble, thou earth, at the presence of the Lord, at the presence of the God of Jacob.' Here the psalmist seems to ask the question why Jordan was driven back, and to give us indirectly as an answer that the earth trembled, or, in other words, that there was an earthquake."

Dr. Brunton seems rather fond of this naturalistic or rationalistic method of explaining the miraculous element in the Old Testament records; but it is evident that the method only serves to let in miracles at the back-door instead of at the front. In this case, for instance, we cannot suppose Joshua to have known that an earthquake was about to take place, or, if he did, that its effect would be to divert the course of the river in the way that Dr. Brunton imagines. (There is a possibility, however, in the subsequent instance with which Dr. Brunton deals, of Joshua commanding the sun and moon to stand still, or become "dumb," that he expected an eclipse, and made good capital of his knowledge.) Therefore we must attribute the occurrence of the earthquake at the moment when the tribes were ready to pass over the river as due to a lucky coincidence which in itself would have been little short of miraculous. And the multiplication of such coincidences that would be required to explain all the Pentateuch miracles by this method would render their occurrence unaccountable save on the hypothesis of a designing mind; and this would constitute them miraculous in the sense of being supernatural. Moreover, many of the miracles cannot possibly be met even by the hypothesis of coincidence. Thus the passage through the Red Sea, which is so analogous to the passage through the Jordan, cannot be thus met. Here no earthquake could have produced the effect described, and if we accept the record as history we are compelled to "imagine the waters standing up as in a heap," with all the difficulty of "picturing a steep, glassy wall of

water," &c. We therefore question whether the theory which led, as Dr. Brunton tells us, to the "puzzles" of his "childhood's days," was really more beset with "difficulty" than the one whereby he now endeavours to make his "Bible" square with his "Science." Better swallow miracles in the lump, and so obtain at least consistency, than try to save the historical accuracy of the Pentateuch by playing hide-and-seek with scientific principles, with the result of always losing the game.

The closing chapters of the book are occupied with an endeavour to make evolution acceptable to the orthodox mind. Here we wonder that no mention is made of the circumstance that the order in which the flora and fauna are said by the Mosaic account to have appeared upon the earth corresponds with that which the theory of evolution requires and the evidence of geology proves. On the other hand there are some original ideas which may be found of use among Churchmen of Broad Church proclivities. Thus, after quoting Milton's account of Adam and Eve in Paradise, Dr. Brunton says:—

"This is a very beautiful picture, but it is not at all the one given in Genesis, for there we find that man, after the fall, was a being in the condition of savages of the Stone Age of Europe, clad in skins, and tilling the ground with implements of wood or stone, the use of metals being unknown till generations afterwards. And yet this being, low in the scale as we would term him, is represented as being so much higher in wisdom than Adam before the fall, that he was reckoned almost as a God in comparison, for in Genesis iii. 22, we read that 'The Lord said Behold! the man is become as one of us, to know good and evil.' So that while the Miltonic account of primitive man is an absolute contradiction of the notions of evolution, the Mosaic account is in conformity with them."

Obviously, enough allowance is not here made for what Mr. Darwin would call the "changed conditions of life" which befell Adam and Eve on being turned out of Paradise; the curse so materially altered their "environment" that, as our other apostle of evolution would say, they were no longer "in harmony" with it. Surely, then, Milton was right in representing Adam and Eve in Paradise, not as worse than "savages of the Stone Age of Europe," but rather as a happy and innocent pair living in the midst of plenty, and having access to certain trees which presented physiological properties of so remarkable a character that we greatly wish Dr. Brunton, with his well-known ability in this line of inquiry, could find an opportunity of making them the subject of his next experimental research.

Less open to criticism is the following:—

"Now it is very remarkable that the doctrine of evolution, be it true or no, exactly agrees with the Mosaic account in reference to the place where man was created, whether this creation took place by special act or by evolutionary process. It took place in a paradise, where the air was balmy, where fruit-trees were plentiful, and where there were no carnivorous animals to prey upon and attack man. For man differs from the lower animals in the absence of a furry or hairy coat (although, curiously enough, such a coat is possessed by unborn children). Now, if for a moment we suppose ourselves driven to conclude that, in respect of his physical nature, man was evolved from a lower type of life, he could not have lost his hairy coat unless the air had been soft and balmy; for the essence of the doctrine is that the fittest only survive, and the fittest to survive exposure to heat or cold would not have been the naked, but the hairy individuals. Had not food been abundant and easily masticated, like the fruit of trees, man would not have lost the projecting muzzle and larger jaws of the apes, as a small jaw would be less fitted for the mastication of hard and innutritious food. Had man been liable to the attacks of wild beasts in this paradise, he could not have lost the large canines which form such powerful implements of defence in the gorilla. Nor would he have remained so long helpless, and unable to take care of himself, unless in such a paradise as we have supposed, where all the conditions of life were favourable. The children, which were long in developing, would have been at a disadvantage in the struggle for existence; they would have died off; and the progenitors of the human race could never have developed into men.

"The site, too, of the paradise, according to the evolution theory, agrees exactly with that indicated in the Book of Genesis, and, indeed, until I saw a map by Haeckel, the most prominent defender of the evolution theory in Germany, I was puzzled to understand the Mosaic account. It reads thus [see Genesis]. The site thus indicated with the utmost precision by Moses is perhaps the only one upon the surface of the whole earth which fulfils the demands of the doctrine of evolution. For, as we have already seen, according to this doctrine man must have been developed in a genial climate, in a spot where abundance of food existed. Now such a place might perhaps be found in a similar latitude in America, but it is agreed by all evolutionists that man could not possibly have been developed in the new world, because his affinities are altogether with the monkeys of the old world, and not with those of the new. This is the only point, too, from which man could have spread in such a way as would agree with the distribution of races which we now find.

"But man did not always continue to live in this paradise. He was driven out; according to the theory of evolution, he was probably forced to migrate from this sacred spot for the same reason that races have been forced to migrate ever since, namely, want of food due to increasing numbers. These increasing numbers would, first of all, consume the natural fruits of the trees; they would then be forced to till the ground, and, finally, some of them would be obliged to leave altogether. We read in Genesis that the woman was cursed in her conception being multiplied, and that the man was cursed by having to till the ground by the sweat of his brow. While in paradise he was naked, but after he left it he wore coats of skin. He had not yet learnt the use of metals, and his tools and implements must have been those of wood and stone. For, according to Genesis, it was not until several generations afterwards that Tubal-Cain taught men the use of brass and iron.

"However man was formed, then, the Mosaic account corresponds with what we find in the progress of civilisation—the Stone Age precedes that of Bronze and Iron. The paradise whose locality was indicated by Moses has now disappeared beneath the waters of the Indian Ocean. Whether its disappearance was preceded by some great volcanic eruption or not, and whether such an eruption is referred to in the mention of the flaming sword which turned every way, we cannot tell; but we have no indication in Genesis of the submergence of paradise until the time of the Flood, which," in accordance with Hugh Miller's idea, is supposed by our author to have been due to a subsidence of the land.

We have quoted this passage at length, because it serves to suggest that "the grand old legend" may contain in its beautiful allegory more of traditional history than the present age is always inclined to suppose. Enough has now been said to indicate the general nature of "The Bible and Science," although it may be added that it is fur-

nished with an excellent index. It is an entertaining and instructive book, and we wish it all success.

GEORGE J. ROMANES

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Thought-Reading

BY the courtesy of Dr. G. M. Beard of New York I had the opportunity of witnessing some interesting experiments in artificial trance performed on one of his trained patients, thought-reading being one of the phases exhibited. After his discovering objects in the usual way, I used a fine copper wire about a yard in length. I wound one end round the right hand of the patient (after he was hypnotised) and then placed his wired hand against his forehead. The patient then wandered round the room in an aimless sort of manner, the wire all the time being quite slack, but the moment I attempted, however gently, to increase the tension just sufficient for him to feel it, he instantly moved off along the direction of the wire, like a horse with a rein. I subsequently tried a thicker wire. The patient stood with his face in a direction at right angles to my own; he moved straight towards the table on my left hand, and after oscillating his head sideways as if trying to find some particular spot, he finally brought his forehead slowly but with great accuracy down upon a metal disk about 1½ inches in diameter, and at a distance of about 18 inches from the edge of the table. This was exactly what I had "willed."

The different effects produced by a slack and a stiff copper wire respectively would seem to show, clearly, that the patient cannot acquire the "will" of the operator unless the connection be sufficiently rigid to communicate the involuntary muscular action of the operator, however imperceptible such action may be to the latter himself, who wills what the patient is to do.

GEORGE HENSLOW

A Gun-Signal Recorder

IN the judgment recently delivered by Mr. Mansfield on the stranding of the steamer *Britannic*, he says:—"With respect to the signals from the Hook Tower it is stated that the gunner who discharged the gun—a twenty-four pound gun—commenced firing at 1.50 a.m. on July 4, and continued firing at intervals of ten minutes till 10.10 a.m. He took the time from his watch, as his sandglasses were unserviceable; he had no light but a dark lantern in his gunhouse. Without imputing to him intentional neglect of his duty or wilful misrepresentation, it seems to the Court that he may have been less vigilant and less accurate than men who were keenly awake to the difficulties of their position, and who must have known that the safety of the ship was involved in their taking the time between the signals with scrupulous care. In his unsupported testimony the Court cannot find that the signals from the Hook Tower were fired at regular intervals of ten minutes. Looking at the importance of accuracy between the intervals of the fog-signals, the Court wish to draw attention to the statement of the gunner that he has no relief in his duty, however prolonged it may be; nor do the Court find that there is any check, mechanical or otherwise, on the gunner to insure accurate firing."

The writer would suggest that a simple recording apparatus might be made by means of a clock controlling the movement of a strip of paper, as in the Morse telegraph; this strip being divided by transverse lines into spaces representing minutes and seconds.

A diaphragm of thin sheet iron, caoutchouc, or other suitable material, connected with a metal point as in the phonograph, would then register each explosion of the gun by depressing the point on to the paper strip, and either making a pencil-mark or a perforation. Such an instrument would be a check on the accurate firing of the gun in the station where it was placed, and the production of the strip would do much to remove the uncertainty which appears to have existed in the case above cited.

Liverpool, July 30 A. G. P.

Symbolical Logic

As Mr. Venn appears to be really serious in accusing me of having misquoted him, I may as well give the whole sentence which contains the statement which he says I distorted. The complete sentence is this:—

"Take, for instance, such problems as those of which Prof. Jevons has discussed a sample under the name of Numerical Logic (*Pr. of Science*, p. 169), as any of those which play so large a part in Mr. Macfarlane's volume, or, still more, as those problems in Probability which Boole justly regarded as the crowning triumph of his system."

I certainly thought that in this sentence the last relative pronoun *which* referred to Boole's probability problems in general, but especially to that much discussed problem (sometimes called his "challenge problem") which Boole gave in illustration of what he conceived to be the superiority of his "general method" over the usual methods. It never struck me therefore that Mr. Venn would seriously accuse me of misquoting him because (in order not to inflict upon the readers of NATURE the irrelevant three-quarters of the above sentence) I represented him as saying that Boole "justly regarded *his problems in probability* as the crowning triumph of his system." What then are the problems to which Mr. Venn refers? This, I own, is not a point upon which I have "any claim to call for an answer," but I think it is a point upon which he might courteously condescend to gratify the natural curiosity of many admiring readers of his "Symbolic Logic," who (unlike me, I am afraid) cannot be suspected of any unkind wish to place him in a difficulty.

Boulogne-sur-Mer, August 2 HUGH MCCOLL

Bisected Humble-Bees

AT the end of my garden two magnificent lime-trees grow, on which bees—of specimens of which I herewith send you portions—feed at this time of the year by hundreds—by thousands. What kind of bees are they? But the following are the points on which I should like some information. Every morning I find numbers of them on the ground, helpless, behaving very much like men when they are drunk. What causes this? Next, how comes it to pass that, apparently, these helpless bees all become bisected or trisected as the specimens I send? This morning there are hundreds of portions under the trees. We have a family of "fly-catchers" in the garden—would they do it? T. MASHEDER

The Grammar School, Ashby-de-la-Zouch, July 29

[The bees are a common species of Bombus (Humble-bees), mostly workers, and mostly bisected at the junction of thorax and abdomen. Perhaps wasps are the culprits, adopting this method in order to rob the bees of their honey-bags. We shall be glad to have information on this point.—ED.]

A New Meter for Electric Currents

IN NATURE, vol. xxiv. pp. 294-5, you notice a new meter for electric currents, giving a description which is fairly correct for a slight sketch, and attributing the invention to Mr. Edison. The invention, however, is not American, but English, and, as the inventor, I think myself entitled to whatever credit this entirely novel system may merit. My patent rights for America have been purchased of me, and the invention will be shortly in use in New York. JOHN T. SPRAGUE

Birmingham

[Our correspondent is doubtless right in his claim. Nevertheless the invention we referred to in the brief note in question has been recently patented in this country on behalf of Mr. Edison, presumably at a later date than our correspondent's invention. We should be glad if he would kindly furnish us with the date of his English patent. We certainly meant no injustice in publishing the note.—ED.]

A POPULAR ACCOUNT OF CHAMÆLEONS [1]

II.

THE next most interesting of the animal's life processes is its change of colour. Mistakes and exaggerations as to this matter are of very old date. Aristotle believed

[1] Lecture delivered at the Zoological Gardens on July 28, 1881, by St. George Mivart, F.R.S. Continued from p. 313.

the change to be due to the inflation of the body, and we all know that in Gay's fables it is represented as changing from black to green, blue, and white. The truth is the ground colour of the animal may vary from pale yellow to light or dark green, and so from a bluish to a dark leaden colour.

It is often of a general pale yellow tint, especially at night, in the dark and when perfectly dormant. The general colour need not be uniform, but in one region of one colour, and of another colour in another region, and yellow and bluish tints may be so mixed as to produce a green appearance. The colours may also be different on the two sides of the body. Its most ordinary colour resembles that of the bark of trees or that of leaves, but very distinct and very varied markings may appear as spots or stripes of pale gray, or brown, or black, or yellow, and the stripes or series of spots may extend longitudinally or transversely. Moreover the spots may be either close or distant, and round or angular. They may be dark on a light ground, or light on a dark ground. All the changes of colour which take place take place gradually, and the spots which appear, disappear, and re-appear, are not reproduced in the same places with the exception of markings which radiate from the eye, and others on the tail and limbs.

My poor friend, the late Mr. H. N. Turner, jun., remarked[1] of a chamæleon kept by him that its general tint varied from brown or olive to bright green and yellow. When brought from the dark into lamp-light he found that the side next the light changed sooner than the other. The line of prominent tubercles in the middle of the under surface of the body remains constantly white. Mr. Turner's experiments and those of van der Hoeven seem, as was to be expected, to negative the idea that the animal can assume the colour of surrounding objects.

This faculty of colour change is not really so exceptional a phenomenon as many persons suppose. It exists in certain mollusks, and notably in the cuttle-fishes, which rival the chamæleon in their changing tints. It is also found in certain frogs and lizards, especially in the American kind, called *Sphærops*. As to fishes, Dr. Günther tells us[2]: "In many bright-shining fishes—as mackerels, mullets—the colours appear to be brightest in the time intervening between the capture of the fish and its death, a phenomenon clearly due to the pressure of the convulsively-contracted muscles on the chromatophores. External irritation readily excites the chromatophores to expand—a fact unconsciously utilised by fishermen, who, by scaling the red mullet immediately before its death, produce the desired intensity of the red colour of the skin, without which the fish would not be saleable. In trout which are kept alive in dark places, the black chromatophores are expanded, and consequently such specimens are very dark-coloured; when removed to the light they become paler almost instantaneously.

The chamæleon lays eggs, and its manner of doing so has been described by Vallisnieri, who carefully observed the actions of a female in his possession. She wandered about on the floor of her inclosure till she found a place devoid of dust or sand. There she began to scratch, and continued scratching for two days, till she excavated a depression four inches wide and six inches deep, in which she deposited thirty eggs. She then carefully covered them up, first with earth, and then with dry leaves and twigs and bits of straw.

There are now fifty known species of chamæleon, and twenty-five of them are distinguished by prominences either on the end or sides of the muzzle, or over the eyes, or on the top of the head, or on the occiput. The first twenty-five of the entire list are devoid of such prominences. Their names and the localities whence they come are as follows:—

[1] *Proc. Zool. Soc.*, 1851, p. 203.
[2] See his recent magnificent work on Fishes, p. 183.

(1) *Chamæleo vulgaris* is found in Southern Spain, Northern and Southern Africa, Asia Minor, Arabia, Hindostan, and Ceylon. No other kind of chamæleon has nearly so extensive a range.

(2) The kinds called *C. lævigatus* and (3) *C. affinis* both come from Egypt or Eastern Africa. *C. Senegalensis* (4), *C. gracilis* (5), *C. granulosus* (6), *C. dilepis* (7), *C. anchietæ* (8), and *C. fasciatus* (9), all come from Western Africa. *C. cristatus* (10) and *C. Burchelli* (11) come from Fernando Po. *C. capellis* (12), *C. ventralis* (13), *C. pumilus* (14), *C. namaquensis* (15), *C. melanocephalus* (16), *C. gutturalis* (17), and *C. tæniabronchus* (18), all come from Southern Africa. The kind called *C. tigris* (19) is from the Seychelle Islands; and the two species, *C. cephalolepis* (20) and *C. pollenii* (21), are from the Comoro Islands. *C. verrucosus* (22), with *C. balteatus* (23), *C. lateralis* (24), and *C. campani* (25), are from the great island called Madagascar.

As to each of the next list a word or two must be said.

The form called *C. antimena*[1] (26) is furnished with an outgrowth flattened from above downwards, at the end of the muzzle, which is cartilaginous towards its distal end. *C. Labordi*[2] (27) has a similar process more prolonged and entirely bony. *C. superciliaris*[3] (28) has a triangular prominence over each eye. *C. pardalis*[4] (29) has a nose dilated and toothed on each side in front. In *C. globifer*[5] (30) a globular prominence projects anteriorly from each side of the end of the muzzle. *C. calyptratus*[6] (31) and *C. calcaratus*[7] (32) have each the summit of the head conically produced. In *C. cucullatus*[8] (33) a very prominent flap extends out on each side from the occiput. In *C. gularis*[9] (34) there is also a pair of occipital flaps, and the same is the case in *C. brevicornis*[10] (35), with the addition of a process on the end of the snout, covered with smooth scales. *C. Malthe*[11] (36) has a pair of slightly different occipital flaps with the addition (in the male) of an obtuse nasal prominence, which is grooved above. *C. rhinoceratus*[12] (37) has a single central elongated bony nasal prominence, but no occipital flaps. In *C. minor*[13] (38) the male has two flat, compressed, diverging nasal prominences covered with large scutes. In *C. bifurcus*[14] (39) there is a similar pair of bony processes, and also in *C. Parsoni*[15] (40). In *C. O'Shaughnessi*[16] (41) there are also two divergent, compressed, scute-covered nasal prominences. In *C. gallus*[17] (42) the nose of the male is provided with a single long conical appendage, but it is flexible and covered with short tubercles. It and the preceding twenty species also all come from Madagascar. *C. nasutus*[18] (43), from Eastern Africa, has a similar flexible protuberance. The snout of *C. montium*[19] (44) has two prominences which are veritable nasal horns horizontally projecting forwards from above the nostrils. Each is encased in a finely-annulated sheath. It comes from the Camaroon Mountains. The male of *C. Owenii*[20] (45) has no less than three such sheathed horns, one projecting from the front of each orbit, and the other from the middle of the nose. It is an inhabitant of the Island of Fernando Po. In *C. Melleri*[21] (46) the male has a single, compressed bony prominence, sharp-edged above. It comes from Eastern Africa. *C. monachus*[22] (47) has two large occipital flaps. It is an inhabitant of

[1] See Grandidier, *Ann. des Sc. Nat.*, xiv. 1872.
[2] *L.c.*
[3] *Archiv. du Mus.*, vi. Pl. XXII. Fig. 14.
[4] *L.c.* Fig. 11.
[5] Günther, *P. Z. S.* 1879, p. 149, Pl. XIII.
[6] *Archiv. du Mus.*, vi. Pl. XXII. Fig. 1.
[7] Peters, *Monatsber* Berlin, 1869, p. 445.
[8] *P. Z. S.* 1864, p. 746.
[9] Günther, *P. Z. S.*, 1879, p. 149. Pl. XII., Fig. B.
[10] *L.c.* Fig. A; and *Ann. and Mag. of Nat. Hist.*, May, 1881, p. 358.
[11] *P. Z. S.*, 1879, p. 148. Pl. XI.
[12] Gray, *P. Z. S.*, 1864, p. 478.
[13] Günther, *Ann. and Mag. Nat. Hist.*, p. 240, Pl. XIII.
[14] *Arch. du Mus.*, vi Pl. XXII. Fig. 3.
[15] *L. c.*, Fig. 12.
[16] *Ann. and Mag. of Nat. Hist.*, p. 357, Pl. XIX.
[17] *Ann. and Mag. Nat. Hist.*, p. 315, Pl. XVI. Fig. B.
[18] *Archiv du Mus.*, vi. Pl. XXII. Fig. 4.
[19] Günther, *P. Z. S.*, 1874, p. 442, Pl. LVI.
[20] *Archiv du Mus.*, vi. Pl. XXII. Fig. 10.
[21] Gray, *P. Z. S.*, 1864, p. 478, Pl. XXXII. Fig. 1.
[22] *P. Z. S.*, 1864, p. 470, Pl. XXXI.

the Island of Socotra. There are also the occipital flaps in *C. Petersii*[1] (48), from Eastern Africa. The two remaining chamæleons are so distinct from the foregoing that they rank as a distinct genus called *Rhampholeon*, a genus which was instituted by Dr. Günther in 1874. The first of these, *R. spectrum*[2] (49) is from the Camaroons; the second, *R. Kerstenii*[3] (50) is from Eastern Africa. Both agree and remarkably differ from all other chamæleons in having the tail short, it being only one-third the total length, or even less. Though its end is prehensile, its prehensile action must be much less perfect than that of the tails of the preceding forty-eight kinds; but this defect is compensated for by the development of a sharp tooth, or denticle, at the inner side of the base of each claw, which must give it a firmer grip. Moreover in *R. spectrum*, though not in *R. Kerstenii*, the grip is yet further aided by a spine which projects vertically from the inner, or flexor, surface of each finger or toe. In *R. spectrum* each eyebrow is produced into a flexible horn-like prominence. In *R. Kerstenii* two long processes project forwards, one over and in front of either eye.

Thus the geographical distribution of the chamæleons is very remarkable. With the single exception of the common species they are entirely confined to Africa and certain more or less adjacent islands, and exist mainly on the south of the equator. No less than twenty-one out of the fifty kinds are from Madagascar, and of the twenty-five kinds which have been enumerated as having horns or other remarkable processes on the head, no less than seventeen are from the same very interesting island, which is thus the great home of chamæleons generally, and especially of these curiously distinguished kinds. The plate-snouted (*C. antimena* and *C. Labordi*), the bony, double-horned species (*C. minor*, *C. bifurcus*, *C. Parsonii*, and *C. O'Shaughnessii*), and the lofty-helmeted (*C. calyptratus* and *C. calcaratus*) kinds are quite peculiar to Madagascar. Those with occipital lobes are found not only there, but also in Mozambique and the Island of Socotra. The Madagascar single-horned *C. rhinoceratus* is resembled by the East African *C. Melleri* and the flexible-snouted Madagascar form, *C. gallus*, is resembled by the East African *C. nasutus*. The species with true horny sheaths to their horns (*C. montium* and *C. Owenii*) are exclusively West African forms.

Fernando Po possesses three species. Two are from the Camaroons. One is an inhabitant of the Seychelle Islands, and two are from the Comoro Islands between Africa and Madagascar. Apart from the common species three kinds are from Eastern Africa, two from Egypt and Abyssinia, nine from Western Africa, and eight from Southern Africa.

Such are the leading facts with respect to chamæleons considered by themselves. Let us now consider their more significant relations to other animals.

The entire mass of animals of all kinds, from what is commonly called the animal kingdom, in contrast with and in distinction from the vegetable kingdom: this great whole is divided into certain vast groups called sub-kingdoms, and the highest of them, called the vertebrate sub-kingdom (because its members possess a spinal column), comprises ourselves, with all beasts, birds, reptiles, efts, frogs and toads, and fishes. We and beasts constitute what is called a class—the class *Mammalia*. Birds form another class—*Aves*. Reptiles (*i.e.* all tortoises, lizards, serpents, and crocodiles, with certain extinct kinds) together constitute the class *Reptilia*. The efts of all kinds, with all frogs and toads, and some other creatures, living and extinct, form the class *Batrachia*, while all fishes are grouped together in the one class *Pisces*. But these five classes are not equally distinct one from another. Birds and reptiles, batrachians and fishes go together as two sets of classes or provinces. On the province containing birds and reptiles the name *Sauropsida* has been bestowed, while the term *Ichthyopsida* has been used to denote the province which contains both Batrachians and Fishes.

The existing class of reptiles contains four orders:—(1) *Crocodilia* (crocodiles and alligators); (2) *Lacertilia* (lizards); (3) *Ophidia* (serpents); and (4) *Chelonia* (tortoises and turtles).

The order *Lacertilia* is made up of a certain number of large groups, each of which is called a family, which family is again composed of genera, while each genus consists of one, two, few or many species.

The chamæleons, as we have seen, form fifty species arranged in two genera: forty-eight species in the genus *Chamæleo*, and two in the genus *Rampholeon*. These two genera together constitute a family—a family of the order Lacertilia.

Putting aside on this occasion a certain very exceptional genus called *Hatteria*, the families of the order Lacertilia may be enumerated as follows:—the true lizards (*Lacertidæ*); the Scincs (*Scincidæ*); the Chalcidians (*Chalcidæ*); the Iguanians (*Iguanidæ*); the Geckos (*Geckotidæ*): and the Monitors (*Varanidæ*).

From all these families that of the chamæleon differs most widely. It differs from all of these:—(1) in the compressed body raised from the ground by its long limbs; (2) in its tongue; (3) in its eyes; (4) in the shape of its feet; and (5) by the form of the tail. It further differs from the Iguanians, Lacertians, Scincs, and Chalcidians, in that its body is not covered with scales.

There are certain Iguanians which present a slight resemblance to the chamæleons: such are the American *Polychrus*, and still more *Sphærops*, which has the eye covered with a granular eyelid with only a small central aperture, and has an equal facility in changing colour. These, however, are but superficial agreements, and in all essential points *Sphærops* is a true Iguanian, and in no way a chamæleon.

Prof. Parker assures us that while the chamæleon is an animal, the structure of the skull of which is "specialised to the utmost," it is nevertheless in other respects a very low form.

The answer to our question, "What is a chamæleon?" is, then, that it is a very exceptional family of the order *Lacertilia*, an order of the class Reptilian, a class which, together with birds, form the Sauropsidian province of the great vertebrate sub-kingdom of animals.

Can we gain any light as to the mode of origin of chamæleons?

The best light we can obtain as to the origin of existing forms is derived from the fossil remains of creatures nearly allied to them. In this way we have been able pretty clearly to ascertain that hog-like creatures and ruminating animals are diverging offshoots from a much more ancient, common, and intermediate type.

In this way also we have, I think, fair evidence to show that the cats are derived from creatures more or less nearly allied to the existing civets.

But the science of organic fossil remains—palæontology—has only as yet been able (so far as I am aware) to point to one relic which has been supposed to be of chamæleon nature—part of a lower jaw from Eocene deposits in North America. It would be curious if an ancient chamæleon should be discovered to have inhabited a region so distant from the home of the existing kinds as is North America. It would not however be an unparalleled fact, for the existing Old World camel was once a New World form. The true nature however of the fragmentary fossil is very doubtful, and we may therefore say that as yet we have no evidence as to the antiquity of the family. But should the fossil turn out to be really part of the jaw of a chamæleon, it would but tend to show that the group itself existed already in Eocene times; it would

[1] *P. Z. S.*, 1864, p. 470. [2] *P. Z. S.*, 1874, p. 443, Pl. LVII.
[3] Peters in von der Decken's "Reisen," iii. p. 10, Table I. Fig 1; see also *Ann. and Mag. of Nat. Hist.*, September, 1880, p. 238.

not throw any light upon the *mode of origin* of that group.

The chamæleons have, as we have seen, their main home in Madagascar. That island is also the main home of another very exceptional group, the exceptional group of beasts called lemurs. But lemurs have much resemblance, though probably no true affinity, with apes, and the apes are a group, even more isolated perhaps than lemurs. It is as yet quite impossible to say from what root the ape order took its origin.

The same thing may be said (and a few weeks ago was said by our president in this room) respecting the cetaceans, the order, that is, of whales and porpoises. The same thing again may be said of that very exceptional order of flying beasts, the bats. The chamæleon family then is only one of many others which have this at present quite isolated character. But if we can obtain no clue as to the chamæleon's origin, can we detect any special or unexpected affinities between it and any other creatures which do not belong to its own class, the class of reptiles?

It is now very generally supposed that birds have been derived from reptiles, and there seem to have been two distinct lines of descent—the ostrich kind of birds, from extinct land reptiles called *Dinosauria* (of which the great *Iguanodon* of the Wealden formation is a type) and the other birds from extinct flying reptiles called *Pterosauria*, which had much analogy with our bats. This double origin (which I advocated ten years ago) has recently been reinforced by investigations of Prof. Vogt with respect to that extinct feathered creature of the Oolite, the *Archeopteryx*, which turns out to have many affinities with the Pterosauria.

Now the chamæleon has no resemblance either to the Dinosaurian or to the Pterosaurian reptiles, and certainly nothing could well be less bird-like in appearance or in habits than the chamæleon. The one only point of resemblance—that between its pincer-like feet and those of the parrots—is but a very incomplete one, as we have already seen. Nevertheless there is one strange and unexpected structural character already noted to which it may be interesting to revert.

In birds the lungs (unlike our own and those of beasts) are not closed bags, but communicate with air-sacs which extend far and wide within the body, and which doubtless facilitate their powers of aërial locomotion. In the most active lizards, which dart so quickly to their shelter that the eye cannot follow them, there is nothing of the kind; neither is there in those little lizards which take such long jumps with the help of their parachute-like wings, that they may be said to flit—lizards called by the absurdly formidable name of "flying dragons;" yet in the chamæleon, in spite of its sluggishness, such sacs are present, and thus render unavailing a character which might otherwise be employed to distinguish all birds from all existing reptiles.

But though neither comparative anatomy nor palæontology yet enables us to speculate profitably on the origin of the chamæleon's family, there is one feature met with in many of the species which tends to shed a certain amount of light on principles of variation, and therefore on that of specific origin generally. I refer to the circumstance that so many kinds of chamæleons develop crests, processes, or horns on the muzzle and over the eyes or on the occiput. These outgrowths are so different one from another that it is impossible to believe that they have arisen by inheritance and descent from any one peculiarity of the kind. Superciliary prominences could not give rise to nasal protuberances, or bony outgrowths to true horn-sheathed excrescences, and none of these could either be the parents or the offspring of occipital flaps.

The phenomenon is parallel to what we find in certain groups of birds, as *e.g.*, the birds of paradise, so many kinds of which develop unusual feathery outgrowths—these outgrowths being often so different in nature that they cannot be supposed to have been derived by inheritance one from another.

In such birds then we must admit (as I have long ago urged) that there exists an innate tendency to unusual outgrowths of feathers of one or another kind, and similarly we must admit that there is extant in the nature or essence of chamæleons a tendency to osseous or horny outgrowths from the head of one or of another kind. It has been suggested that these outgrowths in the males are due to the wayward fancy of female chamæleon taste. And certainly the female chamæleon, with her exceptional power of independently moving her eyes, and so simultaneously considering and accurately comparing the horns and warts of two rival swains, is unusually qualified for making a careful matrimonial choice. Seriously speaking, however, I regard this explanation as quite inadequate.

I have elsewhere[1] given my reasons for considering this explanation to be a mistaken one, but the question is far too wide to discuss to-day, suffice it to say that even if this hypothesis were correct it would but imply the presence of an innate tendency in the female to admire horny and warty prominences of certain varied kinds. The one innate tendency is as mysterious, and when deeply considered as significant as in the other.

But apart from these questions, which, however interesting they may be, are still matters of uncertain speculation, the actual structure and the unquestionable facts of the chamæleon's physiology are, as I trust you will now agree with me in saying, matters of very great interest. They offer fields as yet unexplored for careful observation and experiment. Even the most peculiar and important of all the chamæleon's actions—the emission and retraction of its tongue—are actions which, so far as I know, are not by any means clearly understood. But when to such matters of direct observation or immediate inference we add the problems to the solution of which elaborate reasoning has to be employed—reasoning based on wide knowledge of the structures of animals existing and extinct—it will, I think, be evident that the leisure of a long life might be usefully devoted to obtaining a complete and far-reaching knowledge of the natural history of that exceptional family of Lacertian reptiles, the family of the chamæleons.

THE INTERNATIONAL MEDICAL CONGRESS

THE seventh meeting of the International Medical Congress, which has just been held in London, has been remarkable from many points of view. The sudden growth of the Congress from an assembly of 600 to one of over 3000 members, the truly cosmopolitan character of the gathering, the great scientific activity displayed, the lavish private and public hospitality and marked Royal patronage conferred, have one and all marked out this meeting as a very great event. It has been the largest and most complete assembly of scientific men that this age, and therefore any age, has ever witnessed, and if the results to science should prove to be at all commensurate, it will be a very prominent event in the history of the progress of science.

The many and complicated arrangements have been admirably planned by Mr. MacCormac and his able assistant, Mr. Makins, and they have borne successfully the heavy strain of a larger number of members than was previously expected. The Congress has held six general meetings, at each of which an address has been delivered, and the more special work has been conducted in the fifteen sections among which it has been split up. Sir James Paget, as President, delivered the opening address on Wednesday last, which was characterised by his usual

[1] "Lessons from Nature," Chap. X. (Murray, 1876).

eloquence and scientific ability. He did not confine himself to any one subject, but glanced at the progressive character of science, the need for the work of all varieties of minds, and the aim and purpose of science as applied in the medical arts. On the same afternoon Prof. Virchow discussed the value of pathological experiment in an address displaying the most thorough grasp of his subject and vigour of thought and diction; he attacked the opponents of vivisection for their utter inconsistence, and gave a very weighty protest against their claim to regulate the pursuit of knowledge. The French address was to have been read by Prof. Raynaud of Paris, but his sudden death only a few days before the meeting prevented this arrangement being carried out, and the address he had already prepared was read by his friend, M. Féréol: it dealt with the subject of the right sphere of action, and the influence of scepticism in medicine. On Saturday Dr. Billings gave a masterly address on Medical Literature; his tables showed a most alarming growth in the production of volumes and periodicals during the past ten years, but he was able to give some consolation by the statement that the rate of growth had of late shown some slackening: his wise and witty remarks on book-writing, bibliography, cataloguing, and reference were especially valuable as coming from a man of considerable experience in these matters, and applying equally to all varieties of literature. On Monday, Prof. Volkmann, one of Mr. Lister's most ardent disciples in Germany, gave an address on Modern Surgery, which resolved itself into a review of the progress and results of antiseptic surgery. He was followed by Prof. Pasteur, who in a few moments described his latest experiments, and announced results which promise to have as important effects for useful animals as Jenner's vaccination has for man. The final general meeting was held on Tuesday last, when Prof. Huxley addressed the Congress on the Connection of the Biological Sciences with Medicine, tracing this connection from step to step, and pointing out the necessity for a similar close union in the future. The entertainments during the week have been many and brilliant, including, in addition to many partly private, a soirée at South Kensington Museum, a dinner at the Mansion House, reception at the Guildhall, reception by Earl and Lady Granville, *conversazione* at the College of Surgeons, and informal dinner at the Crystal Palace. Notwithstanding all these diversions the real hard work that has been done every day by the great mass of the members of the Congress has been very great, and this, and the free interchange of ideas in conversation of many workers in the same part of the field of science, must be productive of good, both by its direct effect and by the stimulus to work it must afford. Among the many subjects discussed, the germ theory and its various practical bearings and outcomes, have had a prominent share. In the Surgical section there was a debate on the treatment of wounds, in which it was incidentally raised, and there appeared to be a general consensus of opinion that particulate germs play an all-important part in the production of wound diseases, though there was by no means such agreement as to the best means of treating wounds. In the Pathological section a long and very animated discussion was introduced by Prof. Klebs, who discussed the relations of minute organisms to certain specific diseases. Dr. Charlton Bastian supported his well-known views, and was opposed by Lister, Virchow, Pasteur, Hueter, Cheyne, and Roberts, and it was made abundantly evident that the germ theory of disease has not only established itself firmly in the faith of scientific pathologists, but that its importance is becoming wider and greater with rapid strides. By far the most valuable of all the communications bearing upon this subject was M. Pasteur's account of his recent "vaccination" experiments. He has found that by a special mode of cultivation of the poison of chicken cholera he can obtain an attenuated or weakened virus, and that vaccination with this attenuated virus, which merely causes slight and transient local mischief, protects fowls completely from the most active virus for a certain time, and enables them to resist the disease for a far longer period. He has also demonstrated that the source of the attenuation of the virus is the action of atmospheric oxygen, for it is only when the "germs" are allowed to develop in the presence of abundance of oxygen that the containing fluid becomes less intensely poisonous. A "vaccine" for splenic fever or charbon could not be obtained in this manner, but if the virus be allowed to develop in a solution at a temperature of 42°—43° C., with free exposure to the air, it quickly becomes less active, and ultimately, at the end of a few weeks, dies. Experiments on sheep have shown that vaccination with this "attenuated lymph" protects the animal from the action of the purer and more active poison. But great as will be the value of these researches, even if only applied to the two diseases in question, it is far more important to notice their extreme importance from a scientific point of view. First of all they explain in part the action of oxygen in preventing septic infection, and the inflammatory complications of wounds. But they also excite the hope, and go far towards showing that it is not improbable, that by some special form of cultivation every disease-virus may be thus attenuated and a poison result, which if inoculated will produce only a transient local change, but will protect from the virulent form of the disease as completely as efficient vaccination protects from small-pox. Prof. Pasteur referred to the germ theory of disease as one which has ceased to number the practical triumphs it has won; and every day is giving results to add to its importance and value.

NOTES

MR. W. H. M. CHRISTIE, F.R.S., First Assistant at Greenwich Observatory, has been appointed Astronomer Royal, in succession to Sir George Airy, who retires after holding the office for nearly half-a-century.

ON October 17 next, fifty years will have elapsed since Prof. Bunsen, the eminent chemist, received his doctor's diploma from Göttingen University. He, however, intends to absent himself from Heidelberg on the day in question, in order to avoid all congratulations and speech-making.

MR. W. A. FORBES, B.A., Fellow of St. John's College, Cambridge, Prosector to the Zoological Society, has been appointed Lecturer on Comparative Anatomy at Charing Cross Hospital, *vice* the Rev. J. F. Blake, removed to Nottingham.

THE discussion in connection with Mr. Mundella's able statement on the Education Estimates had no special bearing on the teaching of science in elementary schools. Steps are evidently being taken to make elementary education more and more efficient, to give those whose school years are short and precious every opportunity of acquiring a knowledge of things that will be really useful to them in after life. It is clear from the facts and figures, as well as the tone of Mr. Mundella's address, that the education of the country is safe in his hands. In the proposals for the revision of the Code laid on the table of the House are several changes for the better. In infant schools, for example, part of the course provided for is a systematic one of simple lessons on objects and on the phenomena of nature and common life. Among the "Class Subjects" in boys' and girls' schools are Physical Geography and Elementary Science, and among the specific subjects are Mechanics, Animal Physiology, Botany, Principles of Agriculture, and Domestic Economy. This is all in the right direction, and is just what we should expect from an Education Minister like Mr. Mundella.

MR. MUNDELLA stated on Monday that Prof. Leone Levi has prepared an elaborate report on technical education in Italy,

which will be referred to the Royal Commission about to be appointed.

AT the Exhibition of Electricity the completion of the English telegraphic department is progressing favourably. The series of solid and compact sounders used in the British service will contrast, not without advantage, with the quadruplex Baudat and other apparatus presented by the French administration. The Italian historical section is full of relics of instruments used by Galvani, Volta, &c. A large number of autographs will be exhibited, among which we may note a letter from Volta to Sir Joseph Banks, then president of the Royal Society. This document is stated to be the first description of the Voltaic battery ever written by its inventor. A small magnet, which Galileo armed with his own hand, is exhibited, as well as another magnet used by the academicians "del Cimento" for their determination of the laws of the variation of the attractive power according to distance. The Academy of Aërostation of Paris exhibits a model of the electro-subtractor, an electrical balloon constructed according to the principles advocated by Dupuy de Lome, and a number of other electrical instruments. M. Jules Godard, a well-known aëronaut, has sent an electrical warner; when the balloon is descending an electrical vibrator is set in operation; when it is ascending another bell rings. This effect is obtained very simply by a valve, which is in equilibrium when the balloon keeps its level, and is moved by a slight wind. The formal opening was to take place yesterday by a visit of the President of the Republic, and the doors will be thrown open to the public to-day, although much remains to be done for the completion of the display, which will be a great success.

THE French Government has appointed a Committee, presided over by Rear-Admiral Bourgeois, to study the different applications of electricity to navigation.

THE rapid advance of civilisation, it is admitted, has the effect of causing native races more and more to disappear. It is therefore the duty of scientific ethnology to save the little which exists still in its originality from destruction, and to preserve the few authentic fragments of an epoch which threatens to be annihilated. The Anthropological Society of Hamburg has issued an application to all those who have occasion, either by their position or calling, &c., especially to consuls, missionaries, merchants, captains, to enter their notes on little-known countries and their populations on a schedule which the Society will supply. The questions being intentionally short and as few as possible, any further communications on the character of the country, notes on the climate, corrections of the charts and sailing directions, would be thankfully welcomed. A great service would be rendered also by sending ethnographical objects, photographs, models, &c., which will be entrusted to the care of the Ethnological Museum.

FROM a Report on the means employed in France for protecting the vine from destruction by the Phylloxera, by Mr. C. H. Perceval, H.M. Consul at Bordeaux, we take the following interesting extract:—"The information which I have gathered on this subject, from official and other sources, tends to reduce the methods used to the following three:—firstly, submersion of the vineyard, when practicable; secondly, by employing insecticides; and, thirdly, where the vineyards have been destroyed, by the plantation of American varieties of vines, whose roots offer more resistance to the attack of the insect. M. Armand Lalande, the President of the Chamber of Commerce of Bordeaux, proprietor of extensive vineyards in the Médoc, a gentleman to whom I am much indebted for the information and assistance which he has been kind enough to afford me in drawing up this Report, addressed a meeting of that body held in March last on various topics, and I translate the following from his remarks regarding the Phylloxera:—'The Chamber of Commerce has not ceased to show the extreme importance which it attaches to all the means employable in combating this dreadful scourge. Of the 2,200,000 hectares which composed the vineyards of France, 500,000 are destroyed, 500,000 others are greatly attacked: it is a loss of more than three milliards to the country. The Gironde is one of the departments which has suffered most: one-third of the vineyards are destroyed, another third is badly attacked. We must admit, with sorrow, that the very sources of our commerce and of the well-being of our southern population are most seriously compromised. Still we have great hopes that, by energetic and intelligent efforts, we may be enabled gradually to arrest and repair the evil. For the very important vineyards of the Gironde, where submersion is possible, it is a sure remedy, which is generally employed, and with invariable success. In the cases of vineyards already destroyed, the remedy seems to be, to reconstitute them by planting American vines as stocks for grafting French cuttings on, which plan has been the subject of satisfactory and conclusive experiments for the last few years, especially in Languedoc. Where the vines are not too far gone, a judicious use of sulphur of carbon is a certain means of preservation, and, in most cases, practicable, owing to the moderation of the cost.' He then states that he bases his opinion on astonishing and conclusive results, which he has observed in immense vineyards in Languedoc, and also in others of the Gironde, and proposes that steps may be taken to hold an international congress on Phylloxera here in the autumn." The Congress is to open on September 5. As we intimated last week, another Viticultural Congress meets in Milan next week. Mr. Perceval gives some valuable details on the various methods of treating the disease.

MM. KOCH AND KLOCKE, who have continued during the summer of 1880 their interesting observations on the motion of the Morteratsch glacier, publish their results in the eighth volume of the *Proceedings* of the Natural History Society of Freiburg. They have measured each half-hour during a fortnight the motion of a point on the glacier, and this year, as well as during the foregoing year, their results are almost negative, *i.e.* the motion was so slow, and the advance of their signal-stick was so small and often even negative, that nothing can be inferred until now as to the motion of this glacier. Thus observing, for instance, the advance of their signal each half hour, on September 11, from midday to six o'clock in the evening, they find the following figures, in millimetres: 0·5, −0·5, −0·5, 0·5, 0·0, 0·2, −0·2, 0·2, −1·0, 1·3, −1·5, −1·5, the negative figures showing a back movement of the signal. Therefore MM. Koch and Klocke have undertaken a thorough verification of their instruments, and they have arrived at the conclusion that the motion observed cannot be attributed to errors of observation. Besides they have devised a special arrangement for keeping their signal motionless in the ice; they sink into the ice of the glacier a large copper tube which is filled with ice and salt, and covered by a small hill of ice, and only then they adjust their scale on the tube. This signal remaining firm throughout the day in the ice, the theodolite being also motionless, and the probable errors of observation not exceeding 0·3 millimetres, the small observed motions must be attributed, they suppose, to some cause yet unknown.

AT a recent preliminary meeting at Fishmongers' Hall it was resolved to hold a public meeting in the above hall on Friday, August 5, to make arrangements for holding an International Fisheries Exhibition in 1883.

UNDER the superintendence of Mr. Wallace, rector of Inverness High School, several of the scientific societies of Northern Scotland met at Elgin on July 29 and 30. Several papers were

read and excursions made to places of interest in the neighbourhood, and the meeting seems to have been altogether satisfactory. Arrangements were made to hold a similar meeting next year at Inverness.

THE Annual Meeting of the British Medical Association was opened on Tuesday at Ryde, Isle of Wight.

ANOTHER smart earthquake shock, not so strong however as the last, was felt at Geneva on Friday morning. Three earthquake shocks were felt on Thursday night at Allevard, near Grenoble. An undulatory shock of earthquake was felt at Agram on July 28 at 11h. 8m. a.m. Its direction was from south-east to north-west, and subterranean noise accompanied it. Earthquakes are also reported from Haiti on July 5 and 7, from St. Vincent June 24 and 25, and from Trinidad on June 29.

THE Annual Report of the Paris Observatory for the year 1880 has just been published by the director, Admiral Mouchez. The chief work of the Observatory was the continuation of the revision of the Catalogue of Stars of Lalande; and of the 30,000 observations which were made by the meridian instruments 28,331 were made for this purpose. Until this is finished, the Observatory cannot undertake any other great work; and a catalogue of 20,000 stars observed two or three times up to the end of 1879 is already prepared. As to the precise determination of positions of the fundamental stars, it is not yet begun, the astronomers being engaged in the study of the errors of instruments. M. Lœwy has continued the study of the flexion of the meridional instruments, and the error for the larger one was found to be about 0·02 mm., that is about one second of arc. But M. Mouchez expresses the fear that this small error will be less than several accidental errors depending upon changes of temperature, upon the movements of the telescope and upon the errors of refraction due to imperfect observations of temperature at various heights. The great equatorial telescope was but little used, mainly because of the difficulties of management of the revolving tower. With the other equatorial telescopes the astronomers of the Observatory continued their work on the ecliptical charts, as well as of Jupiter, of the comets, of several small planets, and of double stars. The great telescope was employed for the first time during last year for photography; the photographs of the moon, not, however, as fine as those of Rutherfurd—will probably be soon much improved; several photographs of double stars, and even of nebulæ, were obtained. The most interesting work in physical astronomy was done by M. Thollon with the spectroscope: one of the protuberances he studied was rather remarkable by its immense length of eight minutes, that is of 300,000 kilometres. Much attention was given to the transmission of time to the clocks of the Observatory itself, of Paris, and of provincial towns. The astronomical museum, which will be opened at the Observatory, will soon be quite finished; it will contain a variety of instruments formerly used by renowned astronomers, numerous photographs of instruments of different observatories, and portraits; as to these last, the Report speaks in high terms of the courtesy of several astronomers in England, who have given all facilities for the execution of portraits from originals in their possession. After mentioning the various works pursued by the astronomers of the Observatory, besides their regular business, the Report speaks of the preparations for the observation of the transit of Venus in 1882. None of the methods employed until now have given quite satisfactory results, and the simple observation by telescope may yield errors of as much as ten and fifteen seconds. The photographs, which it was necessary to enlarge thirty and forty times, do not afford the necessary cleanliness. Thus the Observatory proposes to employ micrometrical measures which will afford a greater degree of accuracy when done by telescopes than those which are taken on photographs.

THE opening of the Période "Electorale" has directed the attention of the French Government to the opportunity of connecting the municipal telegraphic system of Paris with the postal organisation. It will be the work of a few days, and of a few hundred pounds.

FROM a privately issued report on silk cultivation in the Chinese province of Kwangtung, we learn that in the Pakhoi district, on the southern seaboard, wild silkworms are found which feed on the camphor tree, and their silk is utilised in a singular manner. When the caterpillar has attained its full size, and is about to enter the *pupa* state, it is cut open and the silk extracted in a form much resembling catgut. This substance, having undergone a process of hardening, makes excellent fish line, and is generally used for that purpose in the Pakhoi district.

FROM the *Colonies and India* we learn that a thick vein of a peculiar substance, which, according to local chemists, contains 50 per cent. of pure paraffin, has been discovered at Hawkes Bay, New Zealand. It is said to be worth 40*l*. per ton, and to exist in enormous quantities.

THE latest excavations made by order of the Athens Archæological Society at Tanagra, the well-known place in Bœotia whence come the charming terra-cotta figures, have yielded important results. On the northern side of the town, in front of the principal gate, fifteen tombs were discovered which were completely untouched. They contained some sixty clay figures, most of them perfect, and measuring between 10 and 35 centimetres in height. They represent satyrs and women standing and sitting, and one is a group of two figures. Besides these many vessels were found, amongst which some twenty lekythoi (paint and oil phials) with antique-painted ornaments. Unfortunately most of these were broken. One vase which was found in a stone case shows an artistic inscription which designates it as a work of Teisias. We may also mention that fourteen scraping irons were found, and also that in two of the tombs some fifty small terra-cotta ornaments were discovered, most of which were brightly coloured, and some covered with thin gold. The excavations became even more important after April 1. The published report mentions twenty vessels, some broken, ten of which are ornamented with paintings. Two of these are said to be particularly fine. Of the numerous clay figures only eight could be got out in a tolerably perfect condition. Of these two are reported to be the most perfect figures ever found at Tanagra. One represents a winged youth who is about to raise himself into the air; before him is a maiden on her knees, her dress forming an arc above her; the youth holds her by the arms as if he wished to take her along with him in his flight. The other masterpiece is an Aphrodite rising from the sea, diving up out of a shell as it were.

THE additions to the Zoological Society's Gardens during the past week include a Polecat (*Mustela putorius*), British, presented by Mr. H. C. Brooke; two Ground Squirrels (*Xerus getulus*) from West Africa, presented by Dr. W. Hume Hart; a Bateleur Eagle (*Helotarsus ecaudatus*) from Africa, presented by Mr. William Waters; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, presented by Capt. Robinson, R.M.S. *Warwick Castle*; two Black Storks (*Ciconia nigra*), European, presented by Dr. Rudolph Blasius; two Wood Owls (*Syrnium aluco*), European, presented by Mr. H. T. Archer; a Slow worm (*Anguis fragilis* albino), British, presented by Mr. A. Phipson, F.Z.S.; two Green Lizards (*Lacerta viridis*) from the Island of Jersey, presented by Mr. Claud Russell; a Sykes Monkey (*Cercopithecus albigularis*) from East Africa, a Common Chamæleon (*Chamæleon vulgaris*) from North Africa, deposited; an Erxleben's Monkey (*Cercopithecus erxlebeni*) from West Africa, two Egyptian Mastigures (*Uromastix spinipes*) from North Africa, two Aldrovandi's Skinks (*Plestiodon auratus*) from

North-West Africa, two Pantherine Toads (*Bufo pantherinus*) from Tunis, on approval; a Bennett's Wallaby (*Halmaturus bennetti*), born in the Gardens. In the Insectarium may now be seen larvæ of the scarce Swallow-tail Butterfly (*Papilio podalirius*), also those of *Attacus atlas* of various sizes, from ones just hatched to ones nearly full-fed. Other noticeable larvæ are the curiously shaped ones of *Stauropus fagi*, and young ones of the North American *Samia cecropia*. Imagos of *Attacus pernyi* are also emerging, reared from eggs laid in the Insectarium in the earlier part of the summer.

OUR ASTRONOMICAL COLUMN

GOULD'S COMET-OBSERVATIONS ON JUNE 11.—Dr. B. A. Gould, director of the Observatory at Cordoba, has communicated to the *Astronomische Nachrichten* particulars of his experiences while observing the great comet of the present year on the evening of June 11. On that evening, he says, "the comet was found with but little difficulty, although considerably north of the estimated place, being recognisable by its diffuse aspect, elongated form, and large diameter, although it was quite pale in the bright twilight, and the tail could not be seen." He had just obtained a rough determination of its position from the equatorial circles for the purpose of finding and identifying some comparison-star, when he found one in the field. He considered it to be some one of the many bright stars of Orion in the vicinity, which would be readily identified, and hence did not complete the approximate determination with the usual care, nor obtain instrumental readings for the star. This he describes as "only a little fainter than the comet itself, and not very dissimilar in aspect: since, although its apparent diameter was much less than the comet's, it was greatly blurred by the exceptionally thick haze and the mists of the horizon, the zenith distance being nearly 80°, I do not think it would have been below the third magnitude, and could rather believe it to have been as bright as the second." Dr. Gould adds: "Only four comparisons were obtained before the comet passed below the horizon; then on attempting to identify the star, I found it in none of the catalogues."

On the next evening he examined the region without finding any visible star, but Rigel was much brighter than the missing object, and there was no visible object in the vicinity of the comet, which he found nearly three degrees to the northward.

The observations gave the following results:—

1881, June 11, position of the comet from the circles of the equatorial. 10h. 58m. 9s. sidereal time. Right ascension, 5h. 11m. 4s. Decl. – 9° 36′.

The comparisons with the star gave:—(Comet—star.)

Cordoba Sid. T.		Diff. R.A.		Diff. Decl.	
h. m. s.		m. s.		r.	
11 8 49	...	+ 0 49	...	– 16·40	One revolution
11 11 2·5	...	49	...	16·16	of micrometer
11 13 11·0	...	48	...	16·17	= 19″·08.
11 14 37·5	...	48·5	...	15·87	
11 11 55	...	+ 0 48·6	...	– 16·15 (– 5′ 8″·1).	

Thus he deduced for the star's position R.A. 5h. 10m. 16s. Decl. – 9° 30′, where our catalogues have no conspicuous star. In his letter to Prof. Krueger he concludes thus:—

"The whole observation has seemed to me so improbable that I have hesitated a good deal before sending it to you, fearing some gross error in reading the circles. But I have discovered none, and the later determination of the comet's geocentric path will remove all uncertainties of this kind."

On receiving these particulars Prof. Krueger, determined the place of the comet for the time of Dr. Gould's observation, from the elements we published in this column, which were founded upon observations between June 22 and July 1, and finds R.A. 5h. 11m. 15s., Decl. –9° 32′·0, and thence for the place of the star R.A. 5h. 10m. 26s., Decl –9° 26′·9, showing only such differences from the observed place as might be well attributed to uncertainty of observation so near the horizon, and to the corrections which the elements used probably required before the perihelion passage. Prof. Krueger remarks that no known bright star exists in this position, and the star-chart of the Berlin Academy for this region, which was formed by Dr. Schmidt, shows here a great blank. He draws attention also to the significant fact that the observed motion in declination in the interval between the first and last comparisons is much less than that which the comet must have had; the elements would indicate about 45″ or more than 2·3 revolutions of the micrometer-screw, while the observations give only 0·5. Dr. Gould especially remarks upon the resemblance of the object to the comet, and Prof. Krueger suggests whether there could have been "eine Verdoppelung des Cometen in Folge einer Luftspiegelung," or again was a *second comet* observed?

The case is a very interesting one. With elements which must give the comet's place on June 11 within a very few seconds of arc, Prof. Krueger's inferences are fully borne out. Thus for June 11·41962, Greenwich mean time, which corresponds to 11h. 11m. 55s. Cordoba sidereal time, diminished by the time for aberration, the right ascension of the comet is found to have been 5h. 11m. 13·0s., Decl. – 9° 35′ 18″, agreeing closely with Dr. Gould's instrumental place obtained a few minutes earlier, and the differential observations thus give for the apparent position of the star, R.A. 5h. 10m. 24·4s., Decl. –9° 30′ 10″. There appears to be a misprint or an oversight in Dr. Gould's letter as regards the zenith distance of the comet and neighbouring object at the time of his observations, which would be nearer 85° than 80°.

SCHÄBERLE'S COMET.—The following elements of this comet have been calculated by M. Bigourdan, of the Observatory at Paris, from observations on July 18, 23, and 28:—

Perihelion passage, 1881, August 22·60205, M.T. at Paris.

	° ′ ″	
Longitude of perihelion	334 41 10	M. Eq. 1881·0
" ascending node	96 48 23	
Inclination	39 56 38	
Log. perihelion distance	9·801788	

Motion—retrograde.

Whence the comet's positions for midnight at Berlin, or about 11h. 6m. G.M.T., will be:—

		R.A.		Decl.		Log Distance from Earth.		Sun.
		h. m. s.		° ′				
August 11	...	7 54 0	...	+52 7·6	...	9·9307	...	9·8307
13	...	8 22 55	...	52 45·6	...	9·8973	...	9·8218
15	...	8 57 39	...	52 47·2	...	9·8638	...	9·8142
17	...	9 37 38	...	51 51·4	...	9·8317	..	9·8083
19	...	10 20 39	...	49 36·7	...	9·8031	...	9·8043
21	...	11 3 21	...	+45 49·6	...	9·7806	...	9·8020

The comet was within naked eye vision on the morning of July 29, and the intensity of light, according to theory, should increase until August 25, about which time we may look for a pretty conspicuous object. The most favourable period for observation will be during the last ten days of August.

THE CONNECTION OF THE BIOLOGICAL SCIENCES WITH MEDICINE[1]

THE great body of theoretical and practical knowledge which has been accumulated by the labours of some eighty generations, since the dawn of scientific thought in Europe, has no collective English name to which an objection may not be raised; and I use the term "medicine" as that which is least likely to be misunderstood; though, as every one knows, the name is commonly applied, in a narrower sense, to one of the chief divisions of the totality of medical science.

Taken in this broad sense, "medicine" not merely denotes a kind of knowledge; but it comprehends the various applications of that knowledge to the alleviation of the sufferings, the repair of the injuries, and the conservation of the health, of living beings. In fact, the practical aspect of medicine so far dominates over every other, that the "Healing Art" is one of its most widely received synonyms. It is so difficult to think of medicine otherwise than as something which is necessarily connected with curative treatment, that we are apt to forget that there must be, and is, such a thing as a pure science of medicine—a "pathology" which has no more necessary subservience to practical ends than has zoology or botany.

The logical connection between this purely scientific doctrine of disease, or pathology, and ordinary biology, is easily traced. Living matter is characterised by its innate tendency to exhibit a definite series of the morphological and physiological phenomena which constitute organisation and life. Given a certain range of conditions, and these phenomena remain the same, within narrow limits, for each kind of living thing. They

[1] Address at the International Medical Congress. By Prof. T. H. Huxley, LL.D., Secretary to the Royal Society.

furnish the normal and typical characters of the species; and, as such, they are the subject matter of ordinary biology.

Outside the range of these conditions, the normal course of the cycle of vital phenomena is disturbed; abnormal structure makes its appearance, or the proper character and mutual adjustment of the functions cease to be preserved. The extent and the importance of these deviations from the typical life may vary indefinitely. They may have no noticeable influence on the general well-being of the economy, or they may favour it. On the other hand, they may be of such a nature as to impede the activities of the organism, or even to involve its destruction.

In the first case, these perturbations are ranged under the wide and somewhat vague category of "variations"; in the second, they are called lesions, states of poisoning, or diseases; and, as morbid states, they lie within the province of pathology. No sharp line of demarcation can be drawn between the two classes of phenomena. No one can say where anatomical variations end and tumours begin, nor where modification of function, which may at first promote health, passes into disease. All that can be said is, that whatever change of structure or function is hurtful belongs to pathology. Hence it is obvious that pathology is a branch of biology; it is the morphology, the physiology, the distribution, the ætiology of abnormal life.

However obvious this conclusion may be now, it was nowise apparent in the infancy of medicine. For it is a peculiarity of the physical sciences, that they are independent in proportion as they are imperfect; and it is only as they advance that the bonds which really unite them all become apparent. Astronomy had no manifest connection with terrestrial physics before the publication of the "Principia"; that of chemistry with physics is of still more modern revelation; that of physics and chemistry, with physiology, has been stoutly denied within the recollection of most of us, and perhaps still may be.

Or, to take a case which affords a closer parallel with that of medicine. Agriculture has been cultivated from the earliest times; and, from a remote antiquity, men have attained considerable practical skill in the cultivation of the useful plants, and have empirically established many scientific truths concerning the conditions under which they flourish. But it is within the memory of many of us that chemistry on the one hand, and vegetable physiology on the other, attained a stage of development such that they were able to furnish a sound basis for scientific agriculture. Similarly, medicine took its rise in the practical needs of mankind. At first, studied without reference to any other branch of knowledge, it long maintained, indeed still to some extent maintains, that independence. Historically, its connection with the biological sciences has been slowly established, and the full extent and intimacy of that connection are only now beginning to be apparent. I trust I have not been mistaken in supposing that an attempt to give a brief sketch of the steps by which a philosophical necessity has become a historical reality, may not be devoid of interest, possibly of instruction, to the members of this great Congress, profoundly interested as all are in the scientific development of medicine.

The history of medicine is more complete and fuller than that of any other science, except perhaps astronomy; and if we follow back the long record as far as clear evidence lights us, we find ourselves taken to the early stages of the civilisation of Greece. The oldest hospitals were the temples of Æsculapius; to these Asclepeia, always erected on healthy sites, hard by fresh springs and surrounded by shady groves, the sick and the maimed resorted to seek the aid of the god of health. Votive tablets or inscriptions recorded the symptoms, no less than the gratitude, of those who were healed; and, from these primitive clinical records, the half-priestly, half-philosophic, caste of the Asclepiads compiled the data upon which the earliest generalisations of medicine, as an inductive science, were based.

In this state, pathology, like all the inductive sciences at their origin, was merely natural history; it registered the phenomena of disease, classified them, and ventured upon a prognosis, wherever the observation of constant co-existences and sequences, suggested a rational expectation of the like recurrence under similar circumstances.

Further than this, it hardly went. In fact, in the then state of knowledge and in the condition of philosophical speculation at that time, neither the causes of the morbid state, nor the *rationale* of treatment, were likely to be sought for as we seek for them now. The anger of a God was a sufficient reason for the existence of a malady, and a dream ample warranty for therapeutic measures; that a physical phenomenon must needs have a physical cause was not the implied or expressed axiom that it is to us moderns.

The great man, whose name is inseparately connected with the foundation of medicine, Hippocrates, certainly knew very little, indeed practically nothing, of anatomy or physiology; and he would probably have been perplexed, even to imagine the possibility of a connection between the zoological studies of his contemporary, Democritus, and medicine. Nevertheless, in so far as he, and those who worked before and after him, in the same spirit, ascertained, as matters of experience, that a wound, or a luxation, or a fever, presented such and such symptoms, and that the return of the patient to health was facilitated by such and such measures, they established laws of nature, and began the construction of the science of pathology.—All true science begins with empiricism—though all true science is such exactly, in so far as it strives to pass out of the empirical stage into that of the deduction of empirical from more general truths. Thus, it is not wonderful that the early physicians had little or nothing to do with the development of biological science; and, on the other hand, that the early biologists did not much concern themselves with medicine. There is nothing to show that the Asclepiads took any prominent share in the work of founding anatomy, physiology, zoology, and botany. Rather do these seem to have sprung from the early philosophers, who were essentially natural philosophers, animated by the characteristically Greek thirst for knowledge as such. Pythagoras, Alcmeon, Democritus, Diogenes of Apollonia, are all credited with anatomical and physiological investigation; and though Aristotle is said to have belonged to an Asclepiad family, and not improbably owed his taste for anatomical and zoological inquiries to the teachings of his father, the physician Nicomachus, the "Historia Animalium," and the treatise "De Partibus Animalium," are as free from any allusion to medicine, as if they had issued from a modern biological laboratory.

It may be added, that it is not easy to see in what way it could have benefited a physician of Alexander's time to know all that Aristotle knew on these subjects. His human anatomy was too rough to avail much in diagnosis, his physiology was too erroneous to supply data for pathological reasoning. But when the Alexandrian school, with Erasistratus and Herophilus at their head, turned to account the opportunities of studying human structure, afforded to them by the Ptolemies, the value of the large amount of accurate knowledge thus obtained to the surgeon for his operations, and to the physician for his diagnosis of internal disorders, became obvious, and a connection was established between anatomy and medicine, which has ever become closer and closer. Since the revival of learning, surgery, medical diagnosis, and anatomy have gone hand in hand. Morgagni called his great work, "De sedibus et causis morborum per anatomen indagatis," and not only showed the way to search out the localities and the causes of disease by anatomy, but himself travelled wonderfully far upon the road. Bichat, discriminating the grosser constituents of the organs and parts of the body, one from another, pointed out the direction which modern research must take; until, at length, histology, a science of yesterday, as it seems to many of us, has carried the work of Morgagni as far as the microscope can take us, and has extended the realm of pathological anatomy to the limits of the invisible world.

Thanks to the intimate alliance of morphology with medicine, the natural history of disease has, at the present day, attained a high degree of perfection. Accurate regional anatomy has rendered practicable the exploration of the most hidden parts of the organism, and the determination during life of morbid changes in them; anatomical and histological post-mortem investigations have supplied physicians with a clear basis upon which to rest the classification of diseases, and with unerring tests of the accuracy or inaccuracy of their diagnoses.

If men could be satisfied with pure knowledge, the extreme precision with which, in these days, a sufferer may be told what is happening and what is likely to happen, even in the most recondite parts of his bodily frame, should be as satisfactory to the patient, as it is to the scientific pathologist who gives him the information. But I am afraid it is not; and even the practising physician, while no wise underestimating the regulative value of accurate diagnosis, must often lament that so much of his knowledge rather prevents him from doing wrong, than helps him to do right.

A scorner of physic once said that nature and disease may be compared to two men fighting, the doctor to a blind man with a club, who strikes into the *mêlée*, sometimes hitting the disease,

and sometimes hitting nature. The matter is not mended if you suppose the blind man's hearing to be so acute that he can register every stage of the struggle and pretty clearly predict how it will end. He had better not meddle at all, until his eyes are opened—until he can see the exact position of the antagonists, and make sure of the effect of his blows. But that which it behoves the physician to see, not indeed with his bodily eye, but with clear intellectual vision, is a process, and the chain of causation involved in that process. Disease, as we have seen, is a perturbation of the normal activities of a living body; and it is, and must remain, unintelligible, so long as we are ignorant of the nature of these normal activities.—In other words, there could be no real science of pathology, until the science of physiology had reached a degree of perfection unattained, and indeed unattainable, until quite recent times.

So far as medicine is concerned, I am not sure that physiology, such as it was down to the time of Harvey, might as well not have existed. Nay, it is perhaps no exaggeration to say, that within the memory of living men, justly renowned practitioners of medicine and surgery knew less physiology than is now to be learned from the most elementary text-book; and, beyond a few broad facts, regarded what they did know, as of extremely little practical importance. Nor am I disposed to blame them for this conclusion; physiology must be useless, or worse than useless, to pathology, so long as its fundamental conceptions are erroneous.

Harvey is often said to be the founder of modern physiology; and there can be no question that the elucidations of the function of the heart, of the nature of the pulse, and of the course of the blood, put forth in the ever-memorable little essay "De motu cordis," directly worked a revolution in men's views of the nature and of the concatenation of some of the most important physiological processes among the higher animals; while, indirectly, their influence was perhaps even more remarkable.

But, though Harvey made this signal and perennially important contribution to the physiology of the moderns, his general conception of vital processes was essentially identical with that of the ancients; and, in the "Exercitationes de generatione," and notably in the singular chapter "De calido innato," he shows himself a true son of Galen and of Aristotle.

For Harvey, the blood possesses powers superior to those of the elements; it is the seat of a soul which is not only vegetative, but also, sensitive and motor. The blood maintains and fashions all parts of the body, "idque summâ cum providentiâ et intellectu in finem certum agens, quasi ratiocinio quodam uteretur."

Here is the doctrine of the "pneuma," the product of the philosophical mould into which the animism of primitive men ran in Greece, in full force. Nor did its strength abate for long after Harvey's time. The same ingrained tendency of the human mind to suppose that a process is explained when it is ascribed to a power of which nothing is known except that it is the hypothetical agent of the process, gave rise in the next century to the animism of Stahl; and, later, to the doctrine of a vital principle, that "asylum ignorantiæ" of physiologists, which has so easily accounted for everything and explained nothing, down to our own times.

Now the essence of modern, as contrasted with ancient, physiological science, appears to me to lie in its antagonism to animistic hypotheses and animistic phraseology. It offers physical explanations of vital phenomena, or frankly confesses that it has none to offer. And so far as I know, the first person who gave expression to this modern view of physiology, who was bold enough to enunciate the proposition that vital phenomena, like all the other phenomena of the physical world, are, in ultimate analysis, resolvable into matter and motion, was René Descartes.

The fifty-four years of life of this most original and powerful thinker are widely overlapped, on both sides, by the eighty of Harvey, who survived his younger contemporary by seven years, and takes pleasure in acknowledging the French philosopher's appreciation of his great discovery.

In fact, Descartes accepted the doctrine of the circulation as propounded by "Hervæus, médecin d'Angleterre," and gave a full account of it in his first work, the famous "Discours de la Méthode," which was published in 1637, only nine years after the exercitation "De motu cordis"; and, though differing from Harvey in some important points (in which it may be noted, in passing, Descartes was wrong and Harvey right), he always speaks of him with great respect. And so important does the subject seem to Descartes, that he returns to it in the "Traité des Passions," and in the "Traité de l'Homme."

It is easy to see that Harvey's work must have had a peculiar significance for the subtle thinker, to whom we owe both the spiritualistic and the materialistic philosophies of modern times. It was in the very year of its publication, 1628, that Descartes withdrew into that life of solitary investigation and meditation of which his philosophy was the fruit. And, as the course of his speculations led him to establish an absolute distinction of nature between the material and the mental worlds, he was logically compelled to seek for the explanation of the phenomena of the material world within itself; and having allotted the realm of thought to the soul, to see nothing but extension and motion in the rest of nature. Descartes uses "thought" as the equivalent of our modern term "consciousness." Thought is the function of the soul, and its only function. Our natural heat and all the movements of the body, says he, do not depend on the soul. Death does not take place from any fault of the soul, but only because some of the principal parts of the body become corrupted. The body of a living man differs from that of a dead man in the same way as a watch or other automaton (that is to say a machine which moves of itself) when it is wound up and has in itself the physical principle of the movements which the mechanism is adapted to perform, differs from the same watch, or other machine, when it is broken and the physical principle of its movement no longer exists. All the actions which are common to us and the lower animals depend only on the conformation of our organs and the course which the animal spirits take in the brain, the nerves, and the muscles; in the same way as the movement of a watch is produced by nothing but the force of its spring and the figure of its wheels and other parts.

Descartes' Treatise on Man is a sketch of human physiology in which a bold attempt is made to explain all the phenomena of life, except those of consciousness, by physical reasonings. To a mind turned in this direction, Harvey's exposition of the heart and vessels as a hydraulic mechanism must have been supremely welcome.

Descartes was not a mere philosophical theorist, but a hard-working dissector and experimenter, and he held the strongest opinion respecting the practical value of the new conception which he was introducing. He speaks of the importance of preserving health, and of the dependence of the mind on the body being so close that perhaps the only way of making men wiser and better than they are, is to be sought in medical science. "It is true," says he, "that as medicine is now practised, it contains little that is very useful; but without any desire to depreciate, I am sure that there is no one, even among professional men, who will not declare that all we know is very little as compared with that which remains to be known; and that we might escape an infinity of diseases of the mind, no less than of the body, and even perhaps from the weakness of old age, if we had sufficient knowledge of their causes, and of all the remedies with which nature has provided us."[1] So strongly impressed was Descartes with this, that he resolved to spend the rest of his life in trying to acquire such a knowledge of nature as would lead to the construction of a better medical doctrine.[2] The anti-Cartesians found material for cheap ridicule in these aspirations of the philosopher: and it is almost needless to say that, in the thirteen years which elapsed between the publication of the "Discours" and the death of Descartes, he did not contribute much to their realisation. But, for the next century, all progress in physiology took place along the lines which Descartes laid down.

The greatest physiological and pathological work of the seventeenth century, Borelli's treatise "De motu animalium," is, to all intents and purposes, a development of Descartes' fundamental conception; and the same may be said of the physiology and pathology of Boerhaave, whose authority dominated in the medical world of the first half of the eighteenth century.

With the origin of modern chemistry, and of electrical science, in the latter half of the eighteenth century, aids in the analysis of the phenomena of life, of which Descartes could not have dreamed, were offered to the physiologist. And the greater part of the gigantic progress which has been made in the present century, is a justification of the prevision of Descartes. For it consists, essentially, in a more and more complete resolution of the grosser organs of the living body into physico-chemical mechanisms.

[1] "Discours de la Méthode," 6e partie. Ed. Cousin, p. 193.
[2] *Ibid.* pp. 193 and 211.

"I shall try to explain our whole bodily machinery in such a way, that it will be no more necessary for us to suppose that the soul produces such movements as are not voluntary, than it is to think that there is in a clock a soul which causes it to show the hours."[1] These words of Descartes might be appropriately taken as a motto by the author of any modern treatise on physiology.

But though, as I think, there is no doubt that Descartes was the first to propound the fundamental conception of the living body as a physical mechanism, which is the distinctive feature of modern, as contrasted with ancient physiology, he was misled by the natural temptation to carry out, in all its details, a parallel between the machines with which he was familiar, such as clocks and pieces of hydraulic apparatus, and the living machine. In all such machines there is a central source of power, and the parts of the machine are merely passive distributors of that power. The Cartesian school conceived of the living body as a machine of this kind; and herein they might have learned from Galen, who, whatever ill use he may have made of the doctrine of "natural faculties," nevertheless had the great merit of perceiving that local forces play a great part in physiology.

The same truth was recognised by Glisson, but it was first prominently brought forward in the Hallerian doctrine of the "vis insita" of muscles. If muscle can contract without nerve, there is an end of the Cartesian mechanical explanation of its contraction by the influx of animal spirits.

The discoveries of Trembley tended in the same direction. In the freshwater *Hydra*, no trace was to be found of that complicated machinery upon which the performance of the functions in the higher animals was supposed to depend. And yet the hydra moved, fed, grew, multiplied, and its fragments exhibited all the powers of the whole. And, finally, the work of Caspar F. Wolff,[2] by demonstrating the fact that the growth and development of both plants and animals take place antecedently to the existence of their grosser organs, and are, in fact, the causes and not the consequences of organisation (as then understood), sapped the foundations of the Cartesian physiology as a complete expression of vital phenomena.

For Wolff, the physical basis of life is a fluid, possessed of a "vis essentialis" and a "solidescibilitas," in virtue of which it gives rise to organisation; and, as he points out, this conclusion strikes at the root of the whole iatro-mechanical system.

In this country, the great authority of John Hunter exerted a similar influence; though it must be admitted that the two sibylline utterances which are the outcome of Hunter's struggles to define his conceptions are often susceptible of more than one interpretation. Nevertheless, on some points, Hunter is clear enough. For example, he is of opinion that "Spirit is only a property of matter" ("Introduction to Natural History," p. 6), he is prepared to renounce animism (*l.c.* p. 8), and his conception of life is so completely physical that he thinks of it as something which can exist in a state of combination in the food. "The aliment we take in has in it, in a fixed state, the real life; and this does not become active until it has got into the lungs; for there it is freed from its prison" ("Observations on Physiology," p. 113). He also thinks that "It is more in accord with the general principles of the animal machine to suppose that none of its effects are produced from any mechanical principle whatever; and that every effect is produced from an action in the part; which action is produced by a stimulus upon the part which acts, or upon some other part with which this part sympathises so as to take up the whole action" (*l.c.* p. 152).

And Hunter is as clear as Wolff, with whose work he was probably unacquainted, that "whatever life is, it most certainly does not depend upon structure or organisation" (*l.c.* p. 114).

Of course it is impossible that Hunter could have intended to deny the existence of purely mechanical operations in the animal body. But while, with Borelli and Boerhaave, he looked upon absorption, nutrition, and secretion, as operations effected by means of the small vessels; he differed from the mechanical physiologists, who regarded these operations as the result of the mechanical properties of the small vessels, such as the size, form, and disposition of their canals and apertures. Hunter, on the contrary, considers them to be the effect of properties of these vessels which are not mechanical but vital. "The vessels," says he, "have more of the polypus in them than any other part of the body," and he talks of the "living and sensitive principles of the arteries," and even of the "dispositions or feelings of the arteries." "When the blood is good and genuine the sensations of the arteries, or the dispositions for sensation, are agreeable. . . . It is then they dispose of the blood to the best advantage, increasing the growth of the whole, supplying any losses, keeping up a due succession, &c." (*l. c.* p. 133.)

If we follow Hunter's conceptions to their logical issue, the life of one of the higher animals is essentially the sum of the lives of all the vessels, each of which is a sort of physiological unit, answering to a polype; and, as health is the result of the normal "action of the vessels," so is disease an effect of their abnormal action. Hunter thus stands in thought, as in time, midway between Borelli, on the one hand, and Bichat on the other.

The acute founder of general anatomy, in fact, outdoes Hunter in his desire to exclude physical reasonings from the realm of life. Except in the interpretation of the action of the sense organs, he will not allow physics to have anything to do with physiology.

"To apply the physical sciences to physiology is to explain the phenomena of living bodies by the laws of inert bodies. Now this is a false principle, hence all its consequences are marked with the same stamp. Let us leave to chemistry its affinity, to physics, its elasticity and its gravity. Let us invoke for physiology only sensibility and contractility."[1]

Of all the unfortunate dicta of men of eminent ability this seems one of the most unhappy, when we think of what the application of the methods and the data of physics and chemistry has done towards bringing physiology into its present state. It is not too much to say that one half of a modern text-book of physiology consists of applied physics and chemistry; and that it is exactly in the exploration of the phenomena of sensibility and contractility that physics and chemistry have exerted the most potent influence.

Nevertheless, Bichat rendered a solid service to physiological progress by insisting upon the fact that what we call life, in one of the higher animals, is not an indivisible unitary archæus dominating, from its central seat, the parts of the organism, but a compound result of the synthesis of the separate lives of those parts.

"All animals," says he, "are assemblages of different organs, each of which performs its function and concurs, after its fashion, in the preservation of the whole. They are so many special machines in the general machine which constitutes the individual. But each of these special machines is itself compounded of many tissues of very different natures, which in truth constitute the elements of those organs." (*l.c.* lxxix.) "The conception of a proper vitality is applicable only to these simple tissues, and not to the organs themselves." (*l.c.* lxxxiv.)

And Bichat proceeds to make the obvious application of this doctrine of synthetic life, if I may so call it, to pathology. Since diseases are only alterations of vital properties, and the properties of each tissue are distinct from those of the rest, it is evident that the diseases of each tissue must be different from those of the rest. Therefore, in any organ composed of different tissues, one may be diseased and the other remain healthy; and this is what happens in most cases. (*l.c.* lxxxv.)

In a spirit of true prophecy, Bichat says, "we have arrived at an epoch, in which pathological anatomy should start afresh." For as the analysis of the organs had led him to the tissues, as the physiological units of the organism; so, in a succeeding generation, the analysis of the tissues led to the cell as the physiological element of the tissues. The contemporaneous study of development brought out the same result, and the zoologists and botanists exploring the simplest and the lowest forms of animated beings confirmed the great induction of the cell theory. Thus the apparently opposed views, which have been battling with one another ever since the middle of the last century, have proved to be each half the truth.

The proposition of Descartes that the body of a living man is a machine, the actions of which are explicable by the known laws of matter and motion, is unquestionably largely true. But it is also true, that the living body is a synthesis of innumerable physiological elements, each of which may nearly be described, in Wolff's words, as a fluid possessed of a "vis essentialis," and a "solidescibilitas"; or, in modern phrase, as protoplasm susceptible of structural metamorphosis and functional metabolism: and that the only machinery, in the precise sense in which the Cartesian school understood mechanism, is, that which co-ordinates and regulates these physiological units into an organic whole.

[1] "De la Formation du Fœtus."
[2] "Theoria Generationis," 1759.

[1] "Anatomie générale," I. p. liv.

In fact, the body is a machine of the nature of an army, not of that of a watch, or of a hydraulic apparatus. Of this army, each cell is a soldier, an organ a brigade, the central nervous system head-quarters and field telegraph, the alimentary and circulatory system the commissariat. Losses are made good by recruits born in camp, and the life of the individual is a campaign, conducted successfully for a number of years, but with certain defeat in the long run.

The efficacy of an army, at any given moment, depends on the health of the individual soldier, and on the perfection of the machinery by which he is led and brought into action at the proper time; and, therefore, if the analogy holds good, there can be only two kinds of diseases, the one dependent on abnormal states of the physiological units, the other on perturbation of their co-ordinating and alimentative machinery.

Hence, the establishment of the cell theory, in normal biology, was swiftly followed by a "cellular pathology," as its logical counterpart. I need not remind you how great an instrument of investigation, this doctrine has proved in the hands of the man of genius, to whom its development is due; and who would probably be the last to forget that abnormal conditions of the co-ordinative and distributive machinery of the body are no less important factors of disease.

Henceforward, as it appears to me, the connection of medicine with the biological sciences is clearly defined. Pure pathology is that branch of biology which defines the particular perturbation of cell life, or of the co-ordinating machinery, or of both, on which the phenomena of disease depend.

Those who are conversant with the present state of biology will hardly hesitate to admit that the conception of the life of one of the higher animals as the summation of the lives of a cell aggregate, brought into harmonious action by a co-ordinative machinery formed by some of these cells, constitutes a permanent acquisition of physiological science. But the last form of the battle between the animistic and the physical views of life is seen in the contention whether the physical analysis of vital phenomena can be carried beyond this point or not.

There are some to whom living protoplasm is a substance even such as Harvey conceived the blood to be, "summâ cum providentiâ et intellectu in finem certum agens, quasi ratiocinio quodam"; and who look, with as little favour as Bichat did, upon any attempt to apply the principles and the methods of physics and chemistry to the investigation of the vital processes of growth, metabolism, and contractility. They stand upon the ancient ways; only, in accordance with that progress towards democracy which a great political writer has declared to be the fatal characteristic of modern times, they substitute a republic formed by a few billion of "animulæ" for the monarchy of the all pervading "anima."

Others, on the contrary, supported by a robust faith in the universal applicability of the principles laid down by Descartes, and seeing that the actions called "vital" are, so far as we have any means of knowing, nothing but changes of place of particles of matter, look to molecular physics to achieve the analysis of the living protoplasm itself into a molecular mechanism. If there is any truth in the received doctrines of physics, that contrast between living and inert matter, on which Bichat lays so much stress, does not exist. In nature, nothing is at rest, nothing is amorphous; the simplest particle of that which men in their blindness are pleased to call "brute matter" is a vast aggregate of molecular mechanisms, performing complicated movements of immense rapidity and sensitively adjusting themselves to every change in the surrounding world. Living matter differs from other matter in degree and not in kind; the microcosm repeats the macrocosm; and one chain of causation connects the nebulous original of suns and planetary systems with the protoplasmic foundation of life and organisation.

From this point of view, pathology is the analogue of the theory of perturbations in astronomy; and therapeutics resolves itself into the discovery of the means by which a system of forces competent to eliminate any given perturbation may be introduced into the economy. And, as pathology bases itself upon normal physiology, so therapeutics rests upon pharmacology; which is, strictly speaking, a part of the great biological topic of the influence of conditions on the living organism and has no scientific foundation apart from physiology.

It appears to me that there is no more hopeful indication of the progress of medicine towards the ideal of Descartes than is to be derived from a comparison of the state of pharmacology, at the present day, with that which existed forty years ago. If we consider the knowledge positively acquired, in this short time, of the *modus operandi* of urari, of atropia, of physostigmin, of veratria, of casca, of strychnia, of bromide of potassium, of phosphorus, there can surely be no ground for doubting that, sooner or later, the pharmacologist will supply the physician with the means of affecting, in any desired sense, the functions of any physiological element of the body. It will, in short, become possible to introduce into the economy a molecular mechanism which, like a very cunningly contrived torpedo, shall find its way to some particular group of living elements, and cause an explosion among them, leaving the rest untouched.

The search for the explanation of diseased states in modified cell life; the discovery of the important part played by parasitic organisms in the ætiology of disease; the elucidation of the action of medicaments by the methods and the data of experimental physiology; appear to me to be the greatest steps which have ever been made towards the establishment of medicine on a scientific basis. I need hardly say they could not have been made except for the advance of normal biology.

There can be no question then as to the nature or the value of the connection between medicine and the biological sciences. There can be no doubt that the future of Pathology and of Therapeutics, and therefore that of Practical Medicine, depend upon the extent to which those who occupy themselves with these subjects are trained in the methods and impregnated with the fundamental truths of Biology.

And, in conclusion, I venture to suggest that the collective sagacity of this Congress could occupy itself with no more important question than with this: How is medical education to be arranged, so that, without entangling the student in those details of the systematist which are valueless to him, he may be enabled to obtain a firm grasp of the great truths respecting animal and vegetable life, without which, notwithstanding all the progress of scientific medicine, he will still find himself an empiric?

ON THE VALUE OF PATHOLOGICAL EXPERIMENTS[1]

As reporter on Medical Education at the last International Medical Congress held in Amsterdam, I raised the question how far the experimental method is necessary to instruction; and the result at which I arrived was that the use of this method to its greatest extent, and especially of vivisection, is an indispensable means.[2] In a still higher measure, however, I had to raise into prominence the importance of this method in research; and, in opposition to those who, with constantly increasing vehemence, brought accusations against the experimental investigators on account of the direction and method of their researches, I was able to say, with the lively assent of the numerous members of the Congress, and without one word in contradiction: "All those who attack vivisection as a means of science have not the least idea of the importance of the science, and much less of the importance of this aid to knowledge."

In the two years which have since passed away, the agitation of the opponents has grown both extensive and important in its object. One country after another has been drawn into their net, and international combinations have been formed, in order by united force to obtain greater results. No increase of satisfaction has been produced by the concessions made in 1876 by the legislation in England. The demands have increased: a petition from the new Leipsic Society for the Protection of Animals, dated March 8 of the present year, desired of the German Reichstag the enactment of a law by which "cruelty to animals under the pretext of scientific research" should be punished "with imprisonment for periods of not less than five weeks to two years, and with simultaneous deprivation of civil rights." All, indeed, do not go so far. Many do not demand that all experiments on living animals should be at once suppressed, but that there should be limitations, some demanding more, others less. But even these do not make it secret that this concession is only provisional; and they demand that even the official laboratories of the universities should be placed under

[1] Address given at the International Medical Congress by Rudolf Virchow, M.D., Professor in the University of Berlin. The Editor of the *British Medical Journal* has kindly allowed us to use his translation of Prof. Virchow's address.

[2] Congrès Périodique International des Sciences Médicales, 6 Session, Amsterdam (1879), 1880, p. 146, *Archiv für Pathol. Anat.*, Band lxxxv. Heft 3.

the control of the members of the Society for the Protection of Animals, so that the members may be at liberty to enter the laboratories at any time.

It would be a mischievous delusion to believe that this movement is without prospect of success, and devoid of danger because of its manifest exaggeration. On the contrary, unmistakable signs indicate that it has gained powerful allies, and that there is an increasingly impending danger in many countries that even the State institutions, created expressly for the purpose of experiment, may have the scientific freedom of their methods attacked. So much the more does it seem to be incumbent on the representatives of medical science to defend their position, and to meet international attacks by international weapons. *The most powerful weapon, however, is truth*; and here, above all, *truth founded on competent knowledge*. If we cannot demonstrate our good right before all the world, and come to a mutual agreement on the ground of this right, our cause must henceforth be looked on as a lost one.

The attacks which are directed against us fall, when closely examined, into two categories, according to the principal point. On the one side it is alleged that the experimental method—yea, modern medicine altogether—is materialistic, if not nihilistic, in its ultimate object; that it offends against sentiment, against morals. On the other side it is denied that the introduction of experiments on animals has had any actual use, that medicine has been really promoted thereby, and especially that the cure of diseases has in consequence made any recognisable progress. Even those who admit that there has been some progress, yet believe that just as much information could have been imparted by anatomy alone as by experiments on living animals.

Such objections are not new to one who knows the history of medicine. For hundreds of years, on similar or identical grounds, the dissection of human bodies was impeded, and anatomists were confined to the dissection of dead animals; if, indeed—as was done by Paracelsus, the contemporary of Vesalius—the insulting question were not asked, whether anatomy was of any use at all. The feeling of the masses was raised against the dissection of human bodies; and it is known that, at the commencement of the fourteenth century, the church for the first time gave permission for this to be done, but only under limitations which were still greater than those under which the larger number of our modern opponents would permit vivisection. It was no accident that the period of the reformation in the church first created for the great Vesalius a free field, so that he might test the truth of Galen's traditional dogmata by his own investigation of human bodies, and place true human anatomy in the stead of that anatomy of animals, which had during centuries formed the groundwork of all medical ideas on the internal arrangement of man.

And now, first of all, pathological anatomy—what obstacles it has had to overcome even in the present time! Nothing is more instructive in this respect than the narrative which Wepfer, the celebrated discoverer of the hæmorrhagic nature of ordinary apoplexy, gives of the acts of enmity with which he was persecuted when—it was towards the middle of the seventeenth century—the council of the town of Schaffhausen had allowed him to dissect the bodies of those dying in the hospital. The only reply which he made to those who said to him that it is injurious and disgraceful to soil his hands with blood and sanies, was, that he could cleanse his hands with some water; but that much more disgraceful and injurious is ignorance of anatomical facts, which inflicts on inexperienced physicians and surgeons a disgrace that not the Rhine, not the ocean itself can wash away.[1] Hence the study of anatomy is much rather to be praised, and to be supported by those who exercise the executive power in the State.

In fact, one Government after another has recognised the decided importance of anatomical science. As far as the civilised world extends, so far at the present day are human bodies dissected. Even the laity comprehends that, without the most accurate knowledge of the structure of the human body and of the changes which disease and recovery produce in it, skilled action on the part of the physician is impossible. Any one who can only take a general survey of the history of science, must know that both the greatest epochs of the resuscitation and reformation of medicine commenced with the definite establishment of both the principal branches of human anatomy, and were even essentially brought about thereby. In the sixteenth century it was physiological anatomy which brought about the definitive victory of empiricism over dogmatism, of science over tradition; in the eighteenth century it was pathological anatomy which replaced mysticism by realism, speculation by necropsy, obscure groping and guessing by systematic thought. The opponents indeed spoke of materialism; but Harvey has rightly said: "Sicut sanorum et boni habitas corporum dissectio plurimum ad philosophiam et rectam physiologiam facet, ita corporum morbosorum et cacheticorum inspectio potissimum ad pathologiam philosophicam.[1]

Antiquity had only one time in which a powerful effort was made for the independent development of human anatomy. It was the time of the Alexandrian School, in the third century B.C., when Erasistratus and his companions, under the protection of the Ptolemies, undertook the first regular dissections of human bodies. The school existed only a short time, and yet it caused the first perceptible agitation of the humoral system of pathology. With the more accurate knowledge of the arrangement of the nerves there grew up a new and more powerful generation of solidists; the empirics raised themselves against the dogmatists, and, though again soon enough subdued, they left behind them as a lasting inheritance the consideration that there is a certain limit to human piety, that the right of the individual to the preservation of the integrity of his body is interrupted by death, and that the veil which covers the mystery of life cannot be raised without the forcible destruction of the connection of the several parts of the body. It is this thought which, as finally realised, has brought forth modern medicine. But, eighteen centuries after the Alexandrian School, the impress of the humoral system of pathology still held independent sway in medicine. Of any positive progress in pathology during that long period nothing can be said. For Bacon has excellently said, in his "Novum Organum," "Quæ in Naturâ fundata sunt, crescunt et augentur: quæ autem in opinione, variantur, non augentur." The old humoral pathology was incapable of development, because it was not founded on nature, but on dogmata. From however different origins they had sprung, Galenism combined everywhere with orthodoxy: among the Arabians with Islam, in the west with Christianity; and it required the powerful movement of the Reformation to burst the chains within which antiquated custom and hierarchical schooling had fettered the thoughts even of physicians. From Erasistratus to Vesalius, and at last to Morgagni, is such an immense stride that it cannot remain concealed even from the weakest eye. Not only the outer form, but the whole nature of medicine has been thereby changed. If one follows Vesalius, yea, even Morgagni, in speaking of the humoral pathology as among still-existing things; if I myself am yet obliged to contend against Rokitansky, the last of the pronounced humoral pathologists, it must still not be forgotten that that was no longer the humoral pathology of Galen or Hippocrates. The four "cardinal juices" Paracelsus had already buried; modern medicine recognises only the actual juices which flow in the vessels, and thence penetrate into the tissues. This modern humoral pathology was essentially blood-pathology (hæmatopathology). In name only does it agree with the humoral pathology of the ancients: in reality, it is quite another thing.

But even hæmatopathology is now happily overcome, and indeed, again, through a proper direction of anatomical study. Since the first but very uncertain researches in the territory of so-called general or philosophical anatomy which Bichat began in the commencement of the present century, down to the more and more rapid advances which the present time has made by means of the microscope, in the knowledge of the more minute processes of healthy and diseased life, attention has been constantly more and more turned from the coarser relations of whole regions and organs of the body to the tissues of which those organs are constituted, and to the elements which again are the efficient centres of activity within those tissues. Immediately after Schwann had demonstrated the importance of cells in the development of the tissues, Johannes Müller and John Goodsir made the happiest applications of the new view to pathological processes; and, looking back to a period in which we ourselves have lived, and which embraces little more than a generation of man, we may now say that never before was there a time when a similarly great zeal in research, and a comparable—though only approximately so—progress in science and knowledge, has

[1] Joh. Jac. Wepfer. "Observ. Anat. ex Cadaveribus eorum quos sustulit Apoplexia." Schaffhausii 1658. "Præfatio: Turpior et damnosior rerum anatomicarum ignorantia est, quæ imperitis Medicis et Chirurgis ignominiam parit, quam nec Rhenus, vec Oceanus abluere potest."

[1] "Guil. Harveji Exercit. Anat." ii., "De Motu Cordis et Sanguinis Circulatione." Roterodami, 1671, p. 174.

spread among physicians. The multiplication of the powers of labour, the constantly increasing emulation in researches, the unmistakable increase in the depth of the questions proposed—all these are phenomena of the most gratifying nature; and one would be very ungrateful if he would not acknowledge that these were in a considerable measure to be ascribed to the improvements in the means of instruction and to the multiplication of laboratories.

No one can be more disposed to concede the high value of anatomical studies to the development of medicine, than one who has made it a part of the task of his life to place anatomy and histology in that commanding position in the recognition of his contemporaries which they deserve. Nothing lies further from me than to discourage those who still expect the greatest benefit to the practice of medicine to arise from following out these studies. May indeed the growing youth, who will have to follow us in assuring the progress of medicine, learn from our example how useful it is to lay the true foundation of our science in anatomy. Assuredly much of that which remains dark to us will then be rendered clear.

But we must not allow ourselves to be forced back on this way as the only permissible one. Were the attempt to hinder totally or in great part researches on living animals to become successful, the same procedure which has been now entered on against vivisection would also be commenced against mortisection. There would no longer be societies for the protection of animals, which we see opposed to us, but societies for the protection of human bodies. There would no longer be thunderings against the tormentings of animals, but against the desecration of corpses. Under the standard of humanity, which is just now unfurled even for animals, there would be preached in a still more impressive manner the campaign against the barbarity of medical men. People would appeal to the feeling of the masses—to the mother on behalf of the body of her child, the son on behalf of the dear remains of his parents. It would be proved that the dismembering of human bodies is injurious to morals and opposed to Christianity. It would be shown that the anatomy of man is useless for the treatment of disease; and perhaps there would be found ignorant or timid or egotistical medical men who would come forth as witnesses against science. The mildest of our opponents would perhaps propose to us the compromise that we should again make the dissection of animals the foundation of instruction. In short, we should be thrown back to the time before Mondini, before Erasistratus.

Such thoughts are by no means the productions of an alarmed fancy. The study of history teaches us sufficiently that victorious fanaticism knows no limits. It desires to heap to the full the measure of its victories; and, even when the traders are contented, the irritated masses press on to obtain the whole results. It is indeed not at all necessary for us to go back to antiquity in order to bring before our eyes the condition of such minds. In no country of modern time are there wanting examples which are recognisable by the eye; for, along with the societies against "scientific tormentors of animals," there exist everywhere, but mostly in a more unassuming form, brotherhoods and associations of all kinds which labour most zealously against the scientific examination of dead bodies. It needs only an impassioned and exciting agitation, such as is now going on against the "torture chambers of science," to denounce to popular indignation the dissecting-rooms as places where the youths under instruction are made barbarous. Whoever undertakes, with the same extravagant fancy as is now used in delineating the physiological laboratory, to describe the *post-mortem* examination of a man, or an anatomical theatre, will not fail to have readers who will turn away with horror and amazement at the misdeeds of anatomists.

In vain will an appeal be made to the fact that not one single school of medicine has existed which has, without a fundamental knowledge of anatomy, established lasting advances in the science or the art of healing. The homœopaths and the so-called nature-doctors (*Naturärzte*), who indeed are already on the scene to strengthen the ranks of the anti vivisectors, will step forth and praise their results. Scepticism, which, from time to time grasps about even in medical circles, and which only too easily finds there followers who have in vain called on medical aid for themselves or their belongings—it will scornfully point out how often the physician is powerless against disease. Therapeutics will be thrown aside as useless lumber; and it will be pointed out to us, as is now already done in the petitions of the societies for the protection of animals, that therapy is to be replaced by hygiene, the treatment of individual patients by general measures of public sanitation. And the attempt will then be made to excite the belief that prophylaxis can exist without anatomy or experiments on animals.

In so large an assembly of medical men as this is, a glance at these present teaches in how many special directions the medicine of to-day has gone. Not every one of these directions is in like measure and as constantly in want of all the means of inquiry and scientific preparation, which are indispensable to cure disease as a whole. Hence, from time to time, a perceptible one-sidedness becomes manifest in certain of these special arrangements. One believes in his own sufficiency, and looks with indifference, sometimes with a kind of polite contempt, on the rest of medicine. Even the truly scientific studies are not exempt from such one-sidedness; on the contrary, human pride, the tendency to over-estimation of oneself, prevail more readily in these than in partial disciplines. We ourselves have seen that organic chemistry, by a most partial use of a very moderate store of knowledge, has made the attempt—and indeed not without some temporary result—to prescribe its laws to medicine: and that numerous practical physicians, unmindful of the history of our science, have in fact sought safety in a new kind of iatro-chemistry. Yes, I have a very lively remembrance of the fact that, when I myself was entering on the scientific career, the hope of giving a purely physical aspect to biology was so powerful, that every attempt at morphological study was treated as something antiquated.

We have not allowed ourselves to be prevented by this from carrying on anatomical research with every exertion; and we are now in the happy position of seeing it everywhere acknowledged, that every advance in minute anatomy sees behind it an advance in physiological knowledge. Physiologists themselves are more and more becoming also histologists. No one however must say that physiology is becoming totally dissolved in histology. No attempt must be made to replace one special subject by another. What is necessary to all branches of medical science in general is the *knowledge of life*. But this can as little be attained by a simple external examination of the living as by a partial investigation of the dead. It can be reached by no single study or specialty; it is much rather the collective result of the cultivation of all individual branches of science.

What is to be attained by a mere external examination of the living body has been thoroughly taught by the older medicine. For centuries sick and healthy have been observed with assiduous diligence, and in fact most valuable material has been collected in the most ingenious manner; but, on the whole, no advance has been made beyond "symptoms." What was perceived were the signs of something internal which was not perceived—indeed the possible perception of which was hitherto doubted. Life itself stood as it were outside observation; it was only a subject of speculation. Intellectual formulæ were laid down, spiritualistic or materialistic, according to the general tendency of the mind of the individual or of the time; but all agreed in the conviction, that life itself is a transcendental and metaphysical problem. For the practical physician, knowledge that was founded in fact began with symptomatology; for disease as such was apparently not less transcendental than life itself, whose antitype it constituted.

How has it now come to pass, that symptomatology has entirely lost the high position in which it still stood little less than a generation ago, to such an extent that in most universities it is no more taught as a specialty? Have symptoms no more any importance for the physician? Can a diagnosis be made without a knowledge of symptoms? Certainly not. But, for the scientific physician, the symptoms are no more the expression of a hidden power, recognisable only in its outer workings: he searches for this power itself, and endeavours to find where it is seated, in the hope of exploring even the nature of its seat. Hence, the first question of the pathologist and of the biologist in general is, Where? That is the anatomical question. No matter whether we endeavour to ascertain the place of the disease or of life with the anatomical knife, or only with the eye or the hand; whether we dissect or only observe, the method of investigation is always anatomical. For this reason, the thoroughly logical founder of pathological anatomy named his fundamental book "De Sedibus Morborum"; and hence this book became the starting-point of a movement which, in a few decades, has changed the entire aspect of science.

This change has been carried out to the greatest extent in ophthalmic surgery. Who could limit himself to perceiving

that modern ophthalmology has scarcely a single point of similarity with that of the last century? Who contents himself with the symptom of amaurosis? Who despairs of recognising in it the existence of glaucoma? Every ophthalmic surgeon has in his hands the means of studying the thing itself, and not merely its signs. Even the anti-vivisectors acknowledge that ophthalmology is a study that is capable of effecting something. But they forget that every organ of the body is not so favourably placed and arranged for the observation of its inner processes as is the eyeball. Since the wonderful discovery of the ophthalmoscope, anatomical analysis, even without the use of the knife, has become capable of penetrating so far into the individually remote, that we can immediately observe and study by themselves the smallest features of the fundus oculi, even, indeed, its single cells, or groups of cells, just as in an artificial preparation of an eye that has been excised. But it must not be forgotten that long anatomical and physiological studies have been a necessary preliminary to the interpretation of that which is now so easily perceived. The structure, arrangement, and function of each single part had first to be laboriously established before it was possible, by a transitory glance at the altered tissue, to recognise what is especially changed; and no medical man will attain to a true comprehension of the essence of these changes if he have not previously learned to recognise most accurately the anatomical and physiological nature and the possible pathological changes of the individual constituent parts of the eye.

They speak lightly who object to us, that not all the branches of medicine stand on the same height with ophthalmology. That will never be the case. Just as it is easier to explore the sea in its depths than the solid land, so will the most transparent organ of the body always be the most convenient place for medical diagnosis and treatment. While it is possible to observe without difficulty a cysticercus in the hinder part of the retina, one will always be taught to bring a cysticercus of muscle or a trichina in a patient to light by vivisection. Never can it be required that every medical specialty should altogether equal ophthalmology in security of treatment and diagnosis; but any measure of success can only be sought in the use of the ophthalmological method in a corresponding manner in the other special departments. This method, however, is anatomical, or, as it has otherwise been expressed, localising.

With this, we have reached the point which denotes the boundary between ancient and modern medicine. *The principle of modern medicine is localisation.* To those who still constantly ask of what use modern science has been to practical medicine, we can simply point out that every branch of medical practice has accommodated itself to the principle of localisation, not only in pathology, but also in therapeutics, and that thereby the greatest benefit has accrued to the sick. It is quite superfluous to seek out single examples in order to show what profit the new knowledge has brought. Such examples are abundant. But we do not require them, for we can point to the general character of modern medicine. All those studies which already at an earlier period had a natural tendency to localisation, such as special surgery and dermatology, have in this way been raised to their present state of perfection. Those, however, which have retained from the old humoral pathology a tendency to the establishment of generalising formulæ gradually renounce the favourite tradition; and the fact is more and more comprehended, that generalisation in truth is nothing else than *multiplication of foci*, and that the cure of a so-called general disease signifies just as much as the eradication of a single focus. That was in fact a reform in head and limbs; and he who has not grasped it ought not to say that he has consciously followed the progress of science.

The notion of the general validity of the doctrine of the localisation of disease and of the multiplication of foci of disease in the same individual, stands, as was often objected to me in the beginning of my career as a teacher, in strict opposition to the idea of the *unity of disease*, or, as it is expressed in customary language, to the *ens morbi*. My former colleagues still retained large portions of this idea; they believed that the practical physician entered into arbitrary, and therefore dangerous, speculations, when, in the presence of a single case of disease, he assumed the disease to be a plurality. To me it seems rather the reverse; that the physician enters on a fruitless project (*schematismus*), and one dangerous to his patients, if he suppose each individual case of disease to correspond to the opinion of his school or his own private view, and calculate his prognosis and treatment thereby. Meanwhile, these considerations, derived from medical practice, on the *utility* of a certain way of perceiving disease, can lead to no decision as to its *truth*, and yet, at this result only is it possible to arrive. How shall we establish it?

All the world is at one on this point, that disease presupposes life. In a dead body there is no disease. With death, life and disease disappear simultaneously. This consideration led the older physicians to assume disease to be a self-living or even animated essence, which took its place in the body along with the vital principle. Many went so far as to define disease as a combat between two contending principles, the innate life and an intrusive foreign body. But all came back to life as a preliminary condition of disease. The view was first lost in the old Leyden school; from Boerhaave emanated the dogma, which his pupil Gaubius placed at the head of his long-used "Handbook of General Pathology," the first written on the subject: *Morbus est vita præter naturam.* Disease is life itself; or, to speak more correctly, it is a portion of life.

This assumption displaced the unfortunate dualism which had so long dominated medicine; or, at least, it ought to have displaced this dualism between life and disease. If, nevertheless, it has not completely done this, and if more than a century has been required to break up the still constantly existing dissonance, the reason lies in the difficulty of finding a satisfactory conception of life. And here the question must not be passed by, Where has life its special seat? *Ubi sedes vitæ?* John Hunter went back to the ancient view, already expressed in the Mosaic formula: "The life of the body is in its blood." Flourens believed that he had found the seat of life, the *nœud vital*, in the central nervous system, in the medulla oblongata. The one, like the other, found himself obliged to institute experiments on living animals for the investigation of this difficult question. Therewith the experimental method in the more strict sense began to pass into the practice of pathologists. Vivisection became a regular aid to research.

Certainly the consideration that a knowledge of life can only be obtained on the living being was long present. Beyond doubt it was already formed in antiquity. But it is difficult to determine with accuracy the time when it first became practically active. Uncertain statements only on the subject are available. Zacharias Sylvius, a physician of Rotterdam, who wrote the preface to the Dutch edition of Harvey's "Exercitationes," calls to mind the tale of Democritus, whom the Abderites regarded as insane, because they saw him constantly engaged in vivisection; when however the great Hippocrates was sent for to cure him, he fully recognised the value of his proceedings, and declared that all the Abderites were lunatics, and that Democritus alone was sane.[1] Probably this story has been narrated at the expense of the good Abderites; but it still shows that vivisection already "lay in the air." I will not attempt to decide whether it is true that the teachers in the Alexandrian school actually availed themselves of the permission of their king to dissect criminals. The only conclusion which I can derive from these tales is that researches on animals must surely have at that time been already practised. For whoever reflects on the vivisection of men must acknowledge that, especially at a time when the anatomy of animals formed the foundation of medical study, vivisection had certainly been previously done on animals. In the school of the empirics which proceeded from that of Alexandria, and in which necropsy was taught as the chief means of knowledge, experiment also appears as having a recognised claim; in the celebrated formula, which has been called the tripod of the empirics, and which served as the programme of their school, deliberately-planned experiment is expressly mentioned (φυσικὴ ἡ αὐτοσχεδίη τήρησις). Only it is not evident to what extent this research on living animals was carried on. Hence it is also unprofitable to inquire what advantage of any kind ancient medicine derived from vivisection.

In fact, the first great and distinctive example of successful vivisection which the history of medicine knows is that of William Harvey. The foundation of the doctrine of the circulation, which in the main was experimental, has radically changed the whole direction of the thoughts of physicians.

[1] "Harveji Exercit. Anat." Roterod., 1671. "Præfatio: Democritus solertissimus operum naturæ perscrutator, cum assidue secandis animalibus occuparetur, existimatus fuit insanus ab Abderitis; qui miserati sortem hominis advocarunt Hippocratem, ut illi medicinam faceret mentemque alienatam restitueret. Rogatus decurrit et offendit Democritum animalia secantem, quo spectaculo mirum in modum oblectatus, omnes Abderitas insanire pronuntiavit, solum sapere Democritum."

Had we this one example alone it would be sufficient to prove brilliantly the utility, yea, the indispensability, of vivisection. Never has a dogma firmly established by the tradition of centuries and every kind of authority, which in truth formed the central point of a powerful and generally acknowledged system, been annihilated with such a headlong downfall. In complete recognition of the importance of such a man, Albert von Haller said that Harvey's name was the second in medicine, that of Hippocrates being the first. But it was a difficult step, to advance a new and unheard-of doctrine which interfered with science in so revolutionary a manner. Having hesitated long whether he should publish his discovery, and when he at last carried his resolution into effect, the great vivisector cried: "Utcumque sit, jam jacta est alea, spes mea in amantium veritatis et doctorum animorum candore sita" (*loc. cit.* p. 81).

It is certainly due, even in the present day, to the purity of a truth-loving and cultivated mind, to exonerate Harvey from the reproach of heartlessness, perhaps of brutality, of which our anti-vivisectionists are so liberal. His new knowledge had cost the lives of many animals; he started, as he himself says, "ex vivorum (experiendi causâ) dissectione, arteriarum apertione disquisitionæque multimoda." And yet that was the least thing with which he was reproached; even kings at that time were so little tender-hearted, or, I may say, with an opponent, were so brutalised, that King Charles I. found pleasure in seeing the experiments of his body-physician.

On the other hand, after Malpighi had, still in the same century, demonstrated the flow of blood in the capillaries of living animals, and after our century has added the knowledge of the existence of an actual capillary wall, the doctrine of the circulation appears so self-evident, it has so thoroughly entered into the ideas of all, that it already requires a peculiarly-trained mind to comprehend the opinion of the older physicians on the local relations of the current of the blood. Whoever goes unprepared to the study of the medical classics, falls from one misunderstanding into another. The ideas of the nature of local processes are entirely changed, and yet the circulation, the capillary certainly more than that of the larger vessels, stands in the foreground of pathological interest almost more than in truth it should. The widely comprehensive doctrine of inflammation and new growth, within which nearly the greater part of practical cases occur, was founded on experiments on the capillary circulation; not less so was the doctrine of the cure of local diseased processes of most varied kinds.

Even the worst opponents of vivisection recognise Harvey's services. But, say they, since then, nothing more of importance has been accomplished by vivisection. They do not know that it is precisely that department of the doctrine of the process of the circulation which embraces the vital properties of the organs of circulation, which is entirely unmentioned by Harvey.

On what does the activity of the heart depend? What influence do the vessels exert on the propulsion and distribution of the blood? What share falls to the arteries, what to the veins, what to the capillaries? All these questions are of the highest practical importance, and none of them can be investigated otherwise than by experiments on animals. But Harvey could not attack these questions, because in his time minute anatomy was not yet developed. Who knew anything of the nerves of the heart, or of the vessels? Who had any notion as to the participation in the manifestations of the action of the heart and blood-vessels, on the part of the nerves, which supply the parietal structures, especially the fine muscles?

An interval of two centuries again intervened before Edward Weber, by experiment on the vagus nerve in a living animal, first revealed the mystery of the innervation of the heart; and this, again, in a quite unexpected and unprecedented manner; and before our now so much abused friend Claude Bernard likewise showed on a living animal the influence of the sympathetic nerve on the vessels of the head and neck.

Now for the first time, and through numerous other experiments which have tended to this end, we understand the circulation in its special characters. The pulse, that so highly treasured object of the old symptomatology, allows itself to be interpreted. It is to us no longer the sign of this or that disease, but the sign of the existence or non-existence of certain activities, of strength or weakness, of irritation or relaxation of certain tissues. Now for the first time we can understand in its individual peculiarities the action of the heart itself and the operation on it of certain substances—*e.g.* cardiac poisons; and it is not almost alone the department of diseases of the valves, to which alone, and with a scorn that cannot be rightly understood, the anti-vivisectors point on account of their incurability, but also the department of febrile diseases, which we are in a position to survey as well with regard to their symptoms as to their nature and their results.

The length of the interval of time between Harvey and the more recent experimenters on the innervation of the vascular apparatus is explained by the circumstance that in that intermediate time two entirely new studies had to be created, to both of which the discovery of the circulation was an impulse and a preliminary condition. I mean physiology and general pathology; thus, indeed, both these studies, which are to be regarded as the chief support of the experimental method, and which it was originally the custom to comprise under the name of "Institutiones Medicæ." Hermann Boerhaave had, in his professorship, combined them, and, indeed, had even united them with practical medicine; under his pupils the division of labour commenced, and the formal separation of the studies. Haller was the special creator of physiology. His experiments went first in the direction of exploring the vital properties of individual parts of the body, of single tissues, as would now be said. Among these properties, following the distinguished Glisson, a man, it seems to me, not even now sufficiently honoured in his country, he assigned a prominent place to irritability. It would lead me too far if I in this place desired to attempt to show forth individually these memorable researches, the comprehension of which was rendered extremely difficult by the then not yet sufficiently complete explanation of the motions "irritability" and "contractility." For our purpose it is sufficient to point out that here for the first time nerve and muscle, the two most highly developed and thereby most energetic portions of the animal body, were made the subjects of experiment with regard to their special forms of activity. Contraction and sensibility appear as the special signs of living activity. Therewith the question of the basis of living activity was so nearly approached that Gaubius, who at the same time laid the foundations of general pathology, indicated the vital force as the source of contraction, without going further.[1]

From these beginnings was developed, at first in a very obscure and equally unprofitable manner, especially clouded by speculative vitalism, the doctrine of life in its modern form. It has required much longer labours, mostly experimental, to arrive at a great and practical result in spite of all deviations. From the conception of irritability, originally created by Glisson, that of contractility has gradually become separate: and the contrast in which Haller placed irritability and sensibility with regard to each other has been dissolved, by the fact that contractility and sensibility are regarded as two special forms of expression of life connected with various elements, and are subordinated to irritability as the general expression. In this sense, irritability and vitality are nearly identical. Both are properties of tissue, and as such directly or indirectly accessible to treatment and experiment.

In fact, experimentation is now rather directed to the tissue itself. Galvani's discovery of electric contractions, the labours of Alexander von Humboldt on irritated muscle and nerve-fibre, and many other contemporaneous researches, afford evidence of the changed direction in which the new biology laboured. More and more sank down the mysticism of the spirits of life and of disease, the speculation as to an individual vital force; and from generation to generation medicine assumed more and more the character of a real natural science. The obscurity which had dominated especially the nervous system, disappeared under the common labours of anatomists and experimenters; and especially since Charles Bell taught the difference of the nerves hitherto considered as similar in nature, and thereby opened the road to research on the special importance and power of the single divisions of the central nervous system, one work after another has appeared, which has diffused new light on this difficult and complicated subject. It is impossible to go through all these works on this occasion, and it would be superfluous in an assembly of such accomplished men, many of whom have themselves laboured in this glorious work.

I will now only briefly point out that among these labours a constantly clearer and more triumphant idea has advanced, which in its beginnings reaches far back into past time—namely, the idea of the proper life (*vita propria*) of the tissues. Every new form of experiment which is devised renders new parts accessible

[1] Gaubius, "Institut. Path. Med.," p. 71. "Vis vitalis solidi est, qua illud ad contactum irritamenti se contrahit."

to scientific examination, and with each step in advance we become more clearly convinced that life, regarded as a great unit in the established sense, is a pure fiction, arising from the observation that in the hierarchical organisation of the human body certain organs attain so elaborate a structure, and therewith so great importance, that they with complete right merit the name of vital organs. And as among these organs the *medulla oblongata* possesses the greatest importance, it is easily comprehensible that the idea should arise that it might really be the seat of life. But we know now that life is a collective functional action of all parts of the higher or vital, as well as of the lower and less important; and that there is no one seat of life, but that every true elementary part, especially every cell, is a seat of life. In biological research, also, as well as in pathological, we have arrived at a multiplication of foci. Of course the number of vital foci is much greater than that of foci of disease can ever be; and hence disease and life, or to speak more accurately, diseased and healthy life, can very well coexist in the same organism; always, however, so that disease signifies a reduction, a *minus* of healthy life. By this research we have even rediscovered the long-lost essence of disease, not indeed in a spiritualistic form, but as a quite material *ens*, a genuine incarnate thing—*the altered cell.*

Has all now produced advantage? Was it worth the trouble to inflict pain on so many animals? to kill so many animals? Is there a really justifiable claim for allowing the experimental method to proceed still further? We can answer all these questions confidently in the affirmative. Not every experiment on animals has results as great as that of Galvani, results which have not merely led to a new and effective method of treating disease—electrotherapy; which have not only disclosed a large new territory of vital processes, but have supplied the first preliminary condition for an incalculable number of the most important technical arrangements, the knowledge of the natural course of events. But galvanism might yet appear to limited and timid heads as an instructive and refreshing play, for the reason that not every result of true observation of nature is usually brought forward at once, and that nevertheless it may be of the highest practical value. The cellular theory and the proof of the *vita propria seu cellularis* are in themselves very abstruse things, and no one can cure patients by their means without understanding something further. And yet they have become the foundation, yea, in a certain measure the security, for localising therapeutics, and they will surely become more so from day to day, when first materia medica in its wider extent shall have gone on the way which toxicology has already for a long time followed in a manner so rich in results.

How then can a great result to the science of healing be expected, if research in animals be cut off? For a long time no remedy has been more rapidly recognised, or more extensively used, than chloral, the effects of which were discovered and established experimentally by Herr O. Liebreich in my laboratory. How would it have been possible to know how to ascertain those effects without experiments on animals? The animals' friends say to us, "Then try the new medicine on yourselves!" They refer us to the provings of medicines by the homœopaths. But, quite independently of the fact that the provings of the homœopaths have not taught us to recognise one single new remedy which can be compared even at a distance with chloral, and that these provings, even in regard to already known remedies, do not in the least correspond to scientific investigations; that thus they cannot be altogether regarded as an original example—one will yet not be able to earnestly desire that very different, possibly poisonous bodies, should be made the subject of self-experimentation by physicians or other men. This kind of morality, which forbids experiments on animals and counsels experiments on one's own life or on sick men, misses, in fact, the first foundations of intelligent examination.

The proof of the great importance of hygiene and prophylaxis is rather superfluous. If any class of men has been active in this direction it is surely medical men. Never has there been a want of zealous hygienists among them; and when a great problem of prophylaxis was to be solved, one might be sure of finding medical men engaged in the work. We are so accustomed to this obligation that we always regard hygiene and prophylaxis as belonging to medicine, and to no other science. But it is empty talk when it is said that prophylaxis will render therapeutics—yea, even in a certain degree, medicine—superfluous. The arrangement of this imperfect world is such that there surely will be sick as long as men exist; and we are not afraid because of the threat that there will be no further need of us. Not even through the assistance of hygiene will people be able to do without us; and still less without experiment on animals. Will even the hygienists be condemned to test the various "causes" cold and warmth, dryness and moisture, dust and noxious gases, micrococci and bacteria, on their own persons, in order that they may from such self-observations determine their effects, and formulate laws? Intelligent Governments will comprehend that it would be an act of madness to sacrifice human life, merely because it occurs to a small number of persons that it is criminal to sacrifice the lives of animals. Medical men are already more exposed in epidemics of all kinds, in the performance of their duties in hospitals, in the country, in their nocturnal visits to the sick, in operations and necropsies, than any other class of the community as a rule; and it requires all the blindness of the animal fanatics to require also of them that they should test on their own bodies the remedial, or poisonous, or indifferent action of unknown substances, or that they should determine the limit of permissible doses by observations made on themselves.

In the name of humanity, of morality, of religion, the suppression of experiment on animals is demanded. For, in fact, it is not merely vivisection that is in question, but experiment on animals; that is, the experimental method in general. When the term vivisection is used it is made to include in like manner all painful actions in which there is no cutting; indeed, to prevent any misconception, not only physiological, but also pathological and pharmacological, experiments, are expressly included. *The criterion is pain. Everything by which, in the way of experiment, pain is inflicted on an animal is torture of animals,* and so far immoral, and contrary to religion. With this definition of torture of animals it might be possible to arrive at exceptional results by applying it to other callings or men. The dog-fanciers, who in the rearing of their dogs often use, or cause to be used, methods full of torture and painful chastisement, would readily come into great danger. The improvement of horses for certain purposes would have to be entirely put down. A great part of our domestic animals would have to remain untrained, so that pain might be spared to them. We should perhaps arrive at conditions similar to those produced by the wild dogs in Turkey.

Individual anti-vivisectors are at least so far consistent that they would see the slaughter of animals also forbidden. From the vegetarian standpoint, the opposition gains a kind of systematic aspect. Thus Herr von Seefeld[1] demands a vegetable diet and the prohibition of vivisectors; but as he, as a vegetarian, has no need of flesh, he is strongly inclined to make still further concessions. Thus he rejects hunting for the purpose of pleasure, but cannot altogether dispense with it as a means of defending life. Others go still further, and sacrifice also war. The principle can scarcely be denied, that death is worse than torture. There could scarcely be a criminal code, which punishes the premeditated killing of a man less severely than the torture of a man. Not without reason is it alleged that a man who still remains alive after his misdeeds may recover and attain to a complete or entire enjoyment of life. Grounds of mitigation in cases of murder and manslaughter are allowed also to men; but, as a foundation, the extremest injury which can be inflicted on man is always and everywhere the most severely punished.

As regards animals, the anti-vivisectors, on the contrary, consider torture to be worse than death. Although they reject every torturing or painful method of death, even for cattle, they without the slightest consideration cause animals, even highly organised ones, to be slaughtered or killed, not only for eating, but also for other purely subjective reasons. They go, indeed, so far as to demand that an animal which has survived vivisection shall be killed, although it might possibly still enjoy a long and happy life. Is there any logic in this, or any morality? How? May we have the right to kill an animal on any ground of public utility, to eat its flesh, to sell its skin, to pound its bones to manure for the field? and are we not to have the right of subjecting it to scientific research, which we institute on entirely ideal grounds, or on the grounds of the public weal, in which we even perhaps run the risk of becoming diseased? It will be difficult to assume that we institute researches on glanders or splenic fever for pleasure, or to pass away time, or without knowledge of the great danger of inoculation. Whoever allows himself the right to kill animals, has no right to forbid physi-

[1] Alfred von Seefeld. "Altes und Neues über die vegetarianische Lebensweise." (Hanover, 1880.)

cians to vivisect animals for experimental purposes, or to undertake painful operations of any other kind.

Of course we cannot desire that the misuse of this right should escape punishment. For it is with such an abuse, not with the production of pain, that torture of animals first comes into operation. Were every production of pain in itself an act of torture, punishment ought to be inflicted on a veterinary surgeon when he operates on a sick horse for the purpose of curing it. Culpable torture of animals lies before us, when pain is inflicted on an animal in a useless manner, and without purpose. Hence nothing can be said against the view that every experimenter should be subject to official inspection; but surely this does not require a society for the protection of animals. He who has a greater interest in domestic animals than in science, that is, in the knowledge of truth, is not qualified to be an official controller of scientific affairs. To what would it lead, if an experimenter, who had commenced his experiment in good faith, had perhaps to answer to some layman during the experiment, or to a magistrate afterwards, the charge that he had not selected some other method, or some other instruments, or perhaps some other experiment?

No: here is no question of objective right. So long as perfect liberty is left to every possessor of animals to kill his animals, be they wild or tame, at any time, and according to his own judgment, so long must it also be permitted that, for scientific ends, and thus on purely internal grounds, experiments should be made on living animals. But the necessity of such experiments can naturally only be decided by the inquirer himself; as to the choice of place, time, the admission of strangers, he may be required to communicate with the inspector; but the carrying out of the experiment must remain in his own hands. So we understand the expression of the freedom of science.

What is objected to us is, that it is the outraged feelings of the possessor of horses, pet dogs, and parlour cats that excite him to the belief that the same thing may happen to his beloved animals as to the animals in the learned institute. We can sympathise with him. We would force no one to deliver to us his favourites, not would we steal them. Were either of the two to occur, probably in every country the intervention of the magistrate would be called on with effect. But we also require that the disposal of the life and maintenance of those animals which have come into our possession in a legitimate way, should not be lessened to us, and that we should not be considered or declared to be *à priori* rough, void of moral feeling, and barbarians standing almost on the threshold of crime. The evidence that moral earnestness is failing in modern medical circles is nowhere afforded. The reproach that Christianity is imperilled by vivisection is worthy of Abdera. The assertion that the medical youth are inevitably "brutalised" by dissection and vivisection is, as usual, snatched from the air; as it is also a calumny that the vivisecting teachers have suffered injury to their morality.

At least however there is no ground to fear for science itself. To it is applicable what Bacon said of the sun: "Palatia et cloacas ingreditur, neque tamen polluitur."

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 1.—M. Jamin in the chair.—The following papers were read:—On the formation of tails of comets (second note), by M. Faye. Herschel, Arago, Delaunay, and other astronomers did not thoroughly study the tails of comets, but Newton had already given a quite sufficient explanation of the phenomena. The tail is nothing else—he maintained—than the result of a continual emission of molecules from the head of the comet. It is very much like the tail of smoke emitted by a running locomotive, its outer end being lost in space, and the inner one continually receiving a new supply of molecules. M. Roche, who has made the necessary calculations, taking account of the repulsive force M. Faye advocates, has worked out all those shapes of tails which we witness in reality.—On the equivalence of quadratic forms, by M. Jordan.—On a modification of the electric lamp, by M. Jamin, being the result of observations on the electric light in vacuum, and in closed vessels containing various gases.—On the perchloric acids, by M. Berthelot.—On the travels of Moncateh-Apé, by M. Quatrefages. This American Indian undertook a journey to the north-western coasts of America at the beginning of last century, in search of the origin of his race; whilst on this coast he learned and witnessed that it was visited every year by white men with long black beards, and M. Quatrefages proves that these men were originally from the Loo-Choo islands.—On the first meteorological, topographical, and hydrographical observations at the future Panama canal, by M. de Lesseps. Several maps of the coast are prepared, and a meteorological station is opened at Colon.—On the application of electromotive power and of M. Planté's secondary piles to the direction of aërostats, by M. Tissandier. In an aërostat which has a volume of 2200 litres, 3·50m. long, with a diameter of 1·30m., and can raise a weight of 2 kilogrammes, having a Siemens machine which weighs 220 grammes, and a secondary couple of 1300 grammes, the propulsory helix makes six and a half revolutions per second, and the balloon acquires a speed of 1 metre per second for forty minutes. The small Siemens machine, with three elements, produces the work of 1 kilogrammetre.—The elements of comet *c* of 1881 (Schäberle), by M. Bigourdan, as deduced from observations at Vienna on July 18, and at Paris on July 23 and 28. Its brightness, which is still increasing, will be on August 23 seventeen times as much as it was on July 18.—Spectroscopical observations on the comets *b* and *c*, 1881, by MM. Thollon and Tacchini.—On the lengths of spectral bands given by compounds of carbon, by M. Thollon.—On the constitution of comets, by M. Prazmowski.—On the theory of trilinear forms, by M. Le Paige.—On the influence of pressure on dissociation, by M. Lemoine.—On the heat of formation of explosives, by MM. Sarrau and Vieille.—On oxycyanides of lead, cadmium, and mercury, by M. Joannis.—On the heat of combustion of heptane and of hexahydrotoluene, by M. Louguinine.—Third note on the magnesia industry, by M. Schlösing.—A contribution to the study of the transmission of tuberculosis, by M. Toussaint. The juices of animals which have had tuberculosis transmit the disease with very great ease, even when submitted to a high temperature, but especially when employed uncooked.—On the injection of the virus of rabies into the circulation, by M. Galtier. It seems to prevent infection.—On hemeralopia and on the functions of the visual purple, by M. Parinaud.—On the applications of electromotors, by M. Trouvé.

VIENNA

Imperial Academy of Sciences, July 21.—L. T. Fitzinger in the chair.—A. Rollett, on the derived albumins noted as acid-albumins and alkaline albuminates.—Dr. Stur, on the Silurian flora of the H-h_1 stratum in Bohemia.—S. Lustgarten, on an ethyl nitrate formed by the action of nitric acid on glycogen.—Ernst Lecher, on the spectral distribution of radiant heat.—Dr. T. Kessel, on the function of the external ear in relation to the space-perception.—On the difference of intensity of a linear-produced sound in different directions, by the same.—F. Fossek, on the products of condensation of isobutyl aldehyde.—Zd. H. Skraup, on quinine and quinidine.—Note on some quinine compounds, by the same.—Prof. Freund, on the formation and preparation of trimethene alcohol from glycerine.—Preliminary note on trimethene, by the same.—H. Weidel, on a compound isomeric to α-sulphocinchoninic acid.—G. Goldschmidt, on mono- and dinitropyrene and amidopyrene.—E. Weiss, a communication on the third comet of the year 1881 (1881 *c*), discovered by Schäberle at Ann Arbor (Michigan).—T. Woehner, report on his observations of the earthquake phenomena in Croatia in the year 1880.

CONTENTS

THURSDAY, AUGUST 18, 1881

THE CENTRAL AFRICAN LAKES

To the Central African Lakes and Back: The Narrative of the Royal Geographical Society's East African Expedition, 1878-80. By Joseph Thomson, F.R.G.S. Two vols. (London: Sampson Low and Co., 1881.)

OUR readers must be familiar with the leading features of the remarkable Expedition which set out at the end of 1878 under the leadership of Mr. Keith Johnston, and returned in 1880 under that of Mr. Joseph Thomson. We have already told the story of the Expedition pretty fully, and Mr. Thomson himself described in these pages the main points in the geology of the Expedition which he traversed. Mr. Thomson was a very young and inexperienced man when, by the sad death of his accomplished chief, the command of the Expedition devolved upon him, just after it had got over the initial difficulties of the coast region and entered on the great tableland which occupies the greater part of the African area. Mr. Thomson showed himself at once equal to the emergency, and succeeded in winning a reputation that is likely to prove fruitful of good results both for himself and for African geology. He has been equally successful in telling the story of his journey. He writes in quiet and simple, but effective, style, his pages are full of incident and information of great interest, and he has a sense of humour and a love of fun which are not only of service to him under trying conditions, but which render his book light and pleasant reading. After seasoning themselves by a little trip to Usambarra, Messrs. Johnston and Thomson (the latter geologist to the Expedition), at the head of 150 men, left Dar-es-Salaam in May, 1879, for the north end of Lake Nyassa. A month later Mr. Johnston died at Behobeho, to the north of the Rufiji, just when the mountains that bound the plateau had been reached, through the usual difficulties attending African travel, partly arising from the nature of the country, partly from the natives, and partly from the men who formed the Expedition; but Mr. Thomson overcame them all with more than usual tact, and with unprecedented success. After a short rest at Lake Nyassa the route was resumed over the previously untraversed country between Nyassa and the south end of Lake Tanganyika. Leaving most of his men here, Mr. Thomson, with a small contingent, proceeded up the rugged west side of the lake to the famous river, about whose course Messrs. Cameron and Stanley gave such inconsistent accounts—the Lukuga. Mr. Thomson's observations on this river are of great value in connection with African hydrography. He found that Cameron and Stanley were both right. At Cameron's visit, what little current existed was towards the lake; Stanley, later on, found a distinct current setting from the lake, and prophesied that in a short time the barrier of vegetation across the river not far from its mouth would be carried away, and the Lukuga would carry the waters of Tanganyika in a full stream to the Lualaba-Congo. And this, Mr. Thomson found, had actually come to pass; he saw the Lukuga as a broad, swift effluent from Lake Tanganyika. When, however, he returned two months later, he found that the strength of the current had considerably decreased, as had also the volume of the river. Mr. Thomson discusses the interesting problem of the Tanganyika at some length, and with great intelligence, as well as with the knowledge of a trained and practical geologist. He refers to the facts observed by previous travellers as well as himself, as to the rise and fall of the level of the lake, the hydrography of the neighbouring country, &c., and concludes as follows:—

"With these facts before us the necessity of Tanganyika having a regular outlet is not so apparent as it seems to have been to Cameron. Neither need we, like Stanley, invoke the aid of great convulsions to account for the interruption or intermittency of the outflow. The phenomena are sufficiently accounted for by the facts I have enumerated, viz. (first), that since the Arabs settled at Ujiji till within the last five years there has been no very marked rise in the level of the lake; (second), that the rapid rise of the lake after that time was due to unusually wet seasons; (third), that the normal rainfall is less than fifty inches in the year; (fourth), that Tanganyika drains a remarkably small area of land, and has only a few insignificant rivers, torrents, and streams falling into it; and (fifth), that the volume of water passing out by the Lukuga is diminishing so rapidly as to be markedly noticeable in two months, even in the course of the rainy season, so that quite possibly the next traveller may arrive to find little or no water leaving the lake. These considerations, then, as well as all my inquiries and observations, lead me to conclude (first), that under normal circumstances the rainfall and evaporation nearly balance each other; (second), that many years ago a series of unusually dry seasons reduced the level of the lake below that of its outlet; (third), that it remained sufficiently long without circulation to become charged with salts, which have given the water a markedly peculiar and unpleasant taste, unlike that of ordinary fresh water, and also an exceptional power of corroding metal and leather; (fourth), that unusually wet seasons set in some five or six years ago, raising the level of the lake; (fifth), that it rose above its normal level, owing to the formation of a barrier in the bed of the Lukuga by rapid vegetable growth, and the depositing of alluvium by the small streams descending from the slopes on either side; and (sixth), that the lake having once overflowed the barrier, soon removed it entirely, thus regaining its original channel and level."

After a short stay with the hospitable and intelligent missionaries at Mtowa, to the north of the Lukuga outlet, and paying a visit to Ujiji, Mr. Thomson set out to make his way back through terrible Urua, where Cameron suffered so much. Here he had to undergo the worst reception he met with during his whole journey; every possible obstacle was placed in his way, every opportunity was taken to insult him and steal his goods, even the very blanket from beneath him when asleep; and it was only by the greatest self-restraint and tact that fighting was avoided and he and his men escaped with their lives. This they were only too glad to do, just before they reached the goal of the expedition, the river Lukuga. Returning to Mtowa, a passage was obtained in Mr. Hore's boat to the south end of the lake. Here Mr. Thomson picked up his men and set out on the return journey. He had intended to go east through Usanga and by the upper waters of the Rufiji, but as the natives were at war this would have been dangerous. So he went north by the east side of Tanganyika, taking occasion to have a good look at the mysterious Lake Hikwa,

which he re-named Lake Leopold. Mr. Thomson did not succeed in reaching the actual shores of the lake.

"The point where we halted was upwards of 7000 feet above the sea, and from the fact that the River Mkafu, which flows into the lake, is only about 3000 feet, at a distance of sixty miles north, I infer that Lake Hikwa is not far from being on a level with Tanganyika. So steeply do the mountains descend, that from the place where we halted we could almost throw stones into the lake; only we lost sight of them before they reached the ground. The general altitude of the surrounding ranges must be quite 8000 to 9000 feet, and they extend in a quite unbroken line all round. At the north end I calculate the breadth of the lake at about twelve miles. Further south the breadth varies from fifteen to twenty miles. Longitudinally it lies north-north-east and south-south-west. Its length, from native report and from my proximity to it in passing between Nyassa and Tanganyika, I conclude to be certainly not less than sixty miles, probably seventy. Between the mountains and the shores there lies a narrow dark green strip of smooth land, apparently representing a once higher level. On this there are many villages, and the ground is highly cultivated. At the north end, as I have already stated, this strip broadens out into a marshy expanse, formed doubtless by the detritus of the River Mkafu."

Although the waters are almost certainly fresh, yet the lake seems to have no outlet. Without accident or obstacle to speak of, the Expedition, proceeding by Unyamyembe, reached Zanzibar, not much more than a year after it set out from Behobeho. Mr. Thomson, with good reason, congratulates himself that he never needed to fire a shot either in offence or defence, and that, besides the loss of Mr. Johnston, he left only one man behind him.

The Expedition is in many ways one of the most successful that ever entered Africa. Not only was it conducted with unusual efficiency, not only were the chiefs and people, with few exceptions, friendly throughout, but for the first time we have obtained trustworthy observations on the geology of the great lake region of Central Africa. The main conclusions reached by Mr. Thomson have already been described by himself in these pages. But he did not confine himself to geology. He gives us a fair idea of the general character of the country traversed, its mineral, vegetable, and animal productions, the characteristics and habits of the people, the nature of the work being done by missionaries, and the capacities of the country for industrial development. On the last point his views are far from being sanguine. He maintains that the resources of Central Africa have been greatly exaggerated, especially as to its minerals. We are inclined to think that on this point he has taken much too gloomy a view, and that, whatever may be the case with the region actually visited by him, there certainly appears to exist, in the districts traversed by Cameron, Livingstone, and more recently by Major Serpa Pinto, stores of iron and copper that may at a future time be turned to great industrial account. Young as Mr. Thomson is, we commend his remarks on missionary work to those whom it most intimately concerns; and we trust that his severe, but evidently just, criticisms on the conduct of the various Belgian expeditions in Africa will receive the attention they deserve from the management of the International African Association. In the Appendices are given notes on the natural history collections, and Mr. Thomson discusses the geology in detail, suggests that at one period the whole of the lake region of Central Africa must have been covered by the sea, the basin of Tanganyika, however, having been formed subsequently by a great fault or narrow depression of great though unknown depth.

Prefixed to the volumes are portraits of Mr. Johnston and Mr. Thomson, and appended a route map and an interesting geological chart.

OUR BOOK SHELF

Marine Algæ of New England and the Adjacent Coast. By Dr. W. J. Farlow. (Washington, 1881.)

THIS valuable essay on the "Marine Algæ of New England" is a reprint from the United States Fish Commission Report for 1879. It includes a list of all the species of sea-weeds, with the exception of the diatoms, which are known to occur on the coast of the United States, from New Jersey to Eastport, Me. Prof. Farlow gives in a compact and more or less popular form a description of the various orders and species, and he adds a short account of the general structure and classification of sea-weeds, so that all persons frequenting the coast of New England are thus furnished with a handy and compact manual of the subject. The fifteen excellent plates drawn by J. H. Blake and W. G. Farlow deserve a special notice, as they give details of structure which will enable the text to be understood by an intelligent student.

Since the appearance (1852-57) of Harvey's classic work on the North American Algæ, but few species have been added to the Flora. This is not perhaps so surprising as regards the Florideæ or Fucoids, to which Harvey paid so much attention; but as regards the unicellular or simple filamentous forms it is a cause of surprise, for Harvey never paid minute attention to these; and it may in part be accounted for that collections do not seem to have been made along the coast in spring. Prof. Farlow gives a most interesting sketch of the geographical distribution of the species met with. Cape Cod is, as was known to Harvey, the dividing line between a marked northern and southern flora, and subsequent observation shows that on the one hand the flora north of the Cape is more decidedly arctic than he supposed, and that on the other hand that south of the Cape is more decidedly that of warm seas. A good share of the commoner species are also natives of Great Britain, another large share are Scandinavian; but while this is the case the marine flora is also marked by the complete absence of many common British species. No members of the order Dictyotaceæ are to be found; no species of Cutleria or Tilopteris are to be met with. The species of Nitophyllum may be said to be wanting. That commonest of our red sea-weeds, *Plocamium coccineum*, is known as native by only one doubtful case. *Fucus canaliculatus*, *Himanthalia lorea* are quite wanting. The nearly ubiquitous *Codium tomentosum* has not yet been found. *Fucus serratus* is very rare, having only one locality recorded for it in the United States and one in Nova Scotia. *Gelidium corneum*, abundant in almost all parts of the world, is only occasionally found in New England, and then only in the starved form known as *G. crinale*.

Prefixed to the orders and genera will be found carefully-written diagnoses, and an artificial key to the genera is also added. The notes in smaller type which are given under the species often contain most valuable critical information, which will command the attention of all phycologists. To the critical students of our native

species of algæ this little manual of the New England species will prove a most welcome volume. They will find in the chapter on the structure and classification facts that were not known in Harvey's day, and which, here collected for them within a brief space, they would otherwise have to search for in the writings of Thuret, Bornet, Janczweski, Rostefinski, Pringsheim, or Reinke.

The Berries and Heaths of Rannoch. (London: G. Bell and Sons, 1881.)

THE berry-bearing plants here described and delineated are eight, viz. *Vaccinium oxycoccos*, *V. Myrtillus*, *V. uliginosum*, *V. vitis Idæa*, *Arctostaphylos uva-ursi*, *A. alpina*, *Empetrum nigrum*, and *Rubus chamæmorus*, all of which do not, strictly speaking, come within the geographical limitation of the title-page. The heaths are three only in number, viz. the common *Erica cinerea* and *Tetralix*, and *Calluna vulgaris*, to which are added two other nearly allied species not actually found within the district, *Andromeda polifolia* and *Loiseleuria* (*Azalea*) *procumbens*. In the letterpress it is not to be expected that anything new could be added to what is already known about these plants; but in an appendix is given a list of the Gaelic names of the various species supplied by the editor of the *Scottish Naturalist*. The coloured plates are exceedingly good and characteristic; but surely it should have been stated that they are taken from Sowerby's "English Botany." The volume is a pretty one to lie on the drawing-room table. A. W. B.

Lehrbuch der Mineralogie. Von Dr. G. Tschermak. I. Lieferung. (Wien: Alfred Hölder, 1881.)

IT is with great pleasure that we have received this instalment of Prof. Tschermak's work, and also learnt from the publisher's introductory note that the rest of the book may be expected during the course of a year. The work is sketched somewhat on the lines of Naumann's well-known "Elemente der Mineralogie," but follows Miller's Mineralogy in the wider scope given to mineral physics. The present number is introductory, and treats of descriptive crystallography, crystal-structure, general mineral physics, and includes a considerable portion of mineral optics. In the crystallography the Millerian notation and the stereographic projection are employed, and the systems are developed from the principle of symmetry in a clear and simple manner. Prof. Tschermak has adopted the four-plane axial system in the rhombohedral system, which is sometimes designated the Bravais-Miller system. Possibly this may appear to non-mathematical students simpler, and may to a certain extent be more easily mastered, but we feel sure that in its practical application to crystallographic problems it does not possess either the elegance or conciseness of the three-plane axial system selected by Prof. Miller. We feel also that it is most unfair to Prof. Miller's memory to attach his name, even in a double-barrelled way, to a system which he steadily refused to adopt. The theories and facts of twin and mimetic crystals are carefully expounded. These constitute a branch of mineralogy which has become of the utmost importance since the application of the microscope in the investigation of the optic properties of minerals. Other sections, which are especially good, are those on mineral inclusions, on the hardness and etching of crystal faces. These contain a large amount of information which is rarely to be found except by a laborious search through scientific periodicals. The book is divided into sections, each dealing with its separate subject, and at the end of each section is a list of the more important literature of the subject. The work so far is excellent, and if, as we have every reason to expect, it be carried through in an equally satisfactory manner, we shall possess a text-book in keeping with the reputation of its author and worthy of the school to which he belongs. W. J. LEWIS

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Panizzi and the Royal Society

THE "Life of Panizzi" by his friend and colleague, Mr. Louis Fagan,[1] is marked by a tone of indiscriminate adulation which disfigures many specimens of modern biography. The hero is perfect, and they who think otherwise are dismissed with words of contempt, or are admonished to go and meditate on their wicked ways and then return in repentant mood to the community of hero-worshippers.

In the Royal Society's treatment of Panizzi, Mr. Fagan endeavours to justify another example of the wolf and the lamb, although it must be owned that in the pamphlets[2] from which the biographer quotes, the lamb's bleatings are sufficiently energetic to lead to the conclusion that he thought himself a match for the wicked wolf.

Mr. Fagan thinks it important "that Panizzi's stormy connection with the Royal Society should be fairly and impartially" stated; although how this can be done without hearing both sides he forgets to say; and yet he professes to give "the proper elucidation of the facts," "the whole circumstances of the case thoroughly weighed and dwelt upon"; how successfully he opposed "the force with which it was attempted to crush the evidence of his superior talent" (vol. i. p. 119), and although "thwarted and impeded at every step, Panizzi at last succeeded in once again proving that right can contend successfully with might" (vol. i. p. 130).

The reader will gain a very lop-sided idea of this quarrel if he trust to Mr. Fagan's account alone; and as in the reviews of this book no one has attempted to ascertain the truth of the matter (which indeed could not be done without access to the Royal Society's papers), I venture, as a member of the present Library Committee, to state the case from the other side, being naturally anxious to sustain the reputation, so unjustly assailed, of a former committee which contained the honoured names of Baily, Beaufort, Children, Greenough, Lubbock, Murchison, Peacock, Roget, and others.[3]

To make a long story short, it is sufficient to state that about the year 1832 the Royal Society wished to bring out a complete catalogue of the books, &c., in its library. As a preliminary step, a list of the mathematical books was compiled and set up in type as a specimen of the kind of work required. In the words of a Council minute, the sheets were "not designed for publication," they being "in a very rough and unfinished state."

In October, 1832, Dr. Roget meeting Mr. Panizzi at dinner, informed him of the Society's intention, and requested him to look over and revise the sheets in question, together with others that might afterwards be forthcoming. This was agreed to, and the first sheets were forwarded to Panizzi, who found so many errors in them that, as he informed Dr. Roget, "although I would never attempt to correct what had been already done, I was ready to undertake a new compilation."

Accordingly on October 16, 1832, the Library Committee resolved to recommend to the Council that Mr. Panizzi be engaged to make a new catalogue according to the mode to be agreed upon by the Committee, he to be paid 30*l.* for every thousand titles, the whole remuneration, however, not to exceed 500*l.*

[1] "The Life of Sir Anthony Panizzi, K.C.B." By Louis Fagan. Two vols., 8vo, 1880.

[2] "A Letter to H.R.H. the President of the Royal Society, on the New Catalogue of the Library of that Institution now in the Press." Pp. 56 and 3. Signed A. Panizzi, and dated January 28, 1837. The last three pages contain a postscript letter to the President, dated November 4, 1837, and a note in which it is stated that the pamphlet was not put into circulation until the latter date, in order that H.R.H. might have an opportunity of replying to it.

The President, not having availed himself of this opportunity, the second pamphlet was put forth. It is entitled "Observations on the Address by the President, and on the Statement by the Council to the Fellows of the Royal Society respecting Mr. Panizzi, read at the general meeting, November 30, 1837." Pp. 24. Dated December 22, 1837.

[3] Strictly speaking there were three committees, namely, one for the catalogue, a second for the library, and a third for deciding in doubtful cases under what division a book should be placed in the new catalogue.

Panizzi agreed to these terms, and offered "to wait on the Committee, as soon as convenient to them, to settle the manner in which they wish the work to be executed."

Now the whole gist of this quarrel consists in this, that the Library Committee naturally wished to control Mr. Panizzi in his mode of executing the work, while he refused to be controlled or interfered with in any manner. He even regarded as personal enemies all those who attempted so to interfere. He fancied that every one who differed from him was actuated by a sense of personal dislike. When he refused Dr. Roget's request to revise the sheets of the Catalogue, he says (p. 6): "I had no idea when I so candidly expressed my opinion that I was making a powerful and unrelenting enemy in one of the most influential officers of the Royal Society." At p. 51 he says: "so gratuitous an insult would never have been allowed had not Mr. Baily filled the chair at that meeting." And again (p. 5), "My statements will be received with derision by those who know that they may be unjust with impunity." At p. 18 he charges the Committee with "indelicate conduct," at p. 22 with "absurdity," at p. 25 such things were done "purposely to annoy me;" and again, "No suggestion of mine would ever be attended to by the Council." At p. 26 his work was regarded with "a malignant eye;" at p. 28 "The annoyance was incessant," "injurious and unjust;" at p. 33, "treating me as if I were their servant," "unwarrantable liberty;" p. 38, "unjustly interfered with;" p. 41, "insulted with an order of submitting my work to revision. . . . I shall never consent for any one, be he who he may, to make any alterations in it." And when, on June 24, 1836, he was requested to attend the Library Committee on the following Monday at 4 p.m., he declined on the ground that "when I attended before I was not so well satisfied with my position as to wish to be in it again." At p. 54, when clamouring for payment of an unascertained balance which he claimed, he charges the Council with not meaning "to pay it unless they be compelled to it. . . . Possibly there is some legal means of obtaining redress; but in a country like this justice is not a luxury for a poor man to indulge in; and the Council, having at their disposal the funds of the Royal Society, can amuse themselves without personal trouble or loss with a law-suit which I have not the means of sustaining." Will it be believed, in the face of such language as this that Panizzi had already been paid the sum of 450*l*., and his whole remuneration was not to exceed 500*l*.

In his second pamphlet (p. 18), after charging the Council with not meaning to act fairly, he hurls at it his "unmixed disgust and contempt." But I cannot help thinking that these vigorous epithets would have been more appropriate had they travelled the other way.

When requested to return the printer's revises, and he refused on the ground that they were his own property, together with the key of a drawer in one of the Royal Society's rooms, and he also refused, what wonder that, after so long a contest with this cantankerous man, the Council should have resolved on July 14, 1837, "that Mr. Panizzi be no longer employed in the formation of the Catalogue."

The reader may well exclaim by this time, What *is* all this hubbub about? Simply this: Mr. Panizzi insisted on adding to some of the items of the Catalogue original comments of his own, to which the Library Committee justly objected as committing the Society to opinions of doubtful value. Panizzi attached the greatest importance to these notes and comments. "The Committee, far from objecting to them, ought to have been thankful that I had taken the trouble of introducing them" (p. 31); and he proceeds to quote specimens illustrative of this part of his work. For example, he says: "To the 'Mémoires' of Charnières on the observations of the longitude, I added this note: 'All the author's additions and corrections carefully put in by J. B.' This note is on the title-page of this copy, and the volume is interspersed with alterations in manuscript. I suppose J. B. to mean James Bradley." Later on in the same page he adds: "The author's additions, if put in by Bradley, are, of course, of much more value than if written by any other J. B."

Now the book in question is only a single Mémoire of De Charnières, not a collection of "Mémoires," as described by Panizzi. Moreover, there are five reasons why the additions and corrections could not have been written in by Dr. Bradley.

1. He died five years before the memoir by Dr. Charnières was published. This may well excuse the other four reasons, but they are curious as illustrating the carelessness of a man who was convinced of his own infallibility.

2. The writing of the anonymous J. B. is small and neat: that of Bradley large and awkward. The Royal Society had in its possession manuscripts of Bradley and his signature, which could be seen by merely asking the assistant-secretary for them, and yet Panizzi did not submit the writing of J. B. to this simple test.

3. Bradley was not in the habit of writing in his books.

4. The so-called "additions and corrections" are simply the corrigenda collected into eight pages at the end of the book, and transferred in MSS. to the text, a fidgety piece of work, not likely to be undertaken by so busy a man as Bradley.

5. At the end of the book J. B. drops his incognito and appears as *J. Bevis*, a fact overlooked by Panizzi.

Other similar examples might be given, and indeed were submitted to the Fellows of the Royal Society at the time, in order to justify the resolution of the Library Committee "that all comments or notes expressing matters of opinion on the articles in the catalogue be omitted"; but the statement of them would occupy too much space, dealing as they do with details which unless given in full would not be understood.

Mr. Panizzi was undoubtedly a vigorous clever man; but in the matter of books, he, unfortunately for his own reputation, aspired to universal knowledge which belongs to no one. The gold of a universalist is apt to shrink down into dross when tested in the crucible of a specialist. Having occasion to consult a book by Gay-Lussac, and not finding it in the Catalogue of the British Museum Library, the attendant requested me to write the name and title on a slip and show it to Mr. Panizzi. No sooner had he glanced at the slip than he exclaimed "Ah! you have made a mistake: it is Guy-Lussac!" This readiness on all occasions to say something apparently to the purpose, may impress subordinates with a sense of power on the part of their chief, but to tell a chemist that Gay-Lussac is Guy-Lussac would be much the same as telling him that potash and soda are identical compounds.

C. TOMLINSON

Highgate, N., August 2

The Oldest Fossil Insects

IN a paper on "The Devonian Insects of New Brunswick" (*Bull. Mus. Compar. Zoology*, 1881, vol. viii. No. 14) I have drawn attention to the fact that a fern on the same slab with Platephemera was determined in 1868 by Prof. Geinitz as *Pecopteris plumosa*, and therefore the slab considered by him as belonging to the Carboniferous. I believed that here an important gap was still to be filled, namely, the reliable determination of the fern, which is not mentioned in Mr. S. H. Scudder's monograph, nor in Principal Dawson's note on the geological relation of those insects, which closes Mr. Scudder's paper.

A paper by Mr. Dawson (*Canad. Naturalist*, 1881, vol. x. No. 2) is intended to fill this gap. The fern is after the study of the original specimen determined as *Pecopteris serrulata*, and said to be a common species in those beds. If I am not entirely mistaken it will be difficult to agree with Mr. Dawson's opinion (*l.c.* p. 2) "that doubts and suspicions thus cast on work carefully and exhaustively done should not seriously affect the minds of naturalists," as it happens that in his work of 1880 this common species is not quoted at all among the plants found in those beds, except in a note (p. 41) stating that in the beds 6 to 8 three or four other species occur, among them *probably P. serrulata*. Mr. Dawson quotes for the species the figures 207 to 209 in his Report of 1870, but I confess to be unable to recognise the Platephemera fern in those figures.

Prof. O. Heer has kindly drawn my attention to his "Flora Fossilis Arctica of Bear Island, Spitzbergen, 1871." He has given (pp. 14, 15) a detailed review of the fossil plants from St. John's, New Brunswick, and, as he still believes, has proven that those layers do not belong to the Devonian but to the Ursa stage of the Lower Carboniferous. This important and elaborate statement is disposed of by Mr. Dawson, as far as I know, only in his report, 1873, p. 8, in the following words:—"The so-called Ursa stage of Heer includes this (Lower Carboniferous), but he has united it with Devonian beds, so that the name cannot be used except for the local development of these beds at Bear Island."

It is true that Mr. Dawson, in the supplement to the third edition of the "Acadian Geology," 1878, p. 72, has tried to explain the different opinion of Prof. Heer by the earlier introduction of the Palæozoic flora in American formations. But this fact, known by every one, and of course by Prof. Heer, is not considered by him to be a sufficient objection to the statements given in the "Flora of Bear Island."

The paper of Prof. Heer states carefully and exhaustively the

facts which induced him to consider those layers at St. John's as belonging to the Lower Carboniferous. Therefore naturalists will scarcely agree that such a statement, made by a prominent and acknowledged authority, can be cancelled by a simple negation not supported by facts. Till this is done in a reliable manner, those oldest insects will have to be considered as belonging to the Lower Carboniferous.

Cambridge, Mass., July 25 H. A. HAGEN

The True Coefficient of Mortality

THE very interesting and suggestive lecture of Alexander Buchan on "The Weather and Health of London" (NATURE, vol. xxiv. p. 143 *et seq.*) reminds me of the propriety of calling the attention of writers on "vital statistics" to a point in relation to the true method of discussing the mortuary data. The specific point to which attention is drawn is the necessity of estimating the relative tendency to special diseases by comparing the number of deaths from the given cause with the number of persons living at the ages embraced in the record; instead of making the comparison (as is usually done) with the total deaths from all causes, or with the total number living at all ages.

In like manner, in discussing the influence of age on the mortality from any given disease, it is very common to prepare tables of the number of deaths at each age, and in some instances these numbers have been assumed to represent the relative tendency to the disease at different ages. It is scarcely necessary to say that this is a very serious error, for it must be borne in mind that the number of persons living at different ages is very unequal. Indeed it is self-evident that the true coefficient of mortality for any given disease at any given age is expressed by the ratio of the number of deaths from the specified disease at the given age to the number of persons living at the same age: or, as it may be otherwise indicated, the number of deaths from the given disease at the given age per 1000 persons living at the same age.

In illustrating this point I shall select cancer, because, in relation to the influence of age, it furnishes an extreme case, and thus affords a glaring instance of the fallacy of taking any basis of comparison other than the number of persons living at each age. The mortuary records of the Department of Seine in France, during the eleven years, from 1830 to 1840 inclusive, furnish a total of 9118 deaths from cancer, 2163 males and 6955 females. The following table relating to the mean annual mortality from this disease among females will illustrate this point:—

Age. Years.	Number of females living.	Mean annual deaths from cancer among females.	Annual deaths from cancer in 1000 females living at all ages.	Annual deaths from cancer in 1000 females living at each age.
(1)	(2)	(3)	(4)	(5)
0 to 10	139,840	1·273	—	0·00910
10 ,, 20	115,269	1·182	—	0·01026
20 ,, 30	104,342	15·364	0·04196	0·14725
30 ,, 40	73,203	74·727	0·20409	1·02081
40 ,, 50	54,124	148·727	0·40619	2·74788
50 ,, 60	36,800	147·273	0·40221	4·00198
60 ,, 70	25,703	133·545	0·36472	5·19564
70 ,, 80	12,852	83·364	0·22767	6·48659
80 ,, 90	3680	24·818	0·06778	6·74408
90 ,, 100	340	2·000	0·00546	5·88769
All ages.	566,153	632·273	—	1·11679

The foregoing table demonstrates the inaccuracy of the popular impression that the tendency to cancer attains its maximum between the ages of 35 and 50 years. The numbers in columns (3) and (4) might seem to support such an opinion; but, as we have seen, those in column (5) are evidently the true indices of the tendency to this disease at different ages; and it will be observed that the mortality goes on steadily augmenting with each succeeding decade of age up to 90 years. The fact likely to be most strongly impressed on the reader by the numbers in column (5) is the remarkable regularity of increase of the coefficient of mortality for cancer with advancing life among females after the age of 25 or 30 years. Between the ages of 25 and 75 the mortality increases nearly in arithmetical progression as the age advances in arithmetical progression, the average increment being about 1·30 per 1000 living at each age for each decade. Assuming this to be the law of mortality from cancer among females, it admits of very simple mathematical expression. Thus, let

A = the age at which liability to cancer begins.

A' = any age greater than A.

C = constant coefficient, variable according to country, state of civilisation, &c.

Then we have—Annual mortality per 1000 living at age $A' = C(A' - A)$.

In our table representing the mortality from cancer in the department of the Seine from 1830 to 1840 inclusive, the value of A may be taken $= 25$, and $C = 0·13$; hence we have—Annual mortality per 1000 living at age $A' = 0·13(A' - 25)$. Thus by the formula the mortality at $55 = 3·90$, and column (5) gives 4·00 between 50 and 60; at 75, formula $= 6·50$; table $= 6·49$ between 70 and 80.

The mortality from cancer seems to be vastly smaller in England than it is in France, so that a less value must be given to the constant C. The foregoing formula represents the law of increasing mortality with advancing life in the simplest form, as a function of the age. This extreme simplicity is probably unique in the case of cancer, and seems to indicate that age is so far the controlling element in the development of this disease as to overpower all other causes. In the case of other diseases we cannot expect to escape the necessity of employing those exponential functions in investigating their laws of mortality, which are essential when a multiplicity of causes are in operation.

Many years ago the attention of the medical profession in this country was called to the fact that the available mortuary data were not discussed in a manner which revealed the true value of the facts contained in the numbers.[1] But there is reason to believe that Prof. Francis A. Walker, the intelligent superintendent of the census of the United States for 1880, will not overlook this point when he comes to the discussion of the mortuary statistics which have been collected.

Berkeley, California, July 7 JOHN LE CONTE

[Mr. Le Conte does not appear to have apprehended the point discussed in the lecture on "The Weather and Health of London"—that point in no part of the inquiry being the tendency to the disease at different ages, but the manner of the distribution of deaths in the case of each disease through the weeks of the year, with the view of arriving at some knowledge of the influence of season in determining that distribution. Only in one case, viz., in discussing the rates of the mortality from diarrhœa in several large towns, was a reference to population required, and in that case the curves were drawn, showing the weekly rate of mortality per 1000 of the population of the respective towns.—ALEXANDER BUCHAN.]

Bisected Humble Bees

I TOO have frequently observed humble bees lying dead or stupefied under lime-trees, sun-flowers, and some other plants, and once I saw a Staphylinus, commonly known as Black Cock-tail, or Devil's Coach-horse, nip a humble bee in two, and on passing that way later I found that it had cleared out the honey-bag and left the two halves of the bee on the path, as described by your correspondent. I have known boys catch humble bees and eat the honey in them; and probably many other animals have learned how to get at the sweet drop.

Trinity College, Cambridge THOS. MCK. HUGHES

AT your request for information on the above I beg to say that I have observed both the flycatchers alluded to by your correspondent, and also the little blue tit (*Parus cærulea*) attack the humble bees in the manner described, to extract the honey-bag. This attacking the bees is not, so far as my experience goes, a general characteristic of these birds, and what should lead them to it occasionally I cannot ascertain.

Exeter, August 15 EDWARD PARFITT

[1] *Vide* papers by the writer, entitled "Statistical Researches on Cancer," *Southern Med. and Surg. Journ.*, new series, vol. ii. pp. 257-293, May' 1846. Also "Vital Statistics," illustrated by the "Laws of Mortality from Cancer," *Western Lancet*, vol. i. pp. 176-190, March, 1872 (San Francisco).

I NOTICE the same phenomenon here, under the sycamore trees, when they are in blossom, which your correspondent Mr. Masheder observed recently under his lime trees, namely, the heads and thoracic segments of severed humble bees lying on the ground, with legs and wings attached, still retaining their vitality in some cases, but without any trace of the abdominal segments, for the sake of whose contents, no doubt, the bees were destroyed. We have no fly-catchers here. I suspect the tom-tits, which are abundant in the vicinity of this wholesale apicide, but I have no direct evidence of their guilt. R. V. D.

Beragh, Co. Tyrone, August 15

Migration of the Wagtail

Apropos of recent letters on this subject in NATURE, permit me to note that on my voyage out to the East Indies in the month of October, 1878, on board the Dutch mail steamer *Celebes*, two wagtails alighted on the ship when not very far north of the equator (the ship's course being then from Aden to Padang in Sumatra). On observing them I pointed them out to a Dutch friend, who at once recognised them as *Kwikstails*. They were rather lively, and did not appear to us to be fatigued; after staying with us for some days they took their departure, but in what direction I had not the satisfaction of observing.

Without affirming positively, I believe the species was the *Motacilla alba*. HENRY FORBES

Sumatra, June

ITALIAN DEEP-SEA EXPLORATION IN THE MEDITERRANEAN

AFTER some delay, beyond our control, the war-steamer of the Italian Royal Navy *Washington*, Capt. G. B. Magnanghi, R.N., left Maddalena on the 2nd inst. on her thallassographic mission. Under the able direction of Capt. Magnaghi, two days were devoted to preliminary dredgings and trawlings in depths from 200 to 1000 metres, principally for testing our apparatus, which works admirably. On the 4th inst. (yesterday afternoon) we did our first deep-sea dredging in 3000 metres; the dredge came up empty, but I had the pleasure of securing, attached to the hempen tangles, a magnificent specimen of that strange blind Crustacean discovered by the *Challenger* in the North Atlantic, and named *Willemœsia leptodactyla;* it is no doubt one of the most characteristic forms of the deep-sea fauna, and its discovery in the Mediterranean is of very great importance and interest, as all students of thalassography will be fully aware, after what Dr. Carpenter has written on the biological conditions of the deeper parts of that sea. Our specimen of *Willemœsia* is slightly smaller than the one dredged by the *Challenger*, and figured in Sir Wyville Thomson's "Atlantic," vol. i. p. 189; but otherwise it differs only in one or two minor details, which may be sexual differences; it was dredged off the west coast of Sardinia.

On account of a slight mishap with our engine we have anchored at Asinara for a couple of days, but shall at once resume our work. HENRY H. GIGLIOLI

Asinara, Sardinia, August 5

KÖNIG'S WAVE-SIREN

EVERY musician is painfully familiar with the fact that two notes nearly, but not quite exactly, in unison with one another, produce, when sounded together, a throbbing sound commonly described as the phenomenon of "*beats.*" In the elementary theory of acoustics the cause of beats is shown to be the mutual *interference* of the two vibrations, one sound interfering with the other and silencing it, when one set of waves is half a vibration behind the other. Just as at certain points on the earth's surface there are no tides when a high tide and a low tide coming from different seas meet, so there is no sound when two sets of sound-waves meet in opposite phases. If the two notes differ just a little in pitch they will alternately reinforce and interfere with one another, and produce the throbbing sound of beats, the number of beats (or maxima of sound) per second being the same as the difference in the number of vibrations per second. If one tone makes m vibrations per second and the other n (a slightly smaller number, being a slightly flatter tone) there will be $m - n$ beats per second heard. If this number be not more than 3 or 4 per second the beats can easily be counted. When they get as rapid as 12 or 14 per second they come too fast to be counted, and are very harsh and grating. They are most disagreeable at about 33 per second; and if yet more rapid, are heard as a harsh, disagreeable, rattling sound quite different from a true note. Imperfect octaves and imperfect twelfths likewise cause beats; in fact there are beats heard for any imperfectly tuned consonance in which the frequency of the higher note is 1, 2, 3, 4, 5, . . . or any integer number of times that of the lower.

But along with the disagreeable and throbbing phenomenon of beats there arises another phenomenon when two notes not in unison with one another are simultaneously sounded. This is a low booming tone, to which musicians give the name of the "grave harmonic." If two stopped organ-pipes are brought to unison, and then one of them is sharpened by gradually pushing in its stopper, the beats are heard first slow, then fast, then unendurably rapid. But when they reach about twenty or thirty per second the low booming note begins, and rises gradually in pitch as the beats become too rapid to be discriminated. When the higher note has reached a point about half-way between unison and the octave note, the beats are practically imperceptible, and from this point the phenomena recur again, but in *inverted order*, the grave harmonic falls in pitch down to a low booming tone, while the beats begin again to be distinguishable, grow harsher, then become slower, until when the interval of the octave is reached they also disappear.

A great controversy with respect to these low tones of the grave harmonics has arisen in recent years, and though it smoulders from month to month, occasionally blazes up into vigorous flame. The controverted question is, What are these grave harmonics, and to what are they due? Also, What becomes of the *beats* when they occur so rapidly that the ear cannot distinguish them? The answer given by Dr. Thomas Young, and by Smith in his "Harmonics" (1749), was that the rapid beats actually passed into the grave harmonic, just as in the generation of any pure tone the separate vibrations (which, when very slow, are heard as separate sounds) blend into one continuous tone whose pitch depends upon their frequency. This view is maintained at the present day with great energy also by the famous acoustician Dr. Rudolph König of Paris. On the other hand, Helmholtz has emphatically maintained that the grave harmonic is not, and cannot be, thus accounted for, and has given very cogent reasons for thinking that it has another explanation; and in this view he is supported by Preyer, Lord Rayleigh, Ellis, Bosanquet, and all the best English physicists. Mere alternations of sound and silence, however rapidly they occur, cannot produce the same effect on the mechanism of the ear as a pure to-and-fro motion of the same periodic frequency. A tuning-fork which vibrates 100 times per second will give out waves which, falling on the ear, push the drumskin in, and draw it back that number of times per second. But a continuous tone interrupted 100 times per second by short periods of silence produces quite a different mechanical action on the mechanism of the ear. The writer of this article once tried to ascertain, by the experiment of rotating a vibrating tuning-fork upon its axis, whether the alternations of sound and silence which are observed as it is rotated would blend into a continuous tone; but no kind of blending took place. Another most conclusive proof that the beats and the beat-tones are distinct phenomena is that at a

certain speed both can be heard going on simultaneously. Helmholtz gives to the grave harmonic the name of "*difference-tone*," because its number of vibrations exactly corresponds to the difference between the number of vibrations of the primaries. Two notes whose frequencies are respectively m per second and n per second will give rise to a difference-tone whose frequence is $m - n$ per second, which is, in fact, just the same number as the number of beats between the two. König uses a different name, and agreeably to his (and Young's) theory, calls these notes "*beat-notes*," and classifies them into two sets, *lower* and *upper*, the lower beat-note being that corresponding to the beats between the lower note and the one that is sharper than it, the higher beat-note being that corresponding to the beats between the higher note and the octave of the lower. For example, if the notes c' and d'' are sounded together, their frequencies being in the ratio 8 : 9, there will be heard a beat-note whose frequency is relatively 1, or three octaves below the lower note. If c' and b' (a seventh) are sounded, their frequencies being in the ratio 8 : 15, there will be heard a beat-note of the upper series of relative frequency 1 (being the difference between 15 and 16), or also three octaves below the c'. So also the interval between c' and $\sharp f''$ (the twelfth-tone flattened by about a semitone, so as to make the ratio 8 : 23) will also give a beat-tone of relative frequency 1, being the difference between 23 and 24.

Now on Helmholtz's theory beats can only arise between vibrations so near together on the scale as to act on the same fibre of Corti in the ear (provided the vibrations be pure and free from upper partial tones), and they should therefore be audible not as *two tones* but as fluctuations in loudness of *one* tone. But when c' and d' are sounded we certainly hear *two separate* tones *plus* the low note which we call the grave harmonic. Helmholtz has therefore concluded that another explanation must be sought, and this he finds in a mathematical investigation of the resultant displacements due to superposing two tones, on the supposition that the vibrations of the primaries are so large that the moving forces are no longer simply proportional to the displacement, but are influenced by the squares or higher powers of them. He has shown that when this is the case combinational tones *must* arise whose frequencies correspond to the difference in the number of vibrations, and he further conjectures that to the dissymmetry of the drumskin and other vibrating parts of the ear is due the fact that the *squares* of the displacements can thus affect the resultant vibration. If so, all the combinational tones other than those of mistuned unisons must really arise in the ear itself and be *subjective* in character, as indeed Mr. Bosanquet, who has lately studied the matter most carefully, roundly declares.

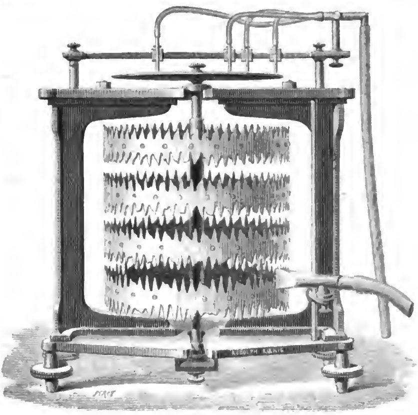


FIG. 1.

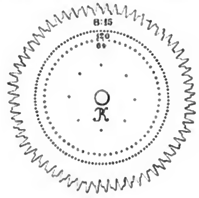


FIG. 2.

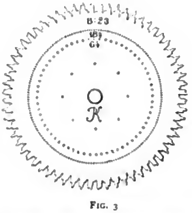


FIG. 3

Dr. König, however, undaunted by Helmholtz's reasonings, has returned to the contest with new weapons. He has repeated all his former experiments with new tuning-forks specially made of massive form, so as to be yet more perfect in tone, and finds his observations on beat-tones confirmed. He has further constructed a new instrument, the *wave-siren*, with which to establish his doctrine that *beats*, when too rapid to be heard separately, blend into a *beat-note*. In this instrument vibrations are set up in the air by blowing through a slit against the edges of a notched disk or rim which rotates rapidly upon an axis. In 1872 Dr. König constructed sirens on this principle, the indentations at the edges of the disks being simple harmonic curves, or "wave-forms," which therefore gave rise to simple tones. In the new wave-siren (Fig. 1) the indentations are determined in the following manner :—Two simple vibrations whose ratio is known are mechanically compounded together by machinery, and a resultant curve is obtained which *exactly* corresponds on a large scale to the resultant motion of the air when the two notes having this interval are sounded together. This compound curve is then set off very exactly round the periphery of a metal disk, and cut out in the metal with the utmost nicety. Fig. 2 shows the form of the curve (set out on the edge of a *flat* disk) for the interval of

the seventh, the ratio being 8 : 15. When this disk is rotated rapidly, and wind is blown through a flat nozzle held with its opening radially at the edge, two notes are heard giving this exact interval. *If the vibration is slow, beats are heard: if the vibration be rapid, the beat-note is heard.* In order to compare these notes the more accurately with a true combinational tone, the same disk is pierced by three concentric rings of holes, one with 64, another with 120, giving the ratio 8 : 15, and another, with 8 holes only, corresponding to the number of beats between 120 and the octave of the 64 set (128), that is to say, to the upper beat-note of the interval 8 : 15. When air is blown through the rings of 64 and 120 holes in the rotating siren-disk, exactly the same notes and same beats or beat-notes are produced as by the wave-curve at the edge. Here there can surely be no partial tones present to complicate the phenomenon. For greater convenience in comparing several combinations, the wave-forms are cut upon cylindrical rims mounted upon one axis as in the first figure, a flat disk pierced with holes being added above for comparison. In every case slow rotation gives beats, and rapid rotation the beat-note exactly as König's theory requires.

It remains yet to be seen what answer Helmholtz and the mathematical acousticians will give to the challenge thrown down by König in this beautiful and ingenious piece of mechanism. Meantime we may mention that Mr. Bosanquet of Oxford has just been examining the very same question, though by different means. He finds that all König's higher beat-tones can be accounted for by the assumption that the terms of higher orders become important in the mechanism of the ear when the displacements are considerable, and that therefore "by transformation" in this sense the variations of maximum displacement in the resultant wave give rise to to-and-fro vibrations of simple form having the same frequency as these variations, and therefore evoke *in the ear* a note whose frequency is the same as the number of beats. He is also positive that such tones exist only in the ear, and are inaudible in resonators. Lastly, he has satisfied himself that in all the cases of beats between mistuned consonances in which the higher note is (nearly) 2, 3, 4, . . . &c., times as rapid as the lower, *the beat consists of variations of intensity of the* lower *of the two primary tones.*

S. P. T.

HYDRODYNAMIC ANALOGIES TO ELECTRICITY AND MAGNETISM

FROM a scientific and purely theoretical point of view there is no object in the whole of the Electrical Exhibition at Paris of greater interest than the remarkable collection of apparatus exhibited by Dr. C. A. Bjerknes of Christiania, and intended to show the fundamental phenomena of electricity and magnetism by the analogous ones of hydrodynamics. I will try to give a clear account of these experiments and the apparatus employed; but no description can convey any idea of the wonderful beauty of the actual experiments, whilst the mechanism itself is also of most exquisite construction. Every result which is thus shown by experiment had been previously predicted by Prof. Bjerknes as the result of his mathematical investigations.

It has long been known that if a tuning-fork be struck and held near to a light object like a balloon it attracts it. This is an old experiment, and the theory of it has been worked out more than once. Among others Sir William Thomson gave the theory in the *Philosophical Magazine* in 1867. In general words the explanation is that the air in the neighbourhood of the tuning-fork is rarefied by the agitation which it experiences. Consequently the pressure of the air is greater as the distance from the tuning-fork increases. Thus the pressure on the far side of the balloon is greater than that on the near side, and the balloon is attracted.

Dr. Bjerknes has followed out the theory of this action until he has succeeded in illustrating most of the fundamental phenomena of electricity and magnetism. He causes vibrations to take place in a trough of water about six inches deep. He uses a pair of cylinders fitted with pistons which are moved in and out by a gearing which regulates the length of stroke and also gives great rapidity. These cylinders simply act alternately as air-compressers and expanders, and they can be arranged so that both compress and both expand the air simultaneously, or in such a way that the one expands while the other compresses the air, and *vice versâ*. These cylinders are connected by thin india-rubber tubing and fine metal pipes to the various instruments. A very simple experiment consists in communicating pulsations to a pair of tambours, and observing their mutual actions. They consist each of a ring of metal faced at both sides with india-rubber and connected by a tube with the air-cylinders. One of them is held in the hand; the other is mounted in the water in a manner which leaves it free to move. It is then found that if the pulsations are of the same kind, *i.e.* if both expand and both contract simultaneously, there is attraction. But if one expands while the other contracts, and *vice versâ*, there is repulsion. In fact the phenomenon is the opposite of magnetical and electrical phenomena, for here like poles attract, and unlike poles repel.

Instead of having the pulsation of a drum we may use the oscillation of a sphere; and Dr. Bjerknes has mounted a beautiful piece of apparatus by which the compressions and expansions of air are used to cause a sphere to oscillate in the water. But in this case it must be noticed that opposite sides of the sphere are in opposite phases. In fact the sphere might be expected to act like a magnet; and so it does. If two oscillating spheres be brought near each other, then, if they are both moving to and from each other at the same time, there is attraction; but if one of them be turned round, so that both spheres move in the same direction in their oscillations, then there is repulsion. If one of these spheres be mounted so as to be free to move about a vertical axis, it is found that when a second oscillating sphere is brought near to it, the one which is free turns round its axis and sets itself so that both spheres in their oscillations are approaching each other or receding simultaneously. Two oscillating spheres, mounted at the extremities of an arm, with freedom to move, behave with respect to another oscillating sphere exactly like a magnet in the neighbourhood of another magnetic pole. I believe that these directive effects are perfectly new, both theoretically and experimentally. The professor mounts his rod with a sphere at each end in two ways: (1) so that the oscillations are along the arm, and (2) so that they are perpendicular. In all cases they behave as if each sphere was a little magnet with its axis lying along the direction of oscillation.

Dr. Bjerknes looks upon the water in his trough as being the analogue of Faraday's medium; and he looks upon these attractions and repulsions as being due, not to the action of one body on the other, but to the mutual action of one body and the water in contact with it. Viewed in this light, his first experiment is equivalent to saying that if a vibrating or oscillating body have its motions in the same direction as the water, the body moves away from the centre of disturbance, but if in the opposite direction, towards it. This idea gives us the analogy of dia- and para-magnetism. If, in the neighbourhood of a vibrating drum, we have a cork ball, retained under the water by a thread, the oscillations of the cork are greater than those of the water in contact with it, owing to its small mass, and are consequently *relatively* in the same direction. Accordingly we have repulsion,

corresponding to diamagnetism. If, on the other hand, we hang in the water a ball which is heavier than water, its oscillations are not so great as that of the water in its vicinity, owing to its mass, and consequently the oscillations of the ball relatively to the water are in the opposite direction to those of the water itself, and there is attraction, corresponding to paramagnetism. A rod of cork and another of metal are suspended horizontally by threads in the trough. A vibrating drum is brought near to them; the cork rod sets itself equatorially, and the metal rod axially.

If a pellet of iron be floated by a cork on water and two similar poles (*e.g.* both north) be brought to its vicinity, one above and the other below the pellet, the latter cannot remain exactly in the centre, but will be repelled to a certain distance, beyond which however there is the usual attraction. The reason is that when the pellet is nearly in the line joining the two poles the north pole of the pellet (according to our supposition) is further from this line than the south one. The angle of action is less; so that although the north pole is further away, the horizontal component of the north pole repulsion may be greater than that of the south pole attraction. Dr. Bjerknes reproduces this experiment by causing two drums to pulsate in concord, the one above the other. A pellet fixed to a wire, which is attached by threads to two pieces of cork, is brought between the drums, and it is found impossible to cause it to remain in the centre.

Dr. Bjerknes conceived further the beautiful idea of tracing out the conditions of the vibrations of the water when acted on by pulsating drums. For this purpose he mounted a sphere or cylinder on a thin spring and fixed a fine paint-brush to the top of it. This is put into the water. The vibrations are in most cases so small that they could not be detected, but by regulating the pulsations so as to be isochronous with the vibrations of the spring, a powerful vibration can be set up. When this is done a glass plate mounted on four springs is lowered so as to touch the paint-brush, and the direction of a hydrodynamic line of force is depicted. Thus the whole field is explored and different diagrams are obtained according to the nature of the pulsations. Using two drums pulsating concordantly, we get a figure exactly like that produced by iron filings in a field of two similar magnetic poles. If the pulsations are discordant it is like the figure with two dissimilar poles. Three pulsating drums give a figure identical with that produced by three magnetic poles. The professor had previously calculated that the effects ought to be identical, and I think the same might have been gathered from the formulæ in Sir William Thomson's "Mathematical Theory of Magnetism," but this only enhances the beauty of the experimental confirmation.

Physicists have been in the habit of looking upon magnetism as some kind of molecular rotation. According to the present view it is a rectilinear motion. Physicists have been accustomed to look upon the conception of an isolated magnetic pole as an impossibility, but here, while the oscillating sphere represents a magnetic molecule with north and south poles, the pulsating drum represents an isolated pole. These are new conceptions to the physicist, let us see whither they lead us. The professor shows that if a rectilinear oscillation constitutes magnetism, a circular oscillation must signify an electric current, the axis of oscillation being the direction of the current. According to this view what would be the action of a ring through which a current is passing? If the ring were horizontal the inner parts of the ring would all rise together and all fall together, they would vibrate and produce the same effect as the rectilinear vibrations of a magnet. This is the analogue of the Amperian currents.

To illustrate the condition of the magnetic field in the neighbourhood of electric currents, Dr. Bjerknes mounted two wooden cylinders on vertical axes, connecting them by link-work, which enabled him to vibrate them in the same or opposite ways. To produce enough friction he was forced to employ syrup in place of water. The figures which are produced on the glass plate are in every case the same as those which are produced by iron filings in the neighbourhood of electric currents, including the case of currents going in parallel and in opposite directions.

The theory is carried out a step further to explain the attraction and subsequent repulsion after contact of an electrified and a neutral substance and the passage of a spark. But it is extremely speculative, and is not as yet experimentally illustrated, and I think that at present it is better to pass it by.

I believe that the professor will exhibit his experiments and give some account of his mathematical investigations, which have occupied his time for five years, to the Académie des Sciences this afternoon. His results have not been published before.

George Forbes

Paris, August 15

NOTES

John Duncan, the Alford weaver-botanist, has at last passed away, and his dust now lies under the earth whose beautiful children he knew and loved so well. He expired a little after noon on the 9th instant, in his eighty-seventh year, and was buried on the 16th in the old churchyard at Alford, in a selected spot, where a monument will soon be raised to his memory by the free-will offerings of those who admired his high character and pure-minded enthusiasm for science. The poor old man has not lived long to enjoy the comforts lately provided for him, but it is pleasant to think that this aged and unselfish student of nature passed the last days of his long and silent life in comparative affluence, and that he now rests in no pauper's grave. His life was so recently sketched in these pages (Nature, vol. xxiii. p. 269) that it is unnecessary here again to rehearse it. In December last, when it was ascertained that, after an unusually laborious life, winning his daily bread by weaving, carried on till beyond his eighty-fifth year, he had through failing strength been at last reluctantly forced to fall on the parish for bare support, an appeal was made in his favour by Mr. Jolly, H.M. Inspector of Schools, in the newspaper press throughout the country, and in our own columns. The response was speedy and ample, so that in a very short time a sum of 326*l.* was spontaneously sent for his relief, with every expression of admiration and regret from all parts of the land, and from most of our most eminent scientific men, whose kindly appreciation of his scientific labours was not unfrequently very aptly and memorably put. His pride and appreciation of all this kindness were genuine, deep, and child-like, and were expressed not seldom in piquant and touching terms; so that his numerous friends have the great satisfaction of thinking, that by their means, though he has departed sooner than was anticipated, they have helped to comfort the evening of his days. His constitution was of the healthiest type, and his tenacity of life remarkable in a frame so exhausted, and he only passed away when the last particle of the expiring taper was slowly consumed. As already told in Nature (vol. xxiv. p. 6), the money raised in John Duncan's behoof has been vested in seven trustees, under a trust-deed executed during his life. By its provisions his valuable books on botany and other sciences are bequeathed to the parish library of Alford for the use of the district; and all remaining funds are to be safely invested and the interest to be devoted for all time to the foundation of certain prizes, to be called by his name, for the promotion of the study of natural science, especially botany, amongst the children in certain parishes in and round the Vale of Alford. A memoir of the old man is now being written by Mr. Jolly, and will be anticipated with interest.

IN the death, on the 27th ult., of Mr. Hewett Cottrell Watson, at the age of seventy-seven, English botany has lost one of her most indefatigable workers. For the space of fifty years Mr. Watson has been a prolific writer on the geographical distribution of British plants, and on the distinguishing characters of the more "critical" species; and in these departments of botany he has left very few who can approach him in the extent and the accuracy of his knowledge. In addition to many smaller publications, and a vast number of contributions to periodical literature, the principal works with which his name will be associated are the "New Botanists' Guide" (1835-7), the "Cybele Britannica" (1847-59), and the numerous editions of the "London Catalogue of British Plants." His garden at Thames Ditton had long been an object of pilgrimage to botanists desirous of seeing growing specimens of rare or little-known species or varieties of British plants; and his judgment was the last appeal in questions of difficulty. In 1847 he spent three months investigating the flora of the Azores, which was then very little known, and added about 100 species to the flora of the Archipelago, many of which were new to science. Throughout life Mr. Watson was an ardent believer in phrenology; when a student at the University of Edinburgh he became acquainted with George and Andrew Combe; and was for a time editor of the *Phrenological Journal.*

PROF. RAOUL PICTET of Geneva, who has been giving his attention of late to marine architecture, announces, according to the *Times* correspondent, a discovery which, if his anticipations be realised, will effect a revolution in the art of shipbuilding and greatly augment the speed of sea-going and other ships. The discovery consists in a new method of construction and such an arrangement of the keel as will diminish the resistance of the water to the lowest possible point. Vessels built in the fashion devised by Prof. Pictet, instead of sinking their prows in the water as their speed increases, will rise out of the water the faster they go, in such a way that the only parts exposed to the friction of the water will be the sides of the hull and the neighbourhood of the wheel. In other words, ships thus constructed, instead of pushing their way through the water, will glide over it. According to the professor's calculations, in the accuracy of which he has the fullest confidence, steamers built after his design will attain a speed of from 50 to 60 kilometres the hour. A model steamer on the principle he has discovered is in course of construction at Geneva. The machinery has been ordered at Winterthur, and when ready the new vessel will make her trial trip on Lake Leman.

THE Electrical Exhibition, though now open to the public, seems far from being completely arranged. Our Paris correspondent writes that the English section was opened on Sunday, a result due to the personal exertions of the Earl of Crawford, the English Commissioner, which has caused great satisfaction. The organisation of the English section is highly approved. The evening sittings have been postponed for an indefinite number of days, owing to a series of mistakes in the engineering department. The electrical railway is not ready. In spite of these drawbacks the receipts of the first three days were from 4000 to 5000 francs each. On Sunday they were largely increased, although the fees were diminished by half. We hope shortly to refer to the Exhibition in detail. Independently of the Catalogue, the administration of the Exhibition has published a handbook on Electricity and its Applications, by Armingaud, Becquerel, Bert, Blanco, Breguet, Clerac, Deprez, Fontaine, Mascart, Reynaud, and others. *L'Électricité* has published a "Petit Vocabulaire raisonné" of every word used by electricians, with an introduction by W. de Fonvielle.

IT is announced that Miss Pogson, daughter of the Madras Government Astronomer, has been appointed Meteorological Reporter to the Government of that Presidency. Miss Pogson has for some years discharged with great ability the duties of Assistant Government Astronomer.

WE believe that the Royal Commission which has been constituted for the purpose of inquiring into and reporting upon the facilities for technical education in various countries is now practically complete. It will comprise Messrs. Samuelson, Slagg, Stevenson, and Woodall, the members respectively for Banbury, Manchester, South Shields, and Stoke-upon-Trent. Mr. Swire Smith of Huddersfield, Prof. Roscoe of Manchester, and Mr. Philip Magnus, the director of the City and Guilds of London Institute, have also accepted invitations, and Mr. G. R. Redgrave of the Science and Art Department will probably be selected to accompany the Commission as secretary. It is expected that the Commission will commence its travels about the middle of October.

THE British Association having decided to hold its annual meeting for 1882 at Southampton, a large and influential committee, including the Corporation and magistrates of the borough and the clergy and ministers of all denominations, has been appointed to make the necessary arrangements. A subscription and guarantee fund to cover the requisite expenses of the meeting has been commenced.

THE meeting of the International Congress at Bordeaux on the Phylloxera having been antedated to August 29, is now postponed, on account of the elections, till October 10.

THE Epping Forest and County of Essex Naturalists' Field Club held a Field Meeting at Chelmsford on Saturday, August 13, in conjunction with the subscribers to the "Essex and Chelmsford Museum." The Chelmsford Museum was visited under the guidance of the Rev. R. E. Bartlett, M.A., the hon. curator, and Mr. E. Durrant, the hon. secretary. After lunch the whole party proceeded in drags to Danbury Hill, the ancient camp of which was visited under the guidance of Mr. H. Corder; the company then assembled to hear an address by Prof. G. S. Boulger on "The Origin and Distribution of the British Flora." About six o'clock the party returned to Chelmsford to tea at the "Saracen's Head" Hotel, and an ordinary meeting of the Essex Club was held, the President, Mr. R. Meldola, occupying the chair. The President communicated on behalf of General Pitt-Rivers the report on the excavation of the ancient earthwork at Ambresbury Banks in Epping Forest. It appears that this investigation has been carried out with considerable success, a number of fragments of pottery of British construction having been found beneath the rampart on or near the old surface of the ground. The Club has thus so far settled the date of the camp by a single cutting, and the current theory that it was the work of the Romans must be abandoned. Although undoubtedly British, further excavations will be required before it can be decided whether it dates from before or after the Roman conquest. We are glad to see that the Great Eastern Railway Company has assisted the Club to a great extent by allowing the members to travel at greatly reduced fares on any of their lines within the County of Essex on the occasion of field or of ordinary meetings.

THE *Daily News* correspondent writes that the Swiss Seismological Commission, which, by the co-operation of its numerous members and correspondents, continues the work of simultaneous earthquake observation, has just issued a report on the earthquake of July 22. This shock was felt over a wide area. In France it extended over the departments of Drôme, Isère, Savoy, Upper Savoy, Saône et Loire, Ain, Jura, and Doubs. In Italy it affected chiefly the high valleys of North-Western Piedmont. In Switzerland the movement was observed in the cantons of Geneva, Vaud, Friburg, Neuchâtel, Solothurn, Basel, and the

western districts of Aargau and Berne. From Valence to Basel, and from Chalons-sur-Saône to Suza and Zinzal, the region of disturbance included both sides of the Jura Mountains, besides traversing the great chain of the Alps. It affected an area 350 kilometres long and 250 kilometres wide, equal to 8000 square kilometres of surface. There were two very slight shocks on the evening of July 21, and a feeble shock at 12.10 on the morning of the 22nd. The principal shock, which took place at 2.48 a.m., was followed at 3.30 and 4.30 by two oscillations that were only just perceptible to the senses. The great shock consisted of two quakes and several smaller, but distinct, vibrations. In some localities as many as ten vibrations were counted. Relatively to its extent, the shock was intense; in the neighbourhood of Chambéry and Aix-les-Bains, chimneys fell and walls were fissured. In Switzerland the shock was stronger near the Jura than nearer the Alps, and especially strong at Geneva, in Vaud, and in Neuchâtel. Prof. Forel, who edits the report, remarks on the singular variations in the intensity and direction of the shock even in the same neighbourhood. These differences, which have been observed in previous earthquakes, are too great to be due solely to errors of observation. An earthquake is often more felt in one quarter of a town than in another; and as this variation is irregular, a locality that hardly feels a shock at all on one occasion feeling it on another, it cannot arise from differences in the density of the underlying strata. Prof. Forel offers no explanation of this phenomenon, albeit he thinks it ought to be explained, and craves for it the particular attention of his brother seismologists.

DR. K. VON FRITSCH of Halle discusses the subject of earthquakes in the last issue of the *Verhandlungen* of the Berlin Geographical Society. He maintains that the cause of earthquakes must be sought for at a rather small depth, the greatest depth ascertained not exceeding ten to fourteen miles, and usually far less, whilst rather feeble forces produce earthquakes which are felt at great distances. It is known that Krupp's hammer, which weighs 1000 centners, and falls from a height of three metres, produces sensible concussions on a surface of eight kilometres diameter; whilst the recent explosion of the Leimbach dynamite manufactory was felt at Halle and Merseburg, forty-one and forty-five kilometres distant. Whilst showing how easily concussions are produced by causes comparatively feeble, Dr. Fritsch points out how earthquakes might be and must be produced by the increase and decrease of volume of rocks under the influence of physical and chemical forces, and by concussions, by the opening of crevices in rocks, and by the subsidence of masses of rocks due to these agencies. Many schists are subjected, as is known, to extension, and when crevices arise the schists must enter into oscillations which must produce very varied phenomena, according to the direction and the force of the oscillations, much like to what we see in the oscillations of tuning-plates. Dr. Fritsch concludes by saying that future researches as to the causes of earthquakes ought to be directed especially to the study of the geotectonical conditions of the localities where they occur.

IN the course of the excavations for the new fort at Lier, in the neighbourhood of Antwerp, a number of bones of extinct animals, mammoth's teeth, and the almost complete skeleton of a rhinoceros have been dug up. It was in the same district that, in 1760, was found the immense skeleton of a mammoth, which has been preserved in the Natural History Museum at Brussels.

THE Faure accumulators have been tried again by the Paris Omnibus Company on a tramway with a carriage arranged for the purpose. The experiment is said to have been highly successful.

THE Committee formed some time ago for the exploration of the subsidences in Blackheath have published a report, in which, while giving an account of their proceedings, and the opinions of various geologists for the probable causes of the subsidences, they themselves have come to no definite conclusion.

A CONGRESS has been opened at Bordeaux on the education of the deaf and dumb. In connection therewith the *Journal Officiel* publishes a series of articles by M. Claveau, General Inspector of "Établissements de Bienfaisance," who tries to prove that the method of teaching the deaf and dumb how to speak was invented and practised by St. John of Beverley, Archbishop of York, in 865, and fully described by the Venerable Bede.

THE Meteorological "Centralanstalt" founded by the Swiss Naturalists' Society at Zurich has become a Government Institution by a decree of the Swiss Senate, and now bears the title "Swiss Meteorological Centralanstalt." Herr R. Billwiller has been appointed director, while the Swiss Home Secretary and a special Commission will superintend the Institution.

THE Royal University Bill (Ireland) on Tuesday night last was read a third time in the House of Commons, having been sent down from the House of Lords. It now only awaits the Royal Assent. The programme of the Natural Science course seems framed in accordance with modern views, and when the Scholarships and Exhibitions shall be finally settled by the Senate, we will probably refer again to the subject.

WE notice in the last number of the *Zeitschrift* of the Berlin Geographical Society (vol. xvi. fascicule 3) an interesting description of spring in Madagascar, from the pen of the late Herr J. M. Hildebrandt, who died on May 29 at Antananarivo. Spring arrives about the middle of November, when the cold south-eastern wind which blew throughout the winter, leaving its moisture on the eastern slopes of the highlands, covered with thick forests, and driving before it the savannah fires, gives place to the north-western wind which brings warmth and moisture. The revival of nature under the influence of this wind is well described by Herr Hildebrandt, and his paper contains valuable information as to the flora and fauna of Madagascar.

THE additions to the Zoological Society's Gardens during the past week include an Orange-winged Amazon (*Chrysotis amazonica*) from South America, presented by Mr. R. Seyd; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Sir Patrick Colquhonn; a Herring Gull (*Larus argentatus*), British, presented by Mr. E. A. Brown; a White-crested Touracou (*Corythaix albocristata*) from South Africa, presented by Capt. T. G. Steer; a Black-eared Marmoset (*Hapale penicillata*) from South-East Brazil, presented by Mrs. Alsop; an American Tapir, ♂ (*Tapirus terrestris*) from Trinidad, presented by Herr Fritz Zurcher; two West Indian Agutis (*Dasyprocta cristata*), three Garden's Night Herons (*Nycticorax gardeni*), and two Martinican Doves (*Zenaida martinicana*) from the Antilles, presented by Mr. H. T. Burford Hancock, F.Z.S.; two Stock Doves (*Columba œnas*), British, presented by Mr. A. E. C. Streatfield; two Topela Finches (*Munia topela*) from China, a Nutmeg Finch (*Munia punctularia*) from India, a Javan Nutmeg Finch (*Munia nisoria*) from Java, a Francis Eagle Owl (*Bubo poensis*) from West Africa, two Aldrovandi's Skinks (*Plestiodon auratus*), and two Pantherine Toads (*Bufo pantherinus*) from North Africa, a Bay Antelope (*Cephalophus dorsalis*) and a Water Chevrotain (*Hyomoschus aquaticus*) from West Africa, purchased; and two Common Marmosets (*Hapale jacchus*) from Brazil, deposited. In the Insectarium may be seen full-fed larvæ, now spinning up, of the Atlas (*Attacus atlas*) and Ailanthus (*Attacus cynthia*) Silk-Moths, also freshly-hatched ones of the Marbled White Butterfly (*Arge galatea*) and Scarlet Tiger Moth (*Callimorpha dominula*). Amongst the aquatic forms

examples of *Hydrous piceus* (the large Water-Beetle), *Pelobius hermanni*, *Notonecta glauca* and *Argyroneta aquatica* are at present exhibited.

GEOGRAPHICAL NOTES

THE current number of the Geographical Society's *Proceedings* gives the paper recently read by Mr. Whymper on some of the geographical results of his expedition among the the Ecuadorian Andes, with a diagram of his routes, while Mr. W. G. Lock supplies a contribution on Iceland, which is published at a convenient season for tourists. Mr. Lock's paper refers chiefly to the Askja volcano, the largest in the island, and is illustrated by a map of the east coast of Iceland. In the "Geographical Notes" a brief reference is made to this season's Arctic expeditions, and we are informed that Mr. Leigh Smith has lately sailed from Peterhead on his fifth Arctic expedition; and on reaching Franz-Josef Land he intends to construct a house and refuge at Eira Harbour, and afterwards to get as far north as possible. A very interesting account is given, from a letter recently sent home by M. de Brazza, of the results of his explorations and of the advantages of his route to Stanley Pool by the Ogowé as compared with Mr. Stanley's along the north bank of the Congo. After some news respecting Russian travellers an account is given of Messrs. Soltau's and Stevenson's journey from the Irawaddy to the Yangtsze, to which we recently referred. Under "Correspondence" is a letter from Major H. G. Raverty on the Dara'h of Nur, which does not leave a pleasant impression on the reader's mind.

LORD ABERDARE has finally accepted the office of representative of the British Government at the International Congress of Geographers at Venice, and he will of course act as chief delegate of the Geographical Society. The India Office and the Admiralty are sending maps, charts, &c., to the Exhibition, and the former will be represented by Lieut.-General Sir H. Thuillier, late Surveyor-General of India, and the latter by Sir F. J. Evans, Hydrographer of the Navy. It is probable that nothing further will be done to represent this country officially, as the Treasury sternly decline to furnish funds.

THE Italian North-African explorers, Massari and Matteucci, to whose journey we have repeatedly referred, instead of returning by Tripoli, as was expected, struck across the Continent and came out at the Gulf of Guinea. Only a few days ago they arrived at Liverpool, and it is sad to record that, after so successfully accomplishing an arduous work, Dr. Matteucci has succumbed to African fever. He died on the morning of his arrival in London last week; his body has been conveyed to his native city, Bologna. Matteucci was only twenty-nine years of age.

LETTERS from Zanzibar of the 1st ult. notify the arrival there of Mr. Thomson, the African traveller, whose services have been engaged by the Sultan to examine and report on the mineralogy of the mainland. It is his Highness's wish that Mr. Thomson's first surveys should be devoted to the discovery of coal mines, of which several are said to exist not far from the coast. His Highness writes that he intends sending the explorer shortly to Makindarry, which is to be the centre of his future operations.

LETTERS from the steamer *Oscar Dickson* have been received at Gothenburg. The steamer, as our readers will remember, was frozen in at the mouth of the Yenisei River in 72° lat. N., and between 76° and 77° long. E. The winter was successfully passed, the difficulties the crew experienced were great, however. The sun was below the horizon for seventy days, and the cold rose to − 41° C. During March and April enormous masses of snow fell, so that it covered the ice to a height of seven feet above the ship's deck; the thickness of the ice was seven and a half feet.

WHAT might have been the climate during the Glacial Period is the subject of an interesting paper published by Dr. Woeikoff in the last issue of the *Zeitschrift* of the Berlin Geographical Society (vol. xvi. fasc. 3). It is well established now that for the formation of glaciers, not only a sufficiently low temperature is necessary, but also a sufficient supply of moisture in the atmosphere. Thus, on the Woznesensky gold-mine, which lies at a height of 920 metres and has a mean temperature of − 9° Celsius, but a rather dry climate, we have no glaciers, nor in the Verkhoyansk Mountains, where the mean temperature is as low as − 15°·6, and the temperature of January is − 48°·6. To show these differences Dr. Woeikoff prepares a table of the temperatures at the lowest ends of glaciers, and we see from his figures that in Western Norway, at the end of the Jostedal glacier (400 metres high), the mean temperature is 4°·8 Cels., 5°·8 at the end of the Mont Blanc glaciers (1099 metres), 6°·8 at the Karakorum glaciers in Tibet (3012 metres), and even 7° on the western slope (212 metres) of the New Zealand highlands, and 10° on the eastern slope (835 metres). In other countries, as, for instance, on the Mounkou Sardyk Mountain, in Eastern Siberia (3270 metres), the mean temperature at the end of the glaciers is as low as − 10°·2, and − 2°·4 in the Daghestan Mountains of the Caucasus. Thus the difference of mean temperatures at the lower ends of glaciers reaches as much as fully 20°. Besides we see that, provided the quantity of rain and snow is great, glaciers descend as low as 212 metres above the sea-level in a country (New Zealand) which has the latitude of Nice and the mean temperature of Vienna and Brussels, that is, higher than that of Geneva, Odessa, and Astrakhan, whilst the average temperature of winter is higher there than that at Florence. Further, Dr. Woeikoff discusses the rather neglected influence of large masses of snow upon the temperature of a country during the summer, and by means of very interesting calculations he shows how much the temperature of summer in higher latitudes is below what it ought to be in consequence of heat received from the sun, and *vice versâ* during the winter, these differences being due on the one side to the refrigerating power of snow, and on the other side to the heating power of sea-currents. In a following paper he proposes to discuss the other causes which might have influenced the climate of different parts of the earth during the Glacial period.

WE notice in the *Verhandlungen* of the Berlin Geographical Society (vol. viii. fascicule 5) a full report on the surveys which were made in the Russian Empire, including Caucasus, Siberia, Turkestan, and the Orenburg military district, during the year 1880. This is translated from the official report published in the organ of the Ministry of War.

DURING the last session of the German Reichstag, Dr. Thilenius, Prof. Virchow, and Herr von Wedell Malchow presented a petition to the Government requesting the participation of Germany in the exploration of the Polar regions proposed by the late Karl Weyprecht in the interest of meteorology, geology, and other sciences. It is now announced that the German Government will probably soon take steps in this direction, and will first give its principal attention to securing the co-operation of other nations.

HEFT VIII. of Petermann's *Mittheilungen* begins with a paper by Dr. Danckelman on the Temperature Conditions of the Russian Empire, after Dr. Wild. The other papers are on M. Desiré Charnay's Expedition in Central America, Dr. I. B. Balfour's visit to Socotra, the Irawaddy above Bamo according to the data collected by the Indian Pundit in 1879–80 (with a map), and an article on the unfortunate Flatters Expedition by Dr. Rohlfs.

IN the *Bulletin* of the Antwerp Geographical Society (tome vi. 2e fasc.) is a paper of much interest by Dr. Delgeur on the Geographical Knowledge of the Ancient Egyptians.

MESSRS. LONGMANS AND STANFORD have published an enlarged edition of the Alpine Club Map of Switzerland. As it is issued in a number of separate sheets, it ought to prove useful to tourists.

THE Geographical Society of Lisbon has resolved to send an exploring party into the Sierra d'Estrella for scientific investigation. The mountain chain in question has never been scientifically explored.

IN 1879 Mr. L. Loth, a Government surveyor in Dutch Guiana, made a survey of a considerable portion of the River Saramaca, and his map of its course, on the scale of 1 : 400,000, together with an account of his expedition, has lately been published in the *Transactions* of the Amsterdam Geographical Society.

THE *Oesterreichische Monatsschrift für den Orient* of this month contains an interesting article on the new Conseil de Santé et d'Hygiène publique en Egypte by Prof. v. Sigmund, a well-known authority of the Medical High College of Vienna. Amongst various other papers we may mention an essay on Japanese paper manufacture by Dr. Rudel of Dresden, and an article on the wines of Cyprus by Dr. Richter of Larnaca.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

The Test supplied by Change of Refrangibility

WE have then got so far. Limiting our studies to iron we find that the prominence spectrum is made up of one set of lines seen in the terrestrial spectrum, and the spot spectrum made up of another set. And more than this, if we add the lines seen in the prominence and spot spectra together we do not then by any means make up the complete spectrum.

It is fair to ask the following question :—Have we any other means of establishing this extraordinary fact of the separation of the iron lines in spots and storms? We have. Reference has already been made to the change of refrangibility of the lines brought about by the change of velocity of movement of the various solar vapours. But if, as already hinted, the lines of iron behave to each other in precisely the same way as the lines of two perfectly distinct substances behave to each other; then if we observe changes of refrangibility in the iron lines, both in spots and flames, we should get the same differentiation as we have already got in the lines thickened or intensified in the spectra of spots and flames.

We will now see the results which have been obtained along this line of research, and it should be pointed out that it is not a method by which it is easy in a short time to accumulate a large number of observations, because metallic prominences are very rare except at the sun-spot maximum, and in the case of spots we not only want a spot, but we want that spot to be in a very considerable state of commotion, in order that the change of refrangibility may be obvious enough to enable us to record the phenomena.

So far as this inquiry has gone at present we have only observed the lines contorted in spots.

In the diagram (Fig. 37) the zig-zag lines indicate the iron lines which changed their refrangibility in a number of spots observed at the end of last year. The point is that, although we have a great many of the iron lines bent, twisted, contorted—with their refrangibility changed, yet some of the iron lines mixed with them give us no indication of movement. All these observations have been made upon lines seen at the same moment in the same field of view. Observations of this nature exist twelve years old, but no importance can be attached to them, for the reason that the phenomenon was not understood, as I hope it is understood now, and precautions were not taken in the observations then made to show that no motion of the slit across the spot took place in the interval between the two observations. For, of course, it is not fair to compare a line which one sees in one part of the spectrum with a line seen in another, unless one is absolutely certain that the slit has not moved on the sun's image; because one-thousandth part of an inch on the sun's image means a good many miles on the sun. Referring to Fig. 37 we have, at wave-length 5366–70, three lines, two in motion, and one at rest, all belonging to a well-known group of iron lines. At a later date we have the line at 5382 at rest, while that at 5378 is in motion. Thus it will be seen that these points and others prove there is just as much individuality in the way in which the lines of iron change their refrangibility as there is in the way in which one particular line, and then another, is thickened in a sun-spot or brightened in a prominence; and if we go further we find this very interesting and additional fact, that the lines which are not contorted are in a great many cases precisely those lines which are seen in the flames, but not in the spots.

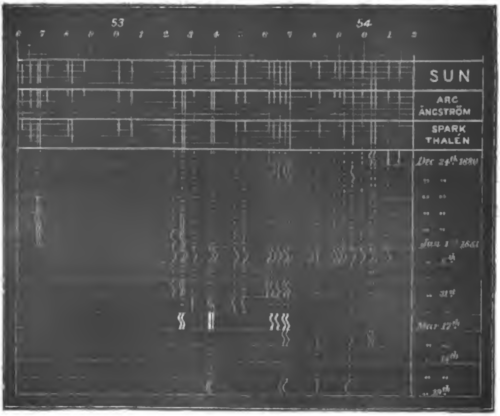


FIG. 37.—Different rates of motion registered by different iron lines.

It is seen therefore that the evidence afforded by change of refrangibility is of like nature to that afforded by the thickening of lines in spots and brightening of lines in flames.

The explanation which lies on the surface is that the vapours in the flames produce one set of lines in one place or at a certain temperature, and the vapours in the spots produce another.

Sometimes these vapours are mixed up by up-or-down rushes, and sometimes therefore the lines are common.

Bearing of these Observations on the Origin of the Fraunhofer Lines

At the end of the last lecture it was pointed out that the observations we are now discussing seem to indicate that in time we may be able to say that the absorption to which any particular Fraunhofer line is due takes place in a certain region of the solar atmosphere, whereas formerly we could only say that it was produced by absorption *somewhere*.

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 324.

The time has now come, I think, to go into this question in more detail.

Let us consider the maps. Of the 96 iron lines in that first region which we considered only 4 are seen in the flames; 92 of those therefore must not be looked for in the flame region, for the reason that twelve years of patient work have not divulged their existence. Again of these same 96 lines only about 32 are seen in the spots at all extensively affected. It is useless therefore to look for the remaining 60 lines or thereabouts in the same spot region, for the reason that they have been looked for for a long time without being seen equally widened.

Of course it must be remembered that these changes are due to *change of intensity*, and that other lines may be there of an intensity so low that they have escaped the keen eyes of those anxious to chronicle them. Still it will be acknowledged, I think, that the method of treatment I have adopted is the best open to us, and is a fair one on the whole.

The facts being so, it looks really as if the origin of the mass of the absorption to which the Fraunhofer spectrum of iron is due is to be sought in a region of the solar atmosphere much nearer to the place assigned to it by Kirchhoff originally than to that lower region where we considered we were driven to place it when the new method was first established. When the new method had been working for some considerable time observers recorded hydrogen with magnesium underlying it, and with sodium underlying that. And since they were metals of low atomic weight and vapour density we were justified in considering them as occupying the highest levels—the very extreme limit of the solar atmosphere.

It was therefore fair to argue that if the substances of the lowest atomic weights were really close to the photosphere, those of highest atomic weights were really in the photosphere itself, and therefore, being in the photosphere, the absorption by means of which we were able to determine their existence really took place in or near the photosphere.

This later work, I think, seems to show that that view requires reconsideration; and it may well be that subsequent work will show that those Fraunhofer lines, which we do not trace in flames and which we do not trace in the spots, are probably absorbed in a cooler, higher region of the atmosphere, much more nearly occupying the place assigned to the general atmosphere by Kirchhoff than that which has been given to it by later observers. If we accept this the work becomes a little plainer, and the reason that we get such an excessively simple spectrum in the lower reaches of the sun is because the more complex vapours exist at a considerable elevation above them, and as the interior of the sun must be hotter than any of its envelopes, no cold substances—nothing approaching the solid state which we have learnt for many years gives us the most complete spectrum of the substance—nothing approaching a solid can enter those charmed regions.

Therefore we are also again driven to the view that these cooler vapours—vapours much nearer the solid state, much more condensed, much more complex than those which can exist alone in the hottest layer—probably originate the great mass of absorption; that is, many lines not traced either in spots or flames are produced in the higher regions.

If this be so, the Fraunhofer spectrum is really not the spectrum of any particular part of the sun; but because it contains lines thickened in the spots, lines brightened in the flames, and other lines about which we know nothing, it must really be the summation of the absorption of the different strata which compose the solar atmosphere; so that chemically the solar atmosphere, with regard to the iron spectrum, gets more and more complex every mile we go upwards. Of course, too, if this is good for iron it is good for every other substance which we believe to exist in, or to have some connection with, the solar atmosphere.

Further Test supplied by this View

If this be so we really can go on with our tests; we can bring the laboratory into the field, and we must learn in our laboratory experiments to make abstraction of those lines which are due to the more complex masses reduced by the transcendental temperature which we employ, if there is any truth in the view that I am bringing before you. In a laboratory experiment, for instance, when we want to observe the vapour of iron we have to employ two poles of solid iron. We have no means, such as are afforded us by the sun, of shielding the precise part we want to observe by a considerable number of envelopes of gradually-increasing temperature, so that even if we can get the highest temperature in the laboratory this result of the highest temperature will be cloaked, masked, and hidden by all those results, by all those simplifications which have been brought about to produce that precise effect of the highest temperature. So that the only thing we can do is to watch the intensities of the lines when we considerably change our temperature. I am speaking now of iron. I will show by and by that for some other substances there is a method which enables us to get over this excessive difficulty, for no doubt a very great difficulty it is; but in the case of iron, that really is the only thing that remains to us. Fig. 38 will give an idea of the way in which we may be misled if we do not examine our light source with the greatest care. It is engraved from a photograph of the spectrum taken between two poles of a Siemens machine, moistened with a salt of calcium, an image of the vertical poles having been thrown on the vertical slit.

It is seen how wonderfully we get the simplifications brought about by the electric current, depicting themselves in two perfectly distinct ways. The lower part gives the spectrum of the positive pole, and the upper part of the spectrum of the negative pole. In the first place it will be seen that there is no axis of symmetry for these lines; some of them elongate considerably in one direction; others of them elongate considerably in the other; some of them are exceedingly short, and only appear close to that region of the negative pole where the lines broaden; others again are brighter in the region much nearer the middle of the field. Others of the lines start from a region far removed from the arc; others again seem to start almost in the arc itself. Now this not only reminds one of what one sees in a solar storm, but it shows us most distinctly that even in the electric arc, when we have had time to study it sufficiently, these very simplifications which we have been so long in search of may be recognised eventually and permanently recorded.

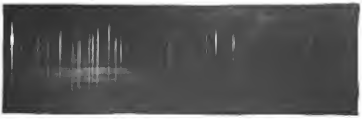


FIG. 38.—Photograph of the spectrum of the poles, showing that the lines start and end in different levels.

Tests supplied by the Variations between Solar and Terrestrial Spectra

Attention has been called to Kirchhoff's statement that the existence of the terrestrial elements in the sun is established by the fact of the coincidence of wave-length and *intensity* between the lines visible in our laboratories and the lines recorded as existing in the solar spectrum.

We have now arrived at a point when we can discuss this with advantage.

I propose to show first that the statement is not true; and, secondly, how the tests supplied by the variations from terrestrial spectra can be explained on, and bring most valuable confirmation to, my view. We are now able to say that at least two causes are at work, and they will require to be discussed separately.

But first as to the facts. We have already seen what enormous differences there are in the spectrum of calcium under different conditions. In the diagram of the calcium spectrum (Fig. 28) we saw that H and K, the most important lines in

the sun, are really thin lines at the temperature of the electric arc, but that they kept intensifying and were rendered visible almost alone, when, instead of using the electric arc we used an induced current of considerable tension. But when we pass from the case of calcium, which occupied the attention of solar observers several years ago, to other elements, and when we go still more into the minute anatomy of the thing, we find that the further we go the less final is the statement that the matching in intensity of the lines is perfect.

Nor is this all. *Not only is the matching less perfect in intensity, but whole reaches of lines in various spectra are left out which cannot be accounted for on the long and short principle.* It has been before pointed out that of the 26 lines of aluminium, 2 only being left in the solar spectrum is easily explained, because the 24 dropped were short lines. But when we come to other elements, we find of adjacent lines—lines of equal length, and which, so far as we can gather, ought to be equally represented in the sun—one is absent, and one is present, probably with more intensity than it would seem to deserve from its behaviour among other lines of the spectrum. A table will best exhibit the sort of variation that crops up and insists on being recorded when the solar spectrum is photographed in anything like the detail which it absolutely demands. The method of recording will be at once understood.

Metal.	Wave-length.	Intensity in sun. 1=darkest.	Intensity in photograph. 1=brightest.	Intensity. Thalèn. 1=brightest.	
Mn	4083·0	4	2	5	
	4083·5	3	2	3	
Fe	4197·5	1	2	–	
	4198·1	3	2	–	Stronger line.
Co	4118·0	2	1	–	
	4120·5	4	1	–	Stronger line.
Ni	4458·6	2	2	–	
	4647·8	3	2	5	
Cr	4344·4	4	3	2	
	4351·8	3	3	2	
Mo	4706·5	3	1	4	
	4757·5	0	1	4	
W	4842·0	5	1	1	
	4887·5	3	2	2	
Ti	3980·8	2	1	–	
	3989·25	1	1	–	
Zn	4679·5	3	1	1	
	4721·4	4	1	1	
Pt	4442·0	4	2	4	
	4551·8	3	2	2	
Pd	3893·0	1	1	–	
	3958·0	3	1	–	Stronger line.
Zr	3957·22	2	1	–	
	3990·45	3	1	–	Stronger line.
Di	3989·65	0	1	–	
	3993·98	3	1	–	
Rb	4201·0	0	1	2	Stronger line.
	4215·5	3	1	0	

Now if Kirchhoff's view be anything like a representation of the whole truth there ought not to be any difference between these intensities; the line least intense in the photograph ought to be least intense in Thalèn's tables, and if it existed in the sun at all, it ought to be least intense amongst the Fraunhoferic lines, but as a matter of fact, there is an absolute inversion. The cobalt line 4120·5 is four times as intense in the sun as in the photograph; in the titanium line 3989·25 the intensities are equal; while in tungsten 4842·0 they are inverted, being represented as of minimum intensity in the sun, and maximum by Thalèn and in the photograph. In the sun one of the lines of iron is given as of first, and the other as of third intensity, while in the photograph they are both of second order. Again, in didymium we get a first order line recorded in the photograph which is absent from the sun altogether, whereas another line of the first order near it is there as a line of small intensity; so also in rubidium, and so we might go on. Indeed it is evident that the moment we go into minute details in this work we find that the general statement requires a very considerable amount of modification. And in addition to that too, there comes the question, how on this theory of the identity of the nature of the substances in the earth and the sun, are we to account for the bright lines seen in the sun itself—for the bright lines seen in the photosphere, to say nothing of those seen in the chromosphere — which have no corresponding Fraunhofer lines at all—lines so numerous that in a prominence of moderate complexity we may say that half the lines are absolutely unknown to us? Now when the other lines observable under these conditions—lines which we can get accurately, are lines known to us (we are dealing with the product of the very highest temperatures which we can command) we are justified, I think, in imagining that these lines which we do not get at, are lines which we could get at if we could proceed a little further. They elude our grasp; we know nothing about them; we put a query against them all because we cannot get at that stage of temperature at which they are produced.

There is one very beautiful case of this kind that comes out from Tacchini's observations (Fig. 39). From the beginning of February, 1872, Tacchini had observed the two iron lines 4922·5, 5016·5, when suddenly the whole rhythm of his observation was broken, and at the end of December, 1872, these iron lines ceased to be visible in the flames altogether.

On no one occasion after this for some time was either of these iron lines observable, but from January to September, 1873, he saw two lines of wave-lengths, 4943 and 5031, about which absolutely nothing whatever is known; so that it really is, I think, a perfectly justifiable suggestion that these lines are the spectrum of a substance which exists in the flames which is produced at a much higher temperature than that needed to give us those other forms of "iron" which produce the lines in the spots.

That is a suggestion which is obvious from a reference to the maps, and if it is correct we must acknowledge that when the sun was in that intense state of quiescence that there were no downward currents—nothing to bring the cooler vapours from the higher regions of the sun down to obstruct the general tenour of the solar way in the flame region, that at last, in consequence of this wonderful tranquillity, even the iron lines—the only two lines which indicate the presence of iron in the flames—faded away because iron, as we know it, faded away. There is no other explanation that I know of. In addition to those two lines we have two other lines about which we know nothing, except that they are probably due to a temperature which we cannot approach.

Special Test with regard to Iron

Part of the work which has been undertaken in connection with this special branch of the investigation, has been a careful inquiry into the changes brought about in the spectrum of iron by exposing it to as widely different temperatures as possible. The research is a very laborious one, and it may be some day we shall get a very much better record than that which my assistants and myself have produced; what we have been able to do we have done over the region of the spectrum which we have already worked over in the spots and flames.

In different horizons we have recorded the results observed when we use either the arc or the coil, or the oxyhydrogen flame or the Bessemer flame or some other light-source, and we vary in each case, as far as can be, the temperature employed. For instance, when we use the quantity coil we use a big jar, a little jar, and no jar at all; and the same with the intensity coil. Now if this map is carefully studied,[1] it will be seen that the intensity of the lines is very considerably changed when we pass from one set of observations taken under one set of conditions, to another set taken under other conditions. It is not a mere question of dropping out the lines when we pass from the temperature of the arc to the temperature of the coil, but it really is a considerable intensification of certain of the lines under certain conditions. There are three conditions under which we get the two lines 5339 and 5340, and they are not seen afterwards. The line 5433 is seen rather faint in the sun and very strong in the Bessemer flame. 5197·5 is very faint in the sun, but its intensity is doubled and even trebled with certain conditions of the quantity coil. I have introduced these facts to point a remark about Kirchhoff's statement; when Kirchhoff made that statement he was amply justified by the science of the time. He was familiar naturally with the spectrum of iron, which he had studied in his own laboratory, and other good observations of the spectrum of iron had been recorded in his time. But, with observations like these before one, which

[1] The map is too large and too detailed to be reproduced here.

one must take into account; it is too *coarse* a statement—I do not use the word in any offensive sense—to say that the iron lines in the sun correspond with the iron lines seen on the earth. Which iron lines—which of these horizons—are to be taken? It will be seen in a moment, if there are differences between these horizons, that if we take any one, we throw all the others out of court, and we have no right to do that; so that statement about the coincidence in the intensity could not be made with the facts now at our disposal. Any one wishing to make that statement would have to go over that work, and he would, following it honestly, I believe, find that the statement was true in no instance whatever. Fig. 40, which is an engraving from a photograph, will show the kind of difference one gets, even when one deals with the electric arc, which undoubtedly gives an iron spectrum which is the nearest approximation to the Fraunhoferic spectrum. The lines at wave-lengths 4325·0, 4300·7, 4271·0 are three of the strongest iron lines in the arc spectrum, and those at 4071·0, 4063·0, 4045·0 are also strong iron lines, though less strong than the others. Now it will be seen that in the solar spectrum the last three are much more important, much thicker, and much darker than the first, so that here is an absolute inversion in the thickness of the lines. I

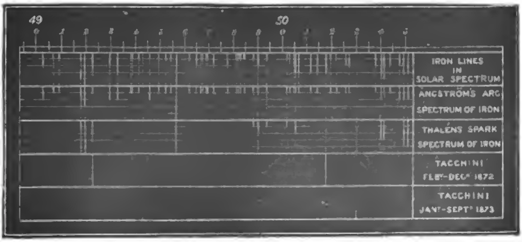


FIG. 39.

appeal to the photograph because there is no partiality about it; it has no view, no anxiety therefore to intensify one particular part of it at the expense of the other. This photograph is referred to only as the exemplar of many similar reversals which we see whenever such observations are made.

Let us now take some iron lines which have been studied in spots and storms, and consider the differences in their intensity among the Fraunhofer lines. We may also note the changes brought about in our laboratories.

The diagram (Fig. 41) gives the main results in a convenient manner. It does not profess to go over the whole ground, but I think it will enable me to point out the way in which the phenomena observed on the sun are re-echoed and endorsed by the work which has been done in the laboratory,

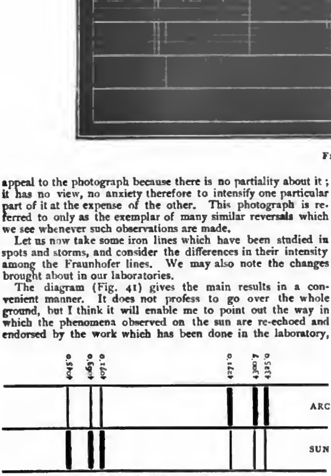


FIG. 40.—Anomalous reversals (iron) from a photograph.

and how severe the tests applied have been, and how well the view has borne the strain.

The diagram refers to three lines visible in the first map—three lines that in an instrument of ordinary dispersion might easily be mistaken for a single line in the sun. We have, as before, the intensities among the Fraunhofer lines recorded in the upper part of the diagram; we then go to our photographs of the arc, and find that the line at 4923·2 is entirely absent. We then pass on to the quantity coil, which gives us the three lines; but there is a difference between the intensities of the lines as seen in the quantity coil with a jar, and the lines seen in the sun, 4918 being thinner than in the sun. If we take the jar out of circuit 4923·2 almost disappears, and we get very nearly the same result as we get from the arc. We then try the intensity coil, which is supposed to give us an equivalent or higher temperature than the quantity coil does. What do we find there? That 4923·2 is enormously expanded and developed, apparently at the expense of 4918, which becomes thin. Taking the jar out, we come back to a result which is very much like the solar spectrum, with the difference, however, that 4918 is somewhat less intense than in the sun. Then come the facts which have already been brought forward throughout with special reference to these particular lines, that the two lines which are seen alone in the arc are seen alone in the spots, or at all events in 73 spots out of 100; and the other line which is so enormously

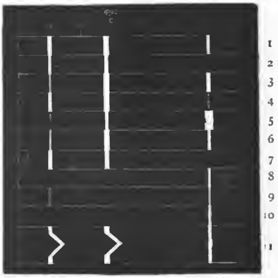


FIG. 41.—Diagram showing the behaviour of three iron lines under different conditions, solar and terrestrial. 1, solar spectrum; 2, arc; 3, quantity coil with jar; 4, quantity coil without jar; 5, intensity coil with jar; 6, intensity coil without jar; 7, spots observed at Kensington; 8, Prominences observed by Tacchini; 9, prominences observed by Young; 10, reversed in penumbra of spot observed on August 5, 1872, by Young; 11, motion indicated by change of refrangibility.

expanded when we use the highest temperature is seen alone in 52 out of 100 prominences by Tacchini. Again, further connecting this diagram with the last one, we have found in several cases when a change of refrangibility has been observed in the iron lines in the spots visible on the sun that the two lines 4918 and

4919·8 have been affected, while 4923·2 has remained at rest. That will give an idea of the way in which we really do find the laboratory work and the observatory work, each coming to the rescue of the other, each helping us to understand something which, without the other record, would be excessively difficult.

Tests supplied by the Absence of Lines from the Solar Spectrum

It is my conviction that many lines of the different chemical substances are absent from the solar spectrum when that absence cannot be attributed to anything depending upon reduction in the quantity of the substance present. In connection with this point there is an experiment to which attention may now be directed, because it is an attempt to imitate solar conditions somewhat; so that the inquiry is rendered possible as to whether these lines may not owe their absence from the Fraunhofer lines to their being the product of a very low temperature, a temperature which we cannot expect to find in the sun in any regions where the pressure would be sufficient to enable any absorption phenomena to take place. The point of the experiment is this: There are bodies which we can render incandescent at low temperatures. For iron, as we have already seen, we have to use a coil, but such substances as magnesium, sodium, lithium and the like can be volatilised at the temperature of the Bunsen flame, and at that temperature we get a certain spectrum from them. Now a great many different spectra have been recorded by different observers for these bodies, and the question was, could we get any independent method of determining which lines were really due to high and which to low temperatures. Now it is generally conceded that the temperature of the Bunsen flame is lower than the temperature of an induction spark; and we have an arrangement by which we can pass a spark between horizontal platinum poles through a flame in which the substance to be experimented on is volatilised. In this way we can see what change in the spectrum is introduced by the passage from the temperature of the flame to the temperature of the spark. We can fill the flame with the vapour, say of sodium, and observe its spectrum; then when the flame is nicely charged in the region between the two poles, we can pass a spark through it, and by throwing the image of the spark upon the slit of the spectroscope we can first of all get a spectrum of the flame, and then the spectrum also of that particular part of the flame through which the spark is passing. Now we really have got a good deal of light from that method of observation. In the case of magnesium, for instance, the change is very striking (see Fig. 42).

The flame gives us a spectrum in which are seen two lines corresponding with the two least refrangible members (b_1 and b_2) of a very prominent group of lines in the green part of the solar spectrum, and associated with these is a less refrangible line unrepresented in the sun, the whole forming a wide triplet. On passing the spark this last line is very greatly enfeebled, if not abolished altogether, for the very obvious reason that the molecule which gives rise to it is dissociated more rapidly at the temperature of the spark than it is at the temperature of the flame, and as that line dies out another solar line (b_4) appears, the three forming a triplet similar to, but narrower than, that

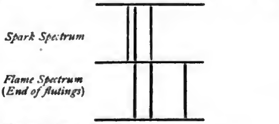


FIG. 42.—Flame and spark spectra of magnesium.

produced in the flame alone. Kirchhoff showed that potassium was not present in the sun, the line upon which he worked being the red line which is seen when potassium is thrown into a flame. The fact that we get that red line in the flame shows that it is a line produced by a low temperature; the molecule which produces the vibration therefore may probably be one which is produced at a low temperature. But when we pass a spark through a vapour giving us that red line we do not increase, but rather reduce, the intensity of the line, and we bring a great many lines into prominence which were not seen before, and those lines, I believe we are justified in saying, do exist among the Fraunhofer lines. In the same way we can colour the flame red with lithium, but the red line of lithium is not in the sun; but by passing a spark through lithium vapour we can intensify the line in the yellow and the line in the blue; and the line in the blue is undoubtedly among the Fraunhofer lines. Therefore it appears that we really can account for a great many of these variations in the solar spectrum by simply assuming that those lines which are absent represent molecular groupings so complex that there is no part of the sun where their absorption could be visibly produced, cold enough to allow them to exist.

Test supplied by the Lines strengthened in Spots and Flames and those seen in the Spectra of Two or more Substances

It has already been pointed out that these lines, which have been called basic lines, have been tested in two ways. In the first place, a list of lines had been prepared from Ångström's tables and Thalèn's tables, and then they had been discussed in connection with the bright lines seen by Young in his observations on Mount Sherman. The result was striking, inasmuch as of the 345 lines which were included by Young, only a small number of which were seen in spots and storms, 15 of the lines which were recorded as common to two substances by Thalèn, had been seen almost without exception, the only exception being in the case of some of the spots. The attack was then varied by taking 100 observations of sun-spots at Kensington, determining, without any reference to the basic nature of the lines at all, the 12 most widened lines in each spot which it was possible to observe; and then taking, side by side with these observations of the spots, 100 observations of flames from the rich store which has been recorded by Prof. Tacchini of Palermo. Then again, without reference to the basic character of the lines, to plot the lines down in each flame day by day.

As a reminder we may again refer to the diagram already given (Fig. 36). It will be remembered that the result was a very remarkable one. We found the lines of iron (we limited ourselves to iron) seen in the spots were few in number; that the lines of iron seen in the flames were still fewer in number, and moreover that the lines seen in the flames were not the lines seen in the spots. That was a result which might have been considered as very extraordinary if we had brought to it no other considerations than those with which we were conversant ten years ago when the work began.

What we have to do now, then, is to find what has been the result of this inquiry with regard to the basic nature of these lines. Have we, as a matter of fact, or have we not, in these most widened lines in spots, and the most brightened lines in flames, picked out those lines which are common to two substances. The facts are these:—We have, in the first horizon of the lower part of the accompanying map, the lines recorded by Ångström in his first memoir as common to two substances; the names of the two substances being given below. In the fourth horizon we have the observations of Thalèn made a few years after the observations of Ångström. And in passing from Ångström to Thalèn we pass from the temperature of the arc to the temperature of the induction coil. Now it will be seen that Thalèn also gives us lines in some cases agreeing with Ångström's, in other cases extending the information given by him, and in order to make this work as complete as possible we have gone over this region with the arc as Ångström did, and with the induction coil as Thalèn did, only we have had the advantage probably of using a more powerful coil. In fact we have used two coils—one so arranged as to give us the maximum effect of tension, and the other the maximum effect of quantity. In the first place it will be seen there is a general agreement between the observations—an agreement marred only in appearance here and there by the fact that in some cases the lines are so near the position of air lines that it has been impossible to make the observation absolutely complete. In other cases the appearance of imperfection arises from the fact that lines which are not seen at the temperature of the arc begin to make their appearance at the temperature of the coil; so that in a case like *that* at wave-length 5017, for instance, where Ångström gives no line as common to two substances, yet Thalèn does. We find that both are right; that at the temperature of the electric arc that line does not appear in one substance or the other, while at the temperature both of the quantity and the intensity induction coil the line is certainly there. In Fig. 36 A represents Ångström's work, T Thalèn's, and L Q and L I my own work with the quantity and intensity coil.

What then is the total result? It is this—that every important line in the spots, every important line in the storms, has been picked up by this method, and in fact *the map of basic lines along this region is practically a map of the lines widened in spots and present in storms, and nothing else.* Now it may be said that result is interesting, and perhaps important, but that it deals only with a very limited part of the inquiry. That is perfectly true.

The spectrum of iron, and the spectra of other substances have however been attacked in other regions. It is unnecessary to go into many details, but the general result is the same; in other regions we have as in the old region an almost perfect coincidence between the lines most widened in spots, and the lines regarded as basic by previous observers.

So much then for the result in the case of iron, to which, although we have not absolutely limited our attention, we have to a very large extent confined it. This result may be expressed in rather a different way, and it will then be easy to see the extraordinary parallelism which goes on between two perfectly distinct sets of facts, first, the statement of the spectroscope that such and such a line is seen in the spectra of two or more substances, and then the statement of the telescope with the attached spectroscope that such and such a line is seen widened in spots or brightened in flames. Here we have the numbers for the two regions which I have already discussed the region from F to *b*, and from *b* towards D.

Iron

	F–*b*	*b*–548
Total number of lines	96	67
Number in spots and flames	38	41
Basic lines	15	17
,, seen in ,, ,,	14	15
,, not seen ,, ,,	1	2

The total number of iron lines in the first case is 96. Of those 96 lines only 38, or less than half, are found in the spots and flames. When we go into the lower regions of the solar atmosphere, we leave in fact more than half of the iron lines on one side. Of these 96 lines 15 are found by other observers as well as myself to be common to two or more substances. Now comes the question, what is the behaviour of these common lines with reference to spots and storms? The table shows that among the lines seen in spots and storms fourteen of these basic lines are seen. It must be remembered that our records only give us day by day the results of the 12 most widened lines, and not of all the lines widened. In the next region the number of iron lines is somewhat less—67; 41 of these, or more than half, are picked out by spots and storms; Seventeen are basic; of the 17 basic ones 15 are seen in the spots and storms, and only two are lines that are not seen.

Now, we will turn to another substance, nickel, and there we see very much the same kind of thing at work. In nickel for the region F to *b* we have 20 lines recorded by Thalén.

Nickel.

	F–*b*
Total number of lines	20
Nnmber in spots and flames	3
Basic	5
,, Seen	3
,, Not seen	2

Of these 20, 17 are dropped, abolished, when we come to observe the bright lines and the widened lines of nickel in the spots and storms—the 20 comes down to 3. Among the 20 lines 5 are found to be common to two substances. Of those 3 are seen in the flames, that is to say, every line of nickel seen in a spot or flame is common to two substances, and only 2 are visible in the 20 lines not affected by spots and storms. This is all the work of this nature which we need now consider, but it is not all the work that has been done. Neither my assistants nor myself, I am sure, have spared our attempts, nor ourselves, for the matter of that, in trying to get at the bottom of this matter, and the facts which have been here brought forward are typical of a much larger number of facts which have been observed. In the case of every part of the spectrum, in the case of every substance, the verdict is the same. We have the fact, that two things are going on exactly parallel to each other—first that some lines are common to two substances; next that the lines common to two substances are seen almost exclusively alone, both in the sun's spots and in the sun's flames. So that in addition to the fact that the hottest regions of the sun seem to simplify the spectra of the substances enormously, we have this result that the simpler the spectrum becomes, the more complex becomes the origin of the lines; by which I mean that in the ordinary solar spectrum there are a great many lines due to iron, and to nothing else; but the moment we come to the simpler spectrum yielded to us in the spots or the flames, then we have no more right to say that those lines belong to iron than that they belong to titanium, cerium, nickel, and other substances with which those lines are generally observed to be basic.

This, then, is a help towards the demonstration of the view which was first announced in the year 1874, that the line-spectra of bodies (we are dealing almost exclusively with line-spectra) are not produced by the vibration of similar molecules, but they probably represent to us the vibrations of a great number of simplifications brought about by the temperature employed to produce the incandescence of the vapours.

Can we go further than this? Here we must confess both our imperfect instrumental and mental means. We cannot talk of *absolute* coincidence because the next application of greater instrumental appliances may show a want of coincidence. On the other hand there may be reasons abont which we know at present absolutely nothing which should make absolute coincidence impossible under the circumstances stated. The lines of the finer constituents of matter may be liable to the same process of shifting as that at work in compound bodies when the associated molecules are changed; but however this may be the fact remains, whatever the explanation may be, that the lines of the elementary bodies mass themselves in those parts of the spectrum occupied by the prominent lines in solar spots and storms.

J. NORMAN LOCKYER

(*To be continued.*)

STATE MEDICINE [1]

FIRST: a few words on what may be called the general theory of our subject-matter. The term "State Medicine" corresponds to the supposition that, in certain cases, the Body-Politic will concern itself with the health-interests of the people—will act, or command, or deliberate, or inquire, with a view to the cure or the prevention of disease. Before any such supposition can be effectively realised, the science of medicine—that is to say, the exact knowledge of means by which disease may be prevented or cured—must have reached a certain stage of development; and unless the science be supposed common to all persons in the State, the existence of State medicine supposes a special class of persons whom the unskilled general public can identify as presumably possessing the required knowledge. Thus, given the class of experts to supply the required exact knowledge, the Body-Politic undertakes that, within the limits of its own constitutional analogies, it will make the knowledge useful to the community.

I have intimated that in State Medicine (just as in private medicine) the medical function may be exercised either in curing or in preventing disease; but practically these two departments of State Medicine are not of equal magnitude, nor are dealt with in quite the same spirit.

As regards curative medicine, modern Governments have in general found it needless to interfere in much detail in favour of persons who require medical treatment.

Larger and far more various than the action taken by the State with reference to the cure of disease is that which it takes in regard of prevention; and it is particularly of preventive medicine that I propose to speak. In its legal aspect it is represented by a considerable mass of statutes (nearly all of them enacted within the last thirty-five years), and by an army of administrative anthorities and officers appointed to give effect to those enactments. I need not describe in detail the laws and administrative machinery to which I refer, but I may remind you of the largeness and variety of the scope, even by quoting only the terms in which I was able, twelve years ago, to speak of the public health law of England: "It would, I think, be difficult to over-estimate, in one most important point of view, the progress which, during the last few years, has been made in sanitary legislation. The principles now affirmed in our statute-book are such as, if carried into full effect, would soon reduce to quite an insignificant amount our present very large proportions

[1] An Address delivered at the opening of the Section of Public Medicine, in the International Medical Association, by John Simon, C.B., F.R.S., D.C.L., LL.D.

of preventible disease. It is the almost completely expressed intention of our law that all such states of property and all such modes of personal action or inaction as may be of danger to the public health should be brought within scope of summary procedure and prevention. Large powers have been given to local authorities, and obligation expressly imposed on them, as regards their respective districts, to suppress all kinds of nuisance, and to provide all such works and establishments as the public health primarily requires; while auxiliary powers have been given, for more or less optional exercise, in matters deemed of less than primary importance to health; as for baths and wash-houses, common lodging-houses, labourers' lodging-houses, recreation-grounds, disinfection-places, hospitals, dead-houses, burial-grounds, &c. And in the interests of health the State has not only, as above, limited the freedom of persons and property in certain common respects, it has also intervened in many special relations. It has interfered between parent and child, not only in imposing limitation on industrial uses of children, but also to the extent of requiring that children shall not be left unvaccinated. It has interfered between employer and employed, to the extent of insisting, in the interest of the latter, that certain sanitary claims shall be fulfilled in all places of industrial occupation. It has interfered between vendor and purchaser; has put restrictions on the sale and purchase of poisons; has prohibited in certain cases certain commercial supplies of water; and has made it a public offence to sell adulterated food or drink or medicine, or to offer for sale any meat unfit for human food. Its care for the treatment of disease has not been unconditionally limited to treating at the public expense such sickness as may accompany destitution; it has provided that, in any sort of epidemic emergency, organised medical assistance, not peculiarly for paupers, may be required of local authorities; and, in the same spirit, it requires that vaccination at the public cost shall be given gratuitously to every claimant. The above survey might easily be extended by referring to statutes which are only of partial, or indirect, or subordinate interest to human health; but, such as it is, it shows beyond question that the Legislature regards the health of the people as an interest not less national than personal, and has intended to guard it with all practicable securities against trespasses, casualties, neglects, and frauds."[1] At the time when that description was written I unfortunately had to confess that the intentions of the Legislature were not carried into effect; for that the then existing laws (especially in respect of the local authorities which should give effect to them) were in a state of almost chaotic confusion and unworkability; but since that time an entirely new constitution of local authorities has been made, some thousands of additional officers have been appointed, and the general fabric of the law has been consolidated, and its powers in some respects extended and made more stringent, with a view to the better prevention of disease, so far as legal powers and facilities can attain that object.

Such being the very large contribution which the Body-Politic makes to the purposes of State Medicine in this country, let us next see how we of the medical profession stand in respect of the scientific contribution which we distinctively owe to the same great object.

In preventive, just as in curative, medicine, it occasionally happens that consequences more or less valuable result from some mere chance-hit of discovery; but except so far as this may sometimes (and but very rarely) happen, disease can only be prevented by those who have knowledge of its *causes*—knowledge which does not deserve to be called knowledge, unless in proportion as it is *conclusive and exact;* and thoroughly to investigate the causes and their mode of operation is the quite indispensable first step towards any scientific study of prevention. Essentially we know how to prevent, by having first learnt exactly how to cause. Therefore it is that preventive medicine has had almost no development until within these later times. The germinal thought of it may be traced in even the first days of our profession. The spirit in regard of which Hippocrates has been aptly called the Father of Medicine—the scientific spirit of observation and experiment, as distinguished from the spirit of priestcraft, was one which his medical writings equally showed in their preventive as in their curative relations; and when he, some twenty-three centuries ago, expounded to his contemporaries that pathology is a branch of the science of nature—that causes of disease are to be found in physical accidents of air and earth and water, and in quantities and qualities of food, and in personal habits of life, he (not without risk of being denounced for impiety) virtually proclaimed for all time the first principle of preventive medicine, and indicated to his followers a new line of departure for those who would most largely benefit mankind. His followers, however, have had their work to do. True knowledge of morbific causes could only come by very slow degrees, and as part of the development with which the physical and biological sciences have, little by little, with the labour of ages, been building themselves up; and so no wonder that, despite the lapse of time, even the most advanced of nations are hitherto but beginning to take true measure of the help which preventive medicine can render them.

Now what is the nature of that *study of causes* through which we may gradually arrive at counter-causing or prevention?

Addressing a skilled audience, I shall utter what to them is the merest commonplace when I say that, in the physical and biological sciences we acknowledge no other study of causes than that which consists in *experiment.* And the study of morbific causes is no exception to that rule: it is solely by means of experiment that we can hope so to learn the causes of disease as to become possessed of resources for preventing disease.

The experiments which give us our teaching with regard to the causes of disease are of two sorts: on the one hand we have the carefully pre-arranged and comparatively few experiments which are done by us in our pathological laboratories, and for the most part on other animals than man; on the other hand, we have the experiments which accident does for us, and, above all, the incalculably large amount of crude experiment which is popularly done by man on man under our present ordinary conditions of social life, and which gives us its results for our interpretation.

When I say that experiments of those two sorts are the sources from which we learn to know the causes of disease, I of course do not mean that the mental process by which an experiment becomes instructive to us is the same in regard of the two sorts of experiment. On the contrary, the ætiological problem (so long as it is a problem) is approached in the two cases from two opposite points of view; and the dynamical continuity of relation, which we call cause and effect, is traced, in the one case, from the one pole, and in the other case, from the other pole of the relation. In the one case, starting with knowledge of our own deliberately-prepared *cause*, our question is, What will be its effect? In the other case, starting from a certain *effect* presented to us, our question is, What has been its cause? But in the second case, just as in the first, when the question is answered, when the problem is solved, when the relation of cause-and-effect has been made clear, we recognise that the conjuring-power which has brought us our new knowledge is the power of a *performed experiment.*

Let me illustrate my argument by showing you the two processes at work in identical provinces of subject-matter.—What are the classical experiments to which we habitually refer when we think of guarding against the dangers of Asiatic cholera? On the one side there are the well-known *scientific* infection-experiments of Prof. Thiersch, and others following him, performed on a certain number of mice. On the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848-9 and 1853-4, were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies which supplied those districts with water. Both the professor and the companies gave us valuable experimental teaching as to the manner in which cholera is spread. I need not state at length the facts of those experiments, probably known to all here, but may rather justify my parallel by referring to an ætiological question which will presently be discussed in our section.

It concerns the *causation of tubercle*—the most fatal by far of all the diseases to which the population of this country is subject. On that subject, for the last sixteen years, we have had a new era of knowledge. It was the great merit of a Frenchman, M. Villemin, that he, in 1865, first made us fully aware that tubercle is an infectious disease. He did this by certain *laboratory experiments* performed on other animals than man. He found that general and fatal tubercular infection of the animal was produced when he inoculated it subcutaneously with a little crude tubercular matter from the human subject. That first laboratory investigation of the subject has been followed most extensively by others; and the further experiments, while

[1] "Eleventh Report of the Medical Officer of the Privy Council," 1869, pp. 20, 21.

entirely confirming M. Villemin's discovery, have shown that subcutaneous inoculation is not the only mode by which the tubercular infection can be propagated. Dr. Tappeiner and others have shown that the same effect is produced on the animal if tubercular matter (such as the sputa of phthisical patients) be diffused in spray in the air which the animal breathes; and Prof. Gerlach of Hanover showed twelve years ago with regard to the bovine variety of tubercular disease (the perlsucht of the Germans), that its infection can be freely introduced through the stomach if bits of tubercular organs be given in the food, or if the healthy animal be fed with milk from the animal which has tubercle. That the communicability of tubercle from animal to animal is also being tested to an immense extent by *popular experiments* on the human subject, is what a moment's reflection will tell; and from that wide field of experiment I select one instance for illustration. I have every reason to believe that Prof. Gerlach's experiments on the communicability of tubercle by means of milk are very extensively parodied by commercial experiments on the human subject. I learn, on what I believe to be the highest authority in this country, that tubercle (in different degrees) is a malady which abounds among our cows; and that so long as the cow continues to give milk, no particular scruple seems to prevent a distribution of that milk for popular use. To the persons who consume that milk an important question as to the causability of tubercle is put in an experimental form. Whether they will become infected with tubercle is a question which the individual consumers do not stand forward to answer for themselves, like the animals of the laboratory experiments: but Dr. Creighton's lately-published book, entitled, "Bovine Tuberculosis in Man," and a paper in which I am glad to say he brings under notice of our section the very remarkable series of facts on which he grounds that startling title, seem to suggest a first instalment of answer in accordance with Prof. Gerlach's experimental finding.

The two sorts of experiment—the scientific and the popular—differ, as I have noted, in this particular: that the popular experiment is almost always done on man; the scientific almost always on some other animal. It is true that many memorable cases are on record, where members of our profession have deliberately given up their own persons to be experimented on by themselves or others for the better settlement of some question as to a process of disease; have deliberately tried, for instance, whether, in this way or in that, they could infect themselves with the poison of plague or of cholera; and as regards one such case which is in my mind, I think it not unlikely that the illustrious life of John Hunter was shortened by the experiments which he did on himself with the ignoble poison of syphilis. There have been cases, too, where criminals have been allowed to purchase exemption from capital or other punishment at the cost of allowing some painful or dangerous experiment to be performed on themselves. And cases are not absolutely unknown where unconsenting human beings have been subjected to that sort of experiment. But waiving such exceptions, the rule is, as I have said, that scientific experiments relating to causes of disease are performed on some animal which common opinion estimates as of lower importance than man. Now, as between man and brute, I would not wish to draw any distinction which persons outside this room might find invidious; but, assuming for the moment that man and brute are of exactly equal value, I would submit that, when the life of either man or brute is to be made merely instrumental to the establishment of a scientific truth, the use of the life should be economical. Let me, in that point of view, invite you to compare, or rather to contrast with one another, those two sorts of experiment from which we have to get our knowledge of the causes of disease. The commercial experiments which illustrated the dangerousness of sewage-polluted water-supplies cost many thousands of human lives; the scientific experiments which with infinitely more exactitude justified a presumption of dangerousness, cost the lives of a few dozen mice. So, again, with experiments as to the causation of tubercle:—judging from the information which I quoted to you, I should suppose that the human beings whose milk-supply on any given day includes milk from tubercular cows might be counted, in this country, in tens of thousands; but the scientific experiments which justify us in declaring such milk-supply to be highly dangerous to those who receive it were conclusive when they amounted to half-a-dozen. So far, then, as regards the mere getting of experimental knowledge, we must not, with a view to economy of life, be referred to popular, rather than scientific, experiment. And in the same point of view, it perhaps also deserves consideration that the popular experiments, though done on so large a scale, very often have in them sources of ambiguity which lessen their usefulness for teaching.

Let me now briefly refer to the fact that, during the last quarter of a century, all practical medicine (curative as well as preventive) has been undergoing a process of transfiguration under the influence of laboratory experiments on living things. The progress which has been made from conditions of vagueness to conditions of exactitude has, in many respects, been greater in these twenty-five years than in the twenty-five centuries which preceded them; and with this increase of insight, due almost entirely to scientific experiment, the practical resources of our art, for present and future good to the world, have had, or will have, commensurate increase. Especially in those parts of pathology which make the foundation of preventive medicine, scientific experiment in these years has been opening larger and larger vistas of hope; and more and more clearly, as year succeeds year, we see that the time in which we are is fuller of practical promise than any of the ages which have preceded it. Of course, I cannot illustrate this at length, but some little attempt at illustration I would fain make.

First, let us glance at our map. When we generalise very broadly the various causes of death (so far as hitherto intelligible to us) we see them as under two great heads, respectively autopathic and exopathic. On the one hand, there is the original and inherited condition under which to every man born there is normally assigned eventual old age and death, so that, sooner or later, he "runs down" like the wound-up watch with its ended chain; and, as morbidities under this type, there are those various original peculiarities of constitution which make certain individual tenures of life shorter than the average, and kill by way of premature old age of the entire body, or (more generally) by quasi-senile failure of particular organs. On the other hand, as a second great mass of death-causing influence, we see the various interferences which come from outside; acts of mechanical violence, for instance, and all the many varieties of external morbific influence which can prevent the individual life from completing its normal course.

As regards cases of the first class—cases where the original conditions of life and development are such as to involve premature death (which in any such case will commonly show itself as a fault in particular lines of hereditary succession)—the problem for preventive medicine to solve is, by what cross-breeding or other treatment we may convert a short-lived and morbid into a long-lived and healthy stock; and this, at least as regards the human race, has, I regret to say, hardly yet become a practical question. But, as regards cases of the second class, evidently the various extrinsic interferences which shorten life have to be avoided or resisted, each according to its kind; and here it is that the scientific experimenters of late years have been giving us almost daily increments of knowledge.

Two early instances, vastly important in themselves, though of a comparatively crude kind, I have already mentioned; and I now wish to glance at some illustrations of the immense scope and the marvellous exactitude of the newer work.

The invaluable studies of M. Pasteur, beginning in the facts of fermentation and putrefaction, and thence extending to the facts of infectious disease in the animal body, where M. Chauveau's demonstration of the particulate nature of certain contagia came to assist them—they, I say, partly in themselves, and partly in respect of kindred labours which they have excited others to undertake, have introduced us to a new world of strange knowledge. We have learnt, as regards those diseases of the animal body which are due to various kinds of external cause, that probably all the most largely fatal of them (impossible yet to say how many) represent but one single kind of cause, and respectively depend on invasions of the animal body by some rapidly self-multiplying form of alien life. This doctrine, which scientific experiment initiated, has, for the last dozen years, been extending and confirming itself by further experiment. As soon as the doctrine began to seem probable, science saw that, should it prove true, it must have the most important corollaries. If the cause of an infecting human disease is a self-multiplying germ from the outside world, the habits of that living enemy of ours can be studied in its outside relations. It becomes an object of common natural history, it has biological affinities and analogies. We can cultivate it in test-tubes in our laboratories, as the gardener would cultivate a rose or an apple, and we can see

what agrees and what disagrees with its life. And then, as the next and immeasurably most important stage, where nothing but experiment on the living body will help us, we can try whether perhaps any of our modifications of its life have made it of weaker power in relation to the living bodies which it invades, or whether, through our more intimate knowledge of its vital affinities, we can artificially give to bodies which it would invade, a partial or complete protection against it. Such, at first blush, were the obvious possibilities of research which the new doctrine of infectious disease suggested to the mind of the pathologist; and never since the profession of medicine has existed, had a field of such promise been before it. The promise has not been belied. A host of diseases has been worked at in such lines as I just now indicated, and with many of them important progress has been made.

It would be impossible for me even to name a twentieth part of the investigations which have been more or less successful. As regards some which have most struck me, I pass with but a word Dr. Klein's investigation of the pneumo-enteritis of swine; Prof. Cohn's and Dr. Koch's and Dr. Buchner's respective contributions to the natural history of the anthrax bacillus; Dr. Bollinger's recognition of the microphytic origin of an important cancroid disease of horned cattle, with Dr. Johne's illustration of the inoculability of this disease; the research by Drs. Klebs and Tommasi-Crudeli into the intimate cause of marsh-malaria; and, not least, the demonstration (as it appears to be) which Dr. Grawitz has recently published, that some of the commonest and most innocent of our domestic microphytes can be changed by artificial culture into agents of deadly infectiveness. I pass these and others, in order that I may more particularly speak of some which have already shown themselves practically useful; for in respect of some of them the time has already come when abstract scientific knowledge is passing into preventive and curative knowledge.

First, and not in a spirit of national partiality, I will mention the application which M. Pasteur's doctrine has received at the hands of Mr. Lister, with regard to the antiseptic treatment of wounds: an application which, enforced and illustrated at every turn by Mr. Lister's own eminent skill as an experimentalist, has been confirmation as well as application of the parent doctrine; and the beneficent uses of which, in giving comparative safety to the most formidable surgical operations, and in immensely facilitating recovery from the most dangerous forms of local injury, are recognised—I think I may say, by the grateful common consent of our profession in all countries, to be among the highest triumphs of preventive medicine.

Secondly, out of the experimental studies of anthrax—chiefly out of those of Dr. Sanderson and Mr. Duguid in this country, and those of Dr. Buchner in Germany and M. Toussaint in France, has grown a knowledge of various ways in which the contagion of that dreadful disease can be so mitigated that an animal inoculated with it, instead of incurring almost certain death, shall have no serious illness; and the further knowledge has been gained that the animal submitted to that artificial procedure is thereby more or less secured against subsequent liability to the disease. In other words, with regard to that disease, an infliction which sometimes spreads to man from his domestic animals, and one which in some parts of Europe is of serious consequence to agricultural interests, as well as to animal life, the later experimenters—of whom I may particularly name M. Toussaint and our countryman, Prof. Greenfield, seem to be giving to the animal kingdom, and to the farmers, the same sort of boon as that which Jenner gave to mankind when he taught men the use of vaccination. Quite recently, our great leader, M. Pasteur, seems to have made, by new experiments, still further progress in the mitigation of anthrax.

Thirdly, a similar discovery has been made by M. Pasteur, with reference to the contagium of a very fatal poultry disease, known by the name of fowl's cholera; he has learnt to mitigate that contagium to a degree, in which, if fowls be inoculated with it, they will suffer no serious ailment; and he has found that fowls so inoculated (or, as he, in honour of Jenner, would say, "fowls so vaccinated") are proof against future attacks of the disease.

Fourthly, Prof. Semmer of Dorpat, through experiments done under his direction by Dr. Krajewski, has made a similar discovery in regard of the infection of septicæmia; has found, namely, that by treatment like that with which M. Toussaint mitigates the contagium of splenic fever, he can bring the most virulent septic contagium into a state in which it shall be mild enough to serve for harmless inoculations; which inoculations, when performed, shall be protective against future infections.

Finally, in a different direction of experimental work, let me name the recent most admirable research which Dr. Schüller of Greifswald has made, nominally in respect of certain surgical affections of joints, but in reality extending to the inmost pathology and therapeutics of all tubercular and scrofulous affections. A knowledge of the fatal infectiveness of crude tubercular matter had been given (as I before said) by Villemin and those who followed him; and Prof. Klebs, four years ago, declared the infective quality to be due to the presence of a microphyte (micrococcus), which he had succeeded in separating from the rest of the matter by successive acts of cultivation in fluids of inorganic origin. Dr. Schüller solidly settles, and widely extends, that teaching. According to his apparently quite unquestionable observations and experiments, the micrococcus which characterises tubercle characterises also certain affections popularly called "scrofulous"—namely, "scrofulous" synovial membrane, "scrofulous" lymph-glands, and lupus: so that these diseases may be defined as essentially tubercular, and that inoculation with matter from any of them, or with a cultivation-fluid in which the micrococcus from any of them has been cultivated, will infect with general tuberculosis. The rapid multiplication of the tubercle-micrococcus in the blood and tissues of any inoculated animal can be verified both by microscopical observation, and by inoculative experiment; and an extremely interesting part of the research, in explanation of certain of our human joint-diseases, is the demonstration that if in the inoculated animal a joint is experimentally injured, that joint at once becomes a place of preferential resort to the micrococcus which is multiplying in the blood, and becomes consequently a special or exclusive seat of characteristic tubercular changes. Even thus far the practical interest of Dr. Schüller's book is such as it would not be easy to overstate, but still greater interest attaches to the last chapter of the book, in which, confidently resting on the pathological facts which I have quoted, he makes proposals for the treatment of tubercle on the basis of its microphytic origin, and shows the successful result of such treatment as he has hitherto tried, from that basis, on animals artificially infected by him.

I venture to say that in the records of human industry it would be impossible to point to work of more promise to the world than these various contributions to the knowledge of disease, and of its cure and prevention; and they are contributions which from the nature of the case have come, and could only have come, from the performance of experiments on living animals.

At this most productive epoch in the growth of medical science, our English studies have been interrupted. An Act of Parliament, passed five years ago under the title of the Cruelty to Animals Act, has made it difficult or impossible for scientific observers any longer to follow in this country any such courses of experiment as those which of late years, at the cost of relatively insignificant quantities of brute suffering, have tended to create an infinity of new resources of relief for the sufferings both of brute and man. The Act does not in express terms interdict all performance of such courses of experiment: it nominally allows them to be done under a variety of limited licences which may be granted by a Principal Secretary of State; but the limitations under which these licences are granted, and the trouble, delay, and friction which necessarily to some extent, and, in fact, often to an intolerable extent, attend the obtaining of any one of them, are practically little better than prohibition.

The Act apparently contemplates, as the chief subjects of its operation, an imaginary class of unqualified persons, who, with no legitimate relation to scientific research, would, under pretence of such research, torture, and (it is supposed) take pleasure in torturing, live animals; and against this devilish class of persons the Act is very indulgently framed: for, instead of expressly refusing licence to unqualified persons, and perhaps hinting to such of them as would do wilful cruelty under pretence of study that the lash and the treadmill are for such scoundrels—instead of this, I say, the Act virtually confounds together that imaginary class of unqualified and cruel persons, and, on the other hand, our professional class of *bonâ fide* scientific investigators, on whom the progress of medicine depends, and whose names are sufficient security for their conduct. What is counted good for the one class is also counted good for the other. The law will trust no licensed experimenter farther than

it can provide for his being minutely watched and regulated by the Secretary of State: and in respect of the details of experimental procedure, the supervision of that high political officer is substituted for the discretion and conscience of the scientific investigator.

Consider for a moment what this means in regard of the members of our profession whom it affects. Contrast with it the almost unbounded trust with which the world, from time immemorial, has regarded the character of our profession. Consider the relation of inmost confidence in which members of our profession in every corner of the kingdom are admitted to share in the sanctities and tendernesses of domestic life. Consider our immense daily responsibilities of human life and death. Consider that there is not a member of our profession to whom the law does not allow discretion that, in certain difficulties of childbirth, he shall judge whether he will kill the child to save the mother. And in contrast with all this, is it to be seriously maintained that society cannot trust us with dogs and cats? that our foremost workers—(for it is essentially they who are affected)—cannot be trusted to behave honestly towards their brute fellow-creatures, unless they be regulated and inspected under a special law in much the same prevenient spirit as if they were prostitutes under the Contagious Diseases Act?

I have reason to believe that, if that Act continues on the statute-book, one of two results will follow. Experiments, indispensably necessary for the growth of medical science in relation to the cure and prevention of disease, will cease, or almost cease, to be done in this country; or, as the alternative to this, persons who desire to advance the science of their profession, will be tempted to clandestinely ignore the law and to run their chance, if the worst comes to the worst, of having to try conclusions with the common informer.

Let me illustrate this by two personal references: I have already mentioned Prof. Lister as an experimenter, whose name is now classical wherever science has reached, and whose work has been of signal advantage to mankind. Last autumn Mr. Lister wished to do some experiments in extension of the particular branch of knowledge with which his name is identified, and at a point which he considered of extreme importance in surgical pathology. He found he must either abandon his investigation or must conduct it in a foreign country, and in his zeal for science he chose the latter course. His experiments (which had to be on large animals) were done at the Veterinary College of Toulouse; and in stating this fact in a letter, from which I quote, Mr. Lister added that "even with reference to small animals, the working of the Act is so vexatious as to be practically prohibitory of experiments by a private practitioner like myself, unless he chooses to incur the risk of transgressing the law." A second name which I have mentioned is that of Prof. Greenfield, who has so highly distinguished himself in developing, by means of experiments, the preventive medicine of splenic fever. Dr. Greenfield, in order to perform his inoculation-experiments, had of course to become a licence-holder under the Act; and his experience of the hindrances which attach to that position is expressed to me in the following terms: "It is my deliberate conviction, as a result of my experience, that these hindrances and obstacles are so numerous and so great as to constitute a most serious bar to the investigation of disease, and even of such remedial measures as would by common consent be for the direct benefit of the animals experimented upon. When to this is added all the annoyance and opprobrium which are the lot of investigators, it is to be wondered at that any one should submit to be licensed." Dr. Greenfield's experimental operations consisted only in inoculating the virus of animal diseases, and he says: "I have not been engaged in other investigations for the simple reason that, with the present restrictions and the difficulty in obtaining a licence, I regard it as almost hopeless to attempt any useful work of the kind in this country."

As I feel sure that the Act must at no distant time be reconsidered by the Legislature, and as I also very strongly feel that, quite apart from any question of legal enactments, there is the question of moral right or wrong to be considered in the matter, I would beg you to allow me to make my own public confession of faith (from which I dare say yours will not much differ) in that extremely important matter of controversy.

The question being whether medical science can rightly use living animals as subjects for experiments which may be painful, and even, in exceptional cases, very painful to them, the answer may be sought in either of two directions: 1. What says the voice of the experimenter's own conscience? and 2. What says the standard of common contemporary conduct in analogous cases?

As regards the first, if I may take the liberty of expressing my own feeling, I would say this. I do not in any degree regard it as matter of indifference that, in certain cases, by my own hand or by that of some one acting for me, I must inflict death or pain on any living thing. I, on the contrary, think of it with true compunction; but I think of it as good or bad according to the end which it subserves. Where I see my way to acquire, at that painful cost, the kind of exact knowledge which, either in itself or in contribution to our common stock, will promote the cure or prevention of disease in the race to which the animal belongs, or in the animal kingdom generally, or (above all) in the race of man, I no more flinch from what then seems to me a professional duty, though a painful one, than I would, in the days before chloroform, have shrunk from the cries of a child whom I had to cut for stone. If, in a case of the latter sort, the surgeon nerves himself to his work by the conviction of an indispensable usefulness in what he has to do, so does the pathologist in his, and surely in a much larger sense. The agitated parent of the child might sometimes be tempted to say: "Forbear giving this cruel pain; let the poor little sufferer die"; but the surgeon's reply would have comforted her. And so with the physiological experimenter: except that he, instead of looking at one individual life to be saved, is looking at a race or at many races, and reflects how, in respect of some grievous physical misery, the whole of them, in all their multitudinous successions, may be redeemed through the suffering of the few. This is my personal view of the abstract right or wrong in the question. I state it because, in matters of right and wrong, no man ought to shelter himself behind authority. I believe I may add that if it, or something very like it, had not for centuries been the general view of the medical profession, our professional knowledge would probably be standing in this present age about where it stood in the days of the Plantagenets. Of Harvey and Hunter and Bele, we well know that such was the view on which they acted in rendering their immortal services to mankind; and I am not aware that any man, whose opinion really counts in matters of medical science, would express any material dissent from it.

The second standard to which I referred was that of the common conduct of men in *analogous* cases. I pointedly say "analogous," rather than "similar," because common life does not in fact give cases which, properly speaking, are "similar" to ours. But what, I ask, is the common *principle* of behaviour of civilised man towards the so-called lower animals? He in every respect subordinates their lives to his own. If he thinks he can get an advantage to himself by killing or painfully mutilating an animal, he does so with apparently no hesitation. See, for one instance, the sexual mutilations which are inflicted on all but a small minority of most kinds of domestic animals, and, as regards some kinds, on many of the females. When I appeared as a witness six years ago before the Royal Commission which was considering the question of our experiments, I particularly endeavoured to draw their attention to this view of the case; and in one of my answers (No. 1491) I entered on it more fully than would be suitable to the present occasion.

Thus, either way, whether I look to what I may call the general conscience of the medical profession, or look to the principles by which men in general govern their conduct towards the brute creation, nowhere do I see fair ground on which exception from outside can be taken to a limited, a strictly economical, use of animal life for purposes of scientific experiment.

No doubt there can be found, outside the medical profession, excellent persons, and plenty of them, whose first inclination would be to dissent from that position of ours; and some such persons have (as I think, hastily) given public utterance to such first impressions, and done their best to promote legislation against us. Among names which I see identified with opinions different from ours, are some for which I have deep respect. Particularly of one such man, whom I have the honour to know, I think it may be truly said that his own whole life has been one of practical beneficence, and I would not willingly incur the censure of any such man. But even to him I would fearlessly say, that I think he has not done justice to the case of our profession. To him, and such as him, I would confidently appeal to reconsider their first impressions. On him, and such as him, I would urge that the practice of scientific experimentation on

living animals is but an infinitesimally small application of the licence which common life claims for itself in regard of animals; and I would challenge such men to examine, with strict impartiality, what are their own responsibilities, direct and indirect, in regard of the infliction of pain on living animals.

I protest against any man's applying to this extremely important question a purely arbitrary standard of right or wrong. Those who pronounce judgment on their neighbours must be prepared to state the principle on which they judge. "Compound for sins you are inclined to, by damning those you have no mind to," is the Pharisee's easy-going formula. Where would life be if that were generally accepted? Suppose a *genus* of action; let men draw an arbitrary line across it—a line prescribed by no better rule than that which governed the lady's dislike to Dr. Fell; let them affix a nickname of praise to all on one side of the line, and a nickname of dispraise to all on the other: truly we should thus have the readiest of royal roads to unlimited mutual persecution.

And I protest against a standard of right and wrong being fixed for us on grounds which are merely sentimental. In certain circles of society, at the present time, æsthetics count for all in all; and an emotion against what they are pleased to call "vivisection" answers their purpose of the moment as well as any other little emotion. With such sections of society, our profession cannot seriously argue. Our own verb of life is *ἐργάζεσθαι*, not *αἰσθάνεσθαι*. We have to think of usefulness to man. And to us, according to our standard of right and wrong, perhaps those lackadaisical æsthetics may seem but a feeble form of sensuality.

Of the mere screamers and agitation-mongers who, happy in their hysterics or their hire, go about day by day calumniating our profession and trying to stir up against it the prejudices and passions of the ignorant, I have only to express my contempt.

I regret to have had to speak at so much length of the heavy cloud which at present hangs over the study of scientific medicine in England, and which, in my opinion, is likely to be of specially disastrous effect on the progress of preventive medicine. As a very old public servant in that cause, I should indeed grieve to see it brought to a stand-still for want of the scientific nurture which, in truth, is its very basis of life; and, speaking publicly of the danger on this occasion, I have hoped that the occasion may give importance to what I say.

And now, gentlemen, from contemplating that cloud, which happily is but local, and which perhaps may be but temporary, I gladly turn to skies which have no cloud. If there exist in the social organism any function whatsoever for which development and eventual triumph may be foretold, surely it is that of State Medicine. Of the two great factors concerned in it—the two strong powers which within our own time have converged to make it the reality which it is—the growth of science on the one hand, and the growing stress of common humanity on the other, neither one is likely to fail. Of our science it is needless to say that it will grow. To the science of nature indeed is allotted that one incomparable human day which knows no sunset. In the pure light of its ever-present daybreak, individual workers will pass away, generations will change, but the studies of Nature, and, above all, the gathering of such knowledge as can lessen man's physical difficulties and sufferings, will surely grow from age to age, and, as on Proserpina's sacred tree, one golden fruit will follow another: "simili frondescet virga metallo." And no less also in the other direction, the auguries are wholly for our cause. Popular education is gradually making its way, and it will grow to be a force on our side. Masses of mankind that now have to be humbly pleaded for by others, will then be strong to speak for themselves. Physical interests, now but little understood, will then be within grasp of all men's apprehension. Not only will health be recognised at its true value, and its elementary requirements be regarded, but also the frauds and villanies which are now committed against it will have become intelligible to the common mind; and the workman of the future will strike against being cheated in health as he would now strike against being cheated in wages. As such times come to the world, the science and the profession which care for man as man will get to be better appreciated than now. And in proportion as an educated people grows to become Body-Politic, State Medicine will be seen to represent the true ideal of Government-action which sets its standard of success in the "greatest happiness of the greatest number."

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1881.—The observations of this body in both hemispheres from its discovery on May 22 by Mr. Tebbutt at Windsor, N.S.W., to the end of last month, are closely represented by a parabolic orbit. The intensity of light is now rapidly going off, and if any decided deviation from the parabola is established it can only be through the later observations in these latitudes. It is therefore important for the theory of the comet that the larger instruments in our observatories should be brought to bear upon the accurate determinations of position, and that this should be continued as long as practicable. The following ephemeris for Greenwich midnight is calculated from elements, which are likely to give the comet's places pretty closely:—

	Right Ascension. h. m. s.	Declination. ° ′	Log. Distance from Earth.	Log. Distance from Sun.
August 20	14 31 0	+77 19·6	0·1206	0·1501
22	38 10	77 3·0		
24	45 22	76 47·1	0·1376	0·1672
26	52 36	76 31·9		
28	14 59 52	76 17·2	0·1532	0·1837
30	15 7 10	76 3·2		
Sept. 1	14 31	75 49·2	0·1676	0·1995
3	21 54	75 35·7		
5	29 18	75 22·4	0·1810	0·2148
7	36 46	75 9·3		
9	44 16	74 56·3	0·1934	0·2295
11	51 51	74 43·4		
13	15 59 29	74 30·8	0·2050	0·2436
15	16 7 11	74 18·1		
17	14 57	74 5·4	0·2160	0·2572
19	22 47	73 52·6		
21	30 42	73 39·7	0·2264	0·2704
23	16 38 41	+73 26·7		

The intensity of light on September 23 will be only one-third of that on August 20.

Dr. B. A. Gould has published in pamphlet-form an account of the Cordoba observations of this comet, with particular reference to his observations of June 11, to which we referred last week. We give his conclusions respecting the object seen that evening in his own words:—"La latitud considerable presta poca probabilidad á la hipótesis de que esta estrella haya sido un planeta interior. El movimiento relativo en declinacion, y la falta de cualquier objeto visible de la misma clase en la vecindad del cometa el dia siguiente, no parecen admitir la suposicion que el cometa se hubiera dividido como el de Biela. El brillo que se necesitaba, para que fuese visible la estrella en aquel momento y aquella posicion, indica una magnitud no inferior á la tercera.

"Esta observacion tambien tiene que esperar su solucion en lo futuro, y tal vez solo despues de muchos años."

SCHÄBERLE'S COMET.—According to M. Bigourdan's elements, the position of this comet at Berlin midnight on August 23 will be in R.A. 11h. 42·5m., Decl. +40° 34′, and at the same hour on August 25 in R.A. 12h. 16·0m., Decl. +34° 14′, and the intensity of light will be at a maximum between these dates. It may be observable in the other hemisphere for some weeks after perihelion passage.

THE COMPANION OF SIRIUS.—Prof. Colbert of the Dearborn Observatory, Chicago, has calculated the following orbit of the companion to Sirius:—Apastron passage, 1867·0, position of node, 42°·4; node to periastron in the direction of the star's (retrograde) motion, 133°; inclination, 57°·1; eccentricity, 0·58; semi-axis major, 8″·41; period, 49·6 years. These elements give, for 1881·2: angle of position, 45°·6; distance, 9″·9; and for 1882·2, position 43°·1; distance, 9″·5. For 1890·2 the position is 322°·2; distance, 2″·2; and the distance is near its minimum.

SCIENTIFIC SERIALS

The Journal of Anatomy and Physiology, vol. xv. Part IV. July, 1881, contains: On the ovary in incipient cystic disease, by Dr. V. D. Harris and A. Doran (Plate 23).—The anatomy of the Koala (*Phascolarctos cinereus*), by Dr. A. H. Young.—On the lymphatics of the pancreas, by Drs. George and F. Elizabeth Hoggan (Plate 24).—A case of primary cancer of the femur, by R. Maguire (Plate 25).—A case of chronic lobar pneumonia, by

Thomas Harris.—A contribution to the pathological anatomy of primary lateral sclerosis (sclerosis of the pyramidal tracts), by Dr. Dreschfeld (Plate 26).—On the form and proportions of a fœtal Indian elephant, by Prof. Turner (Plate 27).—On the femoral artery in apes, by Dr. J. Macdonald Brown.—The brain and nervous system: a summary and a review, by Robert Garner.—Index to vol. xv.

THE recent numbers of *Trimen's Journal of Botany* (217–223) contain quite the average number of articles of interest, relating both to British and to foreign botany.—H. and J. Groves describe *Chara obtusa*, a species new to Britain.—Dr. Vines summarises the existing literature on the very difficult and intricate subject of the morphology of the Scorpioid Cyme, referring especially to the confusion resulting from the difference in the use between Continental and English writers of the terms "helicoid" and "scorpioid."—Prof. Dickson gives a very interesting account of the morphology of the pitcher of *Cephalotus follicularis*, illustrated by two plates. His conclusions on its structure are thus summarised:—"1. That the pitcher results from a calceolate pouching of the leaf-blade from the upper surface. 2. That the apex of the leaf is on the far side of the pitcher-orifice (from the main axis and from the lid, and is probably represented by the tip of the middle dorsal wing. 3. That the pitcher-lid represents an outgrowth or excrescence from the upper leaf-surface." Mr. C. B. Clarke and Dr. Hance continue their descriptive papers, the former relating chiefly to Indian, the latter to Chinese botany, and including the description of many new species. Among the more important of Mr. Clarke's contributions is a complete review of the order Commelinaceæ, in which are comprised 307 species (including the common *Tradescantia virginica*, or Virginian spider-wort of our gardens), distributed over twenty-six genera. These are placed by Mr. Clarke in three tribes—the Pollicæ (26 species), Commelineæ (144 species), and Tradescantieæ, which includes nineteen out of the twenty-six genera.

THOUGH the recent numbers (41, 42) of the *Scottish Naturalist* contain no article calling for special remark, this quarterly fully maintains the character which it has already acquired under the editorship of Dr. Buchanan White.

THE *American Naturalist*, July, 1881, contains: On the origin and descent of the human brain, by S. V. Clevenger.—On the eastern snow bird, by Samuel Lockwood.—On bacteria as a cause of disease in plants, by T. J. Burrill.—Record of American carcinology for 1880, by J. S. Kingsley.—Aboriginal stone-drilling, by Charles Rau.—On the effects of impacts and strains on the feet of mammalia, by E. D. Cope.

IN the last number of the *Bulletin* of the Torrey Botanical Club which has reached us are two interesting articles. Mr. F. Wolle described several freshwater algæ new to the flora of the United States, several of them being previously undescribed. These are *Synechococcus racemosus*, *Calothrix Hosfordii*, and *C. laculosa*. Prof. C. E. Bessey describes and figures a simple dendrometer for measuring the height of trees.

Annalen der Physik und Chemie, No. 6.—Determination of the specific gravity of distilled mercury at 0°, and the disturbing reactive dilatations of the glass therewith connected, by P. Volkmann.—Researches on sound-strength, by A. Oberbeck.—On the quantity of electricity furnished by an influence-machine of the second kind, and its relation to moisture, by E. Riecke.—On the distribution of electricity on the surface of moved conductors, by H. R. Hertz.—On Herr Exner's experiments with regard to the theory of Volta's fundamental experiments, by V. A. Julius.—The determination of the transference-numbers of the ions for lithium and carbonic acid compounds, by J. Kuschel.—On the galvanic behaviour of carbon, by H. Muraoka.—Remarks on Herr Warburg's paper, on some actions of magnetic coercitive force, by C. Fromme.—The intensity of the horizontal terrestrial magnetic force for Göttingen in 1880, with the secular variation of the same, by K. Schering.—On a new volumenometer, by A. Paalzow.—On the oxygen spectrum, by A. Paalzow and H. W. Vogel.—The photometry of the Fraunhofer lines, by K. Vierordt.—A polarisation-apparatus of platino-cyanide of magnesium, by E. Lommel.—On the law of dispersion, by the same.—Researches on the height of the atmosphere, &c. (continued), by A. Ritter.—On the absolute size of gas-molecules, by E. Dorn.—Remarks on Herr Besselhagen's paper on a new form of the Töpler mercury air-pump, by F. Neesen.

THE *Nuovo Giornale Botanico Italiano* for April contains an article by A. Piccone on the cause of the disease which was so destructive to the chestnut trees in the province of Genoa in the year 1880, and which was previously obscure. He claims to have established that it is due to the attacks of a parasitic fungus, *Septoria castaneæ*, which attacks the branches and leaves, but the reproductive organs of which he has been unable to detect. Its extraordinary development during that year appeared to be due to the rainy character of the summer. Fitzgerald and Bottini's article on the bryology of the valleys of the rivers Secchio and Magra is accompanied by a valuable coloured map showing the nature of the soil in the district bordering the western coast of the peninsula stretching from near the Gulf of Spezzia to Lucca.

Zeitschrift für wissenschaftliche Zoologie, Bd. 35, Heft 4, June 14, 1881, contains: On the minute structure of the stigmata in the insects, by O. Krancher (plates 28 and 29).—A revision of the species of Holothuroids described by Prof. Brandt from Mertens' collection, by Dr. Hubert Ludwig.—Contribution to a knowledge of the Hydrachnid genus Midea of Bruzelius, by F. Könike (plate 30).—A revision of the Hydrachnids described by H. Lebert from the Lake of Geneva, by F. Könike.—Contribution to a knowledge of the Psorospermeæ of fish, by Prof. O. Bütschli (plate 31).—Studies on the Bopyridæ, by Prof. R. Kossmann (plates 32 to 35).

Revue Internationale des Sciences Biologiques, June 15.—Examination of vision from the stand-point of general medicine (concluded), by M. Charpentier.—Insane conceptions, their mechanism and diagnostic character, by M. Spitzka.

Revue Internationale des Sciences biologiques, July, 1881, contains:—Metallotherapy, by Dr. L. H. Petit.—Protoplasm considered as the basis of animal and vegetable life, by Prof. Hanstein.—The multiplication, colonisation, and encystment of the rhizopods, a review by Dr. Bütschli.—On the coloration of living protoplasm by Bismarck Brown, by L. F. Henneguy.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, August 3.—R. Meldola, F.C.S., vice-president, in the chair.—Miss E. A. Ormerod exhibited some *Coleoptera* and *Hemiptera* from Port Elizabeth, South Africa.—Mr. E. A. Fitch exhibited an ear of wheat infested by *Siphonophora granarii*, every specimen of which was attacked by a parasite belonging to the genera *Allotria* or *Aphidius*.—Papers read: Mr. A. H. Swinton, on the oviposition of *Iodis vernaria*.—Prof. Westwood, description of a new genus of Hymenopterous insects (*Dyscolesthes*) from Chili.—Mr. A. G. Butler, descriptions of new genera and species of *Lepidoptera* from Japan.—Mr. R. Trimen, on some new species of *Rhopalocera* from Southern Africa.—Mr. C. O. Waterhouse, descriptions of new Longicorn *Coleoptera* from India.—Mr. W. L. Distant, descriptions of some new neotropical *Pentatomidæ* and *Coreidæ*, and of the female of *Morpho Adonis*, Cram.

CONTENTS

THURSDAY, AUGUST 25, 1881

PAPIN

Dr. Ernst Gerland's Life and Letters of Papin. (*Leibnizens' und Huygens' Briefwechsel mit Papin, nebst der Biographie Papin's*, von Dr. Ernst Gerland. Berlin, 1881.)

AT a time when Britons have just been celebrating the centenary of Stephenson a glance at the life and works of one who in a much more accurate sense was *the inventor* of the steam-engine may not be amiss. Denis Papin, whose life and letters now appear from the editorial pen of Dr. Ernst Gerland, was born at Blois in 1647. In 1661 or 1662 he proceeded to the study of medicine at the University of Angers, and, receiving his degree in 1669, he appears to have intended to settle down to a physician's life in that city. Why this course was not fulfilled is not known; but we find him in 1674 at Paris, where also he had made the acquaintance of Huygens, with whom he was engaged in experimenting with the newly-discovered air-pump, an account of which was published in that year at Paris under the title of "Expériences du Vuide." The *Philosophical Transactions* of our Royal Society for the following year (1675) were enriched by no fewer than five papers on the same subject by Huygens and Papin jointly. In Paris also Papin met Leibniz, who sojourned there from 1672 to 1676. His acquaintance with Leibniz was however interrupted, for, very shortly after the publication of his "Expériences," he crossed the Channel to England, led, as Boyle tells us, by some hope that here he might procure a situation accordant to his genius. In London he assisted Boyle in his laboratory and with his writings, and shortly afterwards introduced into the air-pump the further improvement of making it with double barrels, and replacing the turncock hitherto used by the two valves. A little later he produced another instrument, the condensing pump, and in 1680, on Boyle's nomination, he was admitted to the distinction of Fellowship in the Royal Society. That honour he repaid in the following year by communicating to the Society his famous invention of "A new Digester or Engine for Softening Bones," in which instrument—now so universally known—he applied as it seems for the first time the now common device of a safety-valve. For that year and the next he devoted himself to experiments with the digester and its various applications. Then he received an invitation to proceed to Venice to take part in the work of the Accademia di Scienze Filosofiche e Matematice, then newly founded, in imitation of the learned societies of Rome, Florence, Paris, and London. Here he appears to have remained nearly two years, and early in 1684 we find him back again in London, where, on April 2, he was elected by the Royal Society as their "temporary curator of experiments," at a salary of 30*l.* per annum. This was in the palmiest days of the young Society, when Newton, Boyle, Hooke, Hawkesbee, and many other famous spirits took the most active part in its proceedings, and Papin shared in the work of bringing their experiments, embracing a most miscellaneous range of subjects, before the Society. Amongst the discoveries of his own, of which Dr. Gerland gives a summary, in this way brought before the Royal Society, was the so-called Würtemberg siphon. He also partially anticipated Franklin in his discovery of the ebullition of water under reduced pressure at lowered temperatures, concerning which point he observes: "This shows that liquors being freed from an external pressure will make bubbles upon the score of the elastic particles lurking in their pores, as has been observed long ago by the Hon. Mr. R. Boyle. I do therefore believe that the vapours raised by heat in an exhausted glass will make a pressure, which is quickly taken off when we condense these vapours by putting the glass into cold water or ice." On another occasion Papin brought forward a model of a machine for raising water by pumps to a height, the pumps being worked by a water-wheel driven by the flow of a river. In November, 1687, this occupation came to an end by Papin resigning his post of curator on being appointed by the Landgrave of Hesse to be Professor of Mathematics at Marburg. Here the most active portion of his life began, to be continued when, in 1696, his place of residence was exchanged for Cassel. Here too began the correspondence with Huygens and with Leibniz, which forms the major part of the volume before us. By both of these great men he was highly esteemed. Huygens explained to him in 1690 his new theory of double refraction in a letter of considerable length. Leibniz wrote to Luca about Papin, referring to his ingenious inventions in the most enthusiastic terms. The correspondence with Leibniz went on almost unbroken until Papin's final return to England heart-broken and worn out. That with Huygens ended much earlier, being terminated by Huygens' premature decease in 1695. To Huygens in 1691 Papin writes from Cassel about a project he is undertaking for the Landgrave, to construct a ship on the plan suggested previously by Drebbel to move under water. In the same letter he mentions the production of fog or mist in the receiver of the air-pump, for which phenomenon Huygens, in replying, propounds an explanation. At the same date we find Papin busy with another invention, a *rotilis suctor*, which was nothing else than a centrifugal fan for supplying a blast for furnaces and for ventilating mines, which instrument he had the satisfaction of applying to a mine in Germany in 1699, and six years later he made the further improvements related in the *Philosophical Transactions* of that year. But his greater work was drawing on. In 1698 he writes to Leibniz that he is constructing a machine for raising water to a great height by the force of fire, and that by the success of his experiments he is persuaded that this force can be applied to many other more important ends. Leibniz replied forthwith with the inquiry whether his invention was based upon the principle of rarefaction; adding that he also had some ideas on the point. After two months Papin replies that he relies upon the principle of rarefaction produced by the condensation of steam, but that he proposes also to use the pressure exerted by the steam in expanding, "the power of which is not limited as is that of the suction" (of rarefaction). He also says that he has made a little model of a carriage which is propelled by his force, but that he fears difficulties for such carriages from the inequalities and sharp turns of ordinary high roads—difficulties which for water carriages do not exist. Leibniz's reply is only known

from a summary of it scribbled in his own handwriting upon the back of Papin's letter. He first congratulates Papin on having set himself to this work; expresses his fears lest the direct pressure of expanding steam should produce explosions; and then suggests, "pour faciliter le chariotage," an idea of his own (derived, he says, from the air-pump), that the steam which is to exert pressure should be introduced into a cylinder into which is fitted a second one, after the manner in which our modern gasometers fit into their cylindrical pits, the whole being rendered steam-tight by mercury. Papin, replying in August, announces that his machine has raised water to the height of 70 feet. He only half approves Leibniz's suggestion on account of the probable friction between the internal cylinders. To this Leibniz retorts that while the friction increases with the diameter of the piston of a pump, the pressure increases as the square of the diameter. The matter seems to have dropped at this stage for three years. In 1702 Papin, still at Cassel, announces to Leibniz that he has invented a steam *ballista*, "an invention to facilitate the capture of the strongest places," which "will reduce France to make most promptly a durable peace!" This invention was a cylinder 5 inches in diameter filled with a piston connected to pivoted lever, which on the descent of the piston on the condensing of the steam below it would project a stone weighing 2 lbs. to a distance of 40 feet. (A similar ballista, unknown to Papin, had been suggested by von Guéricke, in 1672, in his *Experimenta Nova.*)

Early in January, 1705, Leibniz sent to Papin a sketch of Savery's engine for raising water. This set Papin with renewed vigour to work, besides stimulating to emulation the breast of his patron, the Landgrave of Cassel. After some consideration he pronounced in another letter to Leibniz that he would surpass Savery's invention. He related how he had thought it best not to let the steam act directly against the surface of the water (as in Savery's machine), but that the pressure of the steam should be imparted to the water through the intermediation of a *piston* whose surface, becoming hot, would not produce condensation: and he added that experiment had proved the conjecture to be sound. His great difficulty now was, not to make pistons fit accurately, but to construct tubes sufficiently strong to bear the pressure of such columns of water as he wished to raise. Leibniz congratulated him when replying in August, and advised him not to try to force water high, but rather to lift it by a series of pumps, each drawing 30 feet, a suggestion which Papin on his part rebutted by observing that one force-pump driving water 500 feet high was more economical than ten pumps, each raising the water 50 feet. He further lets Leibniz know that he hopes to do away with the delay of letting the cylinder of his engine cool between each stroke (the very first of the improvements subsequently made by Watt), and that he has some ideas about the transmission of power to a distance, with which problem however he thinks it useless to concern himself, "because by means of the heat-engine one can produce, everywhere where one will, so much power, and so cheaply, that it would be a superfluous expense to carry it elsewhere." Strange commentary, indeed, on the present eager strife of inventors to supersede steam by the electric transmission of power! On October 19 he writes again that he is almost satisfied with his engine, which, though having but one cylinder and two valves, yet furnishes a continuous jet, surpassing Savery's machine, which had two pressure-vessels and four valves: he is only waiting the Landgrave's orders as to how he shall apply his engine to drive a mill. On the last day of the year 1705 he declares to Leibniz his intention of propelling vessels by steam, as he is persuaded that by this means one could have vessels which would follow their course correctly in spite of tempests and adverse winds. At this idea he laboured diligently for the next two years—in fact, so long as he continued to remain at Cassel—his devotion to the object in hand being remitted only for the sake of his correspondence and for the work of publishing his treatise, the "Ars Nova," in which his high-pressure boiler and its applications are described. It was towards the close of this time that (on February 4, 1707) he communicated to Leibniz the first suggestion of a hot-air engine, afterwards realised by Stirling and Ericssen. He was now preparing to leave Cassel, where the patronage of the Landgrave had grown on the one hand slack, on the other irksome, in order to regain the more congenial atmosphere of London and of the Royal Society. He strained every nerve and spent all his little resources to accomplish the building of the steam-propelled boat by which his return to England was to be made famous. He was certain that by this means two men on board his boat might do more than a hundred rowers could. In July and August of that year he made diligent efforts to obtain permission to descend the river *viâ* Münden and Bremen into the Veser, permission which was finally granted by the Elector of Hanover, in spite of the monopoly possessed by the guild of boatmen of Münden to pass boats from the Fulde into the Veser. With a boatman of Münden as captain, he sailed from Cassel on September 24, 1707, with his family. At Münden however the guild of boatmen asserted their privileges, the magistrates pronounced the boat confiscated, and a handsome offer of ransom was rejected. Papin pushed forward despairingly for England; only to find himself almost unknown and friendless. The old generation was fast passing away. For two or three years he continued his mechanical inventions, and several times applied through Sloane for a grant of money from the Royal Society to aid him in his work, but in vain. Misunderstanding and misery followed apace. The inventions on which he relied for fame and position were passed by unnoticed. In the loss of his ship he had made shipwreck of his life's hopes. He died in London, probably, in the early half of the year 1712, but in such obscurity that neither place nor date is with any certainty known.

Dr. Gerland appears to have spared no pains in collecting the scattered facts of Papin's life and work from which to build the volume whose contents we have endeavoured to make known to English readers. We congratulate him on his success, and trust that his efforts will be further rewarded by the discovery of the facts still required to fill the *lacunæ* in the career of this remarkable man. The light which the publication of the correspondence between Leibniz and Papin throws upon the relation between two prominent figures amongst men of science at that time is by no means the least interesting feature of the work; and we must henceforth place

Leibniz amongst the worthies to whom the credit of improvements in the steam-engine is given. The one common feature that runs through the many different types of steam-engine is the piston working within a cylinder. No engine before Papin's time was adapted for any useful purpose except for raising water, and none had a piston in a cylinder. No engine since Papin's time of the thousand varied types has been devoid of this feature. But the very feature which Papin introduced, and on the introduction of which his claim to be called *the* inventor of the steam-engine has been founded, was, as we now know, the suggestion of another mind. We owe the application of the piston-principle in the steam-engine, not to Papin, but to Leibniz.

CHEMISTRY OF THE FARM

The Chemistry of the Farm. By R. Warington, F.C.S. Pp. xiii. and 128. (London: Bradbury, Agnew, and Co., 1881.)

THE chapters of this little handbook appeared originally in detached portions in the *Agricultural Gazette*. They have been revised, and are now issued in a convenient and compact form. A well-ordered manual of agricultural chemistry, clearly written and perfectly abreast of recent advances in the sciences underlying the farming art, has long been wanted. So far as the limits of its size and scope allow, Mr. Warington's volume fulfils our expectations. It is a satisfaction which is seldom afforded us to read a book on agricultural chemistry written by a true chemist trained in laboratory work and versed in the progress made through English and foreign researches. The applications of chemistry to agriculture are manifold, and cannot be grasped by chemists who do not combine with their chemistry a competent knowledge of vegetable and animal physiology and of mineralogy. Yet to learn or to teach the Chemistry of the Farm without a knowledge even of the foundations of chemical science is commonly attempted, though it can never succeed in any true sense. And we quite agree with Mr. Warington that a wider range of scientific knowledge than this is needed even for the student of agricultural chemistry—much more then for the teacher. To talk about this applied science to persons without previous scientific knowledge, and to look for satisfactory results, is to expect a plant unfortunately destitute of roots to blossom and bear fruit.

We think then that Mr. Warington's handbook is valuable on account not only of the knowledge with which its subject is handled, but also on account of the spirit with which that subject is approached. That a new work on agricultural chemistry was sorely needed does not admit of question. In France and in Germany the educational literature of this subject includes many excellent works which have no English counterparts. Johnston's treatise, full as it is of valuable observations, is too thoroughly out of date in method as well as in matter to admit of satisfactory revision; much the same judgment must be passed on Anderson's "Agricultural Chemistry," now twenty years old and out of print. Even if Georges Ville's work on Manures included (which it is far from doing) anything like the whole domain of the Chemistry of the Farm, it is about as unsafe and misleading a book as could be put into the hands of a student. Mr. Warington has given us, in fact, not all we want, but a good bit of it. He has used, and that judiciously, both German and English text-books, researches, and memoirs, and has put the main facts they enounce into a neat form, so as to be "understanded of the people." The two capital text-books of Emil Wolff have been laid under contribution by Mr. Warington; while the chief results of some of the matchless Rothamsted Memoirs by Lawes and Gilbert have been skilfully introduced into his pages. Of the contents of these it is now perhaps time to say a few words. In five chapters the growth, the food, the nutrition, and the products of farm crops are discussed; in another five, animal growth, food, nutrition, and products. Of the diverse origins and properties of soils but little is said; as to the utilisation of urban sewage, nothing. And we should have been glad to have found fuller accounts of many subjects which are but lightly touched upon in these pages. But the difficulty of treating so vast and complex a subject intelligibly in so few pages makes us surprised, not that some things are omitted from, but that such a large number of things are included in, this little book. Some facts and figures which the author would doubtless have liked to introduce have been kept out of his pages by the absolute necessity of finding room for numbers and arguments of primary importance. For instance, it would have been unwise to have curtailed the space bestowed upon the "Digestibility of Foods" and the "Albuminoid Ratio."

In reading through this handbook carefully we have been unable to discover more than a very few statements which we cannot completely endorse; in fact the majority of such alterations as we would suggest would be of form rather than of substance. We cannot, however, refrain from expressing our regret that the percentages of albuminoids in potatoes and roots as given in the table on p. 72 should be the old erroneous figures condemned on the very next page as greatly in excess of the truth. Mr. Warington has however so thoroughly recognised the importance of the discrimination between albuminoid and non-albuminoid nitrogen that we must attribute the inclusion of the incorrect figures in his tables to the difficulty of constructing a complete series of analyses comparable with one another in this particular of the percentage of true albuminoids.

A. H. CHURCH

OUR BOOK SHELF

A Handbook of the Vertebrate Fauna of Yorkshire. Being a Catalogue of British Mammals, Birds, Reptiles, Amphibians, and Fishes, showing what species are or have within historical periods been found in the County. By Wm. Eagle Clarke and Wm. Denison Roebuck. (London: Reeve and Co., 1881.)

THIS little volume is dedicated to the President-Elect of the British Association, and most seasonably makes its appearance on the eve of the meeting of that Association in the city and county of its origin, when it will celebrate the completion of the first fifty years of its existence. Its object is the enumeration of those animals with a vertebral column which either are or have been found in Yorkshire, and the careful definition of their faunistic position and geographical distribution within the county. It would appear that there has never been a list of the mammals, birds, or fishes of the county of York published, and in this respect it presents a striking contrast with its neighbouring counties of Norfolk, Northumberland, and

Durham; but by the energetic labours of Messrs. Clarke and Roebuck this reproach no longer exists, and this very useful handbook to the vertebrate fauna of the shire will, let us hope, be soon followed by a second volume, dealing with the larger and perhaps more difficult portion of its, to use a handy term, invertebrate animals. The number of British vertebrata not occurring in Yorkshire being comparatively small, it seemed desirable to the compilers to make this work not only a county handbook, but a complete nominal catalogue of the British species. In this we think they have done well, for such a catalogue undoubtedly furnishes a ready means of comparison with the faunas of other districts. The classification and nomenclature has in all cases been based upon the most recent or the most reliable authorities as to the extinct British mammalia. It having been considered advisable to include notices of these, or at least of such of these as had ceased to exist in Yorkshire within historical times, the species are inserted in their correct zoological sequence, but their names are printed in Old English characters, and they are left un-numbered, as not being now entitled to rank as true members of the fauna. The same has been done in the case of the Great Auk among the birds. To the catalogue is prefixed an interesting chapter on the physical aspect of Yorkshire, the largest county of the British Islands, containing an area of 3,936,242 statute acres—one which, while most compact in form, is perhaps the most varied in geological structure, soil, climate, and physical aspect. The introductory remarks also on the mammals, birds, reptiles, amphibians, and fishes are well worth perusal. From the general summary, the richness of the Yorkshire fauna can be at once seen, it including 513 out of the 717 known British vertebrates. We gladly recommend this volume to our readers, as in every way an excellent and scientific handbook to the vertebrate fauna of Yorkshire.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Schaeberle's Comet

THIS comet, C 1881, was well seen here on the night of Sunday last, the 21st instant. At 9.30, the night being clear, it was at once detected with the naked eye at a point in the north-west, where lines drawn downward through α and β of Ursa Major (the pointers) and γ and δ of the same constellation would intersect, and just above ψ of Ursa Major, a star of the 3rd magnitude. Owing to the comet's close proximity to the horizon I could not use the 6" equatorial, but the position must have been very close upon R.A. 11h. and D.N. 47°. The general appearance to the eye was that of a comet with two nuclei, the one in advance of the other. With a 2½-inch binocular the comet was beautifully sharp and well defined, more so, I thought, than the great southern one when in the same position. The nucleus and star appeared of about the same intensity, but the yellow tint of the latter was strongly contrasted with the almost intense gas blue tint of the former. The tail was well defined, only slightly spreading, and nearly straight, stretching in a line a little to the left of β of Ursa Major, nearly as far as a small triangular group of stars just under β, marked in Maltby's atlas as 44·37 and 246·7. This would give a length of from seven to eight degrees. The tail did not, with the small instrumental power I was using, appear to have any central deficiency of light. The sharpness and brightness of the comet's appearance, as contrasted with the more diffused aspect of the one which has just disappeared, has been remarked upon by several observers.

Guildown, August 22 J. RAND CAPRON

The Descent of Birds

THERE is one passage in the report of Prof. Mivart's lecture on chamæleons (NATURE, vol. xxiv. p. 338) that I cannot allow to pass without demurring to, and that is the suggested probability of a "double origin" for the class Aves. I do not wish at present to raise the issue as to how far the division of all living birds into two groups—"Ratitæ" and "Carinatæ"—is, or is not, a natural one; for at present we have not, I think, sufficient information or evidence on the subject to allow of any very definite reply. But any one who is acquainted with the structure of a Tinamu will, I think, be unable to conceive of the many resemblances that group of birds presents to some of the "Ratitæ" as having been developed independently of any genetic connection between the two—and that is what Prof. Mivart's suggestion practically amounts to. That structures so peculiar as feathers—which, as far as we know, are absolutely confined to birds, though universal amongst them—should have been twice over developed, is to me in the highest degree improbable—as improbable, almost, as that the resemblances of the Tunicates and *Amphioxus* to the rest of the Chordata should also be accidental. W. A. FORBES

West Wickham, Kent

Mr. Wallace and the Organs of Speech

IN his article in NATURE, vol. xxiv. p. 244, Mr. Alfred Wallace has given one of the keys to the formation of speech-language. He says, "When we name the *mouth* or *lips* we use labials; for *tooth* and *tongue*, dentals; for the *nose*, and things relating to it, *nasal* sounds; and this peculiarity is remarkably constant in most languages, civilised and savage." Of this he gives examples from Australasia.

Perhaps it may be said there is not much novelty in Mr. Wallace's observations, as many of us have said the same. I have gone over some of his ground in my small "Comparative Philology" in 1852, but I did not hit the point. Indeed what Mr. Wallace gives us is very little, but when it comes to be applied it acquires the highest importance. We have all known that *nose* is often a nasal, but Mr. Wallace distinctly puts it that *mouth* is a labial, *tooth* a dental, and *nose* a nasal. This however gives us by these words and their connections, as stated by Mr. Wallace, a very poor vocabulary, and leaves most of the phenomena of speech-language unaccounted for, and it gives no explanation apparently of the derivation of speech-language from sign- or gesture-language, and the connection of character with both.

Setting Mr. Wallace's illustrations aside—for though they are true, and taken from his own domain, they are not the most apt—we will search farther afield. Chinese will be convenient. In Chinese, for a reason that need not be explained, *mouth* is not now a labial, but in the series connected with it there are many labials. The series is best illustrated by the characters. The old characters are round; the new characters, as in other classes, are now square, conventionally representing the round. Now *mouth* is a round or circle, O (or □). *Ring* is a round or circle O (or □). The character for *mouth* is in fact a *ring*, or round, or circle. On looking for other corresponding characters we have *eye* with O differentiated. Here we get a labial *mu*. *Face* is another round character, and that is *mien*. *Ear*, *head*, *blood*, *pot* (*ming*), *sun*, *moon*, *woman*, *mother* (*mu*), *white* (a labial), *field* or *garden*, *four* are all differentiated forms of each other and of mouth, as we know they ought to be. In cuneiform these characters are round, square, or triangular.

Of many of these psychological relations of words a list or dictionary will be found in the table of equivalents in my "Prehistoric and Protohistoric Comparative Philology." I observed and collected the facts, but did not know the full meaning of them for a long period; and in a paper as yet unpublished by the Biblical Archæological Society I carried the subject still further, particularly as regards cuneiform and Chinese. Indeed, when Mr. Wallace published his article, I had the facts just cited ready for reference in my hand. The reason I did not grasp the solution was this: I have known for years that words forming what I now call ring characters were related to *eye*, and that *eye* is almost a constant in these investigations, equivalent to a molar in various departments of biological research. Indeed it was by the use of *eye* as a constant that I was able to make those numerous and rapid philological analyses which have excited so much distrust among those unacquainted with the process I used.

I found that if I could classify *eye* in a language under examination, it gave me *sun* and many other words, and it led me to much valuable work, but I was often thrown out for reasons I did not then know. Empirically I found *eye* was a constant, and I knew it was a round, because in many languages east and west *sun* is the day eye or day's eye; *moon* is the night eye, and *eye* the head eye. In the North American languages and in the Malay, for instance, there was the evidence of a common law of psychological philology, which led me to greater results. My knowledge became modified to the extent that *sun* was not day eye, but day round. Until Mr. Wallace's article appeared, I still regarded *eye* as the pivot on which the "round" words and characters turned, although I knew that *mouth* was the prototype of *moon*, *mother*, *woman*, *egg*, &c., and of objects and ideas having a periodicity or a month. Having a false pivot, I was never able to bring the facts into a right connection, although coming very near. The Chinese modifications of the ancient character show that *mouth* and *ring* constitute the primary character, and thereby indicate the primary word.

The researches of Col. Garrick Mallery, U.S.A., and my own, in the paper unpublished, show the connection of sign language and characters, and I have determined a relation between sign language, character, and words, as in the sign or character || for son, offspring, &c. The characters in many cases appear as ancient as the signs, and may have preceded speech language. How words were connected with ideas and their representatives by signs was the problem. The new explanations of Mr. Wallace in your paper, or the old observations of others, in giving explanations from natural cries and sounds, &c., are not always exact, and do not account for the fact that the sounds are in relation with the sign language. Thus the words for *eye* and 2 are the same, and the words for *ear* and 3, and so forth.

In the brief remarks now made I endeavour to steer clear of many things which would require a long explanation, and to bring my observations to bear on Mr. Wallace's article. On speech language being constituted, the application of a labial to *mouth* gave a large series, and so of the dentals, &c. As the numerals are in relation to each object of the universe in primitive symbology, so they were supplied. Indeed nouns, adjectives, pronouns, verbs, numerals, particles, were supplied from a common fount. There are languages constituted of a few differentiated words, which can be traced throughout.

In connection with Mr. Wallace's remarks is to be taken what he says afterwards of the action of the lips. In the sign languages and the characters the lower organs supply a large number of ideas regarded as phallic. Such are ||, ·|·, O, &c. These ideas are not capable of direct connection with sounds; they came however into connection by the acknowledged correspondence of the parts in symbology and mythology. Thus the labial sounds became the representatives of actions or ideas illustrated by the corresponding lower organs, as in *go* and *come*.

Taking Mr. Wallace's terms and applying them, we therefore get the connection established between the sign languages and the speech languages, and we can see the psychological grounds on which they continued in working together, and why the speech languages have not everywhere *always* exterminated their ancestors. For this, and for the whole state of affairs, Mr. Wallace furnishes me with an explanation.

His naked statement is the best, that for *mouth* a labial was used. In the sign languages, and we find this in the prehistoric languages and their equivalents, several signs are used for one idea, and several ideas for one sign. When a labial was applied for the *mouth*, it was indifferent what labial. If one used a *b*, another would use *m*. This is one cause of the variety we find in the prehistoric primary languages, for there never was what philologists are fond of, one primitive language.

Many will object to Mr. Wallace, that *mouth* is not always represented by a labial, and in the common course hold that the negative evidence overcomes the affirmative. In many instances *mouth* is a dental, because the idea includes the *teeth*, which are dental. Again *tongue* is not always a dental, but a sibilant, so far as it is connected with *snake*. It is the whole knowledge of the facts which will better enable us to complete our progress and to overcome difficulties. For myself I have derived particular advantage from Mr. Wallace, in being enabled to understand my own work. HYDE CLARKE

32, St. George's Square, S.W.

Comets and Balloons

THE notion that the tails of comets are produced by an emission of the nucleus prevails at present among astronomers. I have just stated in a small pamphlet, 8vo, 32 pages, the reason why I presume to entertain another opinion on this subject. The details of my last aërial trip of July 2 show that by using an electric light night ascents at a reasonable distance from the sea may be considered as relatively without danger. The appearance of Schaberle's comet seems to me to afford a proper occasion for testing the emission theory, and I will try to explain my idea as shortly as possible.

It is pretty certain that any comet will lose something of its brilliancy in consequence of passage to the perihelion, consequently, *cæteris paribus*, it must be found with a diminished luminous power in the second part of its track. The consequence is that to test this theory the same comet should be observed in a similar position, as close as possible, in the first and in the second parts of its track.

By ascending with a balloon in the northern hemisphere to inspect Schaberle's comet on a moonless night, and estimating its luminous power in a clear sky at several determined heights, a great step will be made in reaching this desirable end.

It would be for the astronomers of the southern half of the world to ascend under similar conditions, and to make corresponding observations. If no visible diminution is proved to have taken place, much will have been accomplished in the determination of the true nature of this mysterious object.

The same observations could, it is true, be prosecuted without the help of aërostation, but not with the same amount of certainty, as much doubt remains as to the true luminosity of a celestial body when it is not inspected in a really perfectly clear sky, which can always be procured with a balloon—it is true not without incurring some personal risk, certainly not out of proportion, at all events, to the results to be expected.

W. DE FONVIELLE

Animal Instinct

I AM exposed to some annoyance from a clever old donkey, who, being turned out on to the green in front of my house, constantly lets himself into my garden to graze on my lawn. This he effects by pushing his nose between the rails of an iron gate, and then pressing down the latch of the gate. Expulsion, with ever so striking an appeal to his feelings, avails only a short time for his exclusion, unless the gate is locked.

Little Park, Enfield, August 19 W. B. KESTEVEN

ITALIAN DEEP-SEA EXPLORATION IN THE MEDITERRANEAN

AFTER my communication of the 4th inst. from Asinara I feel sure that many readers of NATURE will be interested to know something more of our doings; so I take the opportunity of our short stay here to send a very brief account of our doings since leaving Asinara.

The presence of a deep-sea fauna in the Mediterranean which I announced in my last is fully confirmed, and even though most of the species dredged are as yet undetermined, I can venture to say that the character of this fauna is "Atlantic," and, I may add, "Oceanic." My first bit of news was the capture of a *Willemœsia* identical, or very nearly allied, to *W. leptodactyla*; since then some ten or twelve specimens of that most interesting and characteristic Crustacean have been secured off the west, south, and east coasts of Sardinia, in depths varying from 950 to 2145 metres. All our deep hauls have brought up some living animals, usually Annelids and deep-red shrimps of at least three species; the greatest depth we have trawled in is 3115 metres; the greatest we have found sounding is 3630 metres in the eastern basin between Sardinia and Naples.

On the 10th inst., off the west coast of Sardinia we dredged two specimens of a Macrurid fish, which I take to be a *Malacocephalus*, from depths of 2805 and 2908 metres. South of the Gulf of Cagliari we got a new—to me—and exceedingly remarkable Macrurid, with what

look like the so-called supplementary eyes along the belly and tail, the rare *Macrurus sclerorhyncus*, *Hoplostethus Mediterraneus*, and *Haloporphyrus lepidion*, from depths of 508, 656, 860, and 1125 metres. We have at least two species of *Terebratula* from depths varying from 600 to 1200 metres. Several most interesting Crustaceans besides those mentioned, and even a non-swimming Brachyurous Decapod, from 1125 metres! But what is still more interesting is the capture of several specimens of a *Hyalonema*, very probably *H. Lusitanica*, but without any spiral twist in its long spiculæ; we got them off the south and east coasts of Sardinia, in depths from 1600 to 623 metres; we have, besides, several other forms of Sponges, all siliceous, and several of a curious agaric-like form. We have a *Brisinga* from depths of 2145 to 2300 metres, but very few other Echinoderms; I do not yet give up the hope of seeing a *Pentacrinus* before our cruise is ended. We have, as I mentioned, various Annelids and Gephyreans, and some fine species of Madreporia of the deep-sea forms.

We have an interesting set of serial thermometric observations, which show that there is a slight difference in the bottom temperature between the basins east and west of Sardinia, the latter being slightly colder. Negretti and Zambra's new deep-sea thermometers have answered admirably, suspended in the peculiar frame devised by Capt. Magnaghi. HENRY HILLYER GIGLIOLI

Naples, August 20

ON THE VELOCITY OF LIGHT

THE result announced by Young and Forbes (Roy. Soc. *Proc.*, May 17, 1881) that blue light travels *in vacuo* about 1·8 per cent. faster than red light, raises an interesting question as to what it is that is really determined by observations of this character. If the crest of an ordinary water wave were observed to travel at the rate of a foot per second, we should feel no hesitation in asserting that this was the velocity of the wave; and I suppose that in the ordinary language of undulationists the velocity of light means in the same way the velocity with which an individual wave travels. It is evident however that in the case of light, or even of sound, we have no means of identifying a particular wave so as to determine its rate of progress. What we really do in most cases is to impress some peculiarity, it may be of intensity, or of wave-length, or of polarisation, upon a part of an otherwise continuous train of waves, and determine the velocity at which this *peculiarity* travels. Thus in the experiments of Fizeau and Cornu, as well as in those of Young and Forbes, the light is rendered intermittent by the action of a toothed wheel; and the result is the velocity of the group of waves, and not necessarily the velocity of an individual wave. In a paper on Progressive Waves (*Proc.* Math. Soc. vol. ix.), reprinted as an appendix to vol. ii. of my book on the "Theory of Sound," I have investigated the general relation between the group-velocity U and the wave-velocity V. It appears that if k be inversely proportional to the wave-length,

$$U = \frac{d(kV)}{dk},$$

and is identical with V only when V is independent of k, as has hitherto been supposed to be the case for light in vacuum. If however, as Young and Forbes believe, V varies with k, then U and V are different. The truth is however that these experiments tell us nothing in the first instance about the value of V. They relate to U; and if V is to be deduced from them it must be by the aid of the above-given relation.

When we come to examine more closely the form of this relation, we see that a complete knowledge of V (as a function k) leads to a complete knowledge of U, but that a complete knowledge of U—all that experiments of this kind can ever give us—does not determine V, without the aid of some auxiliary assumption. The usual assumption is that V is independent of k, in which case U is also independent of k. If we have reason to conclude from observation that U is not independent of k, this assumption is disproved; but we can make no progress in determining V until we have introduced some other.

It is not easy to see how the missing link is to be supplied; but in order to have an idea of the probable magnitude of the difference in question I have assumed the ordinary dispersion formula $V = A + Bk^2$ to be applicable. Taking the ratio of wave-lengths of the orange-red and green-blue lights employed as 6 : 5, I find that for red light $V = U(1 - \cdot 0273)$, so that the velocity of the wave would be nearly 3 per cent. less than that given by Young and Forbes as the result of the experiment.

Under these circumstances it becomes a matter of interest to examine the bearing of other evidence on the question of the velocity of light. Independently of the method of the toothed wheel, the velocity of light has been determined by Foucault and Michelson using the revolving mirror. It is not very obvious at first sight whether the value thus arrived at is the group-velocity or the wave-velocity, but on examination it will be found o be the former. The successive wave-fronts of the light after the first reflection are not parallel, with the consequence that (unless V be constant) an individual wave-front rotates in the air between the two reflections.

The evidence of the terrestrial methods relating exclusively to U, we turn to consider the astronomical methods. Of these there are two, depending respectively upon aberration and upon the eclipses of Jupiter's satellites. The latter evidently gives U. The former does not depend upon observing the propagation of a peculiarity impressed upon a train of waves, and therefore has no relation to U. If we accept the usual theory of aberration as satisfactory, the result of a comparison between the coefficient found by observation and the solar parallax is V – the wave-velocity.

The question now arises whether the velocity found from aberration agrees with the results of the other methods. A comparison of the two astronomical determinations should give the ratio $U : V$, independently of the solar parallax. The following data are taken from Mr. Gill's "Determination of the Solar Parallax from observations of Mars made at the Island of Ascension in 1877."

The time k, required by light to travel a mean radius of the earth's orbit, has been determined by two astronomers from the eclipses of Jupiter's satellites. Delambre found, from observations made in the last century, $k = 493{\cdot}2$s., but recently Glasenapp has obtained from modern observations the considerably higher value, $k = 500{\cdot}8$s. $\pm$ 1·02. With regard to the constant of aberration, Bradley's value is 20″·25, and Struve's value is 20″·445. Mr. Gill calculates as the mean of the best modern determinations (nine in number), 20″·496.

If we combine Glasenapp's value of k with Michelson's value of the velocity of light, we get for the solar parallax 8″·76. Struve's constant of aberration in conjunction with the same value of the velocity of light gives 8″·81. From these statements it follows that if we regard the solar parallax as known, we get almost the same velocity of light from the eclipses of Jupiter's satellites as from aberration, although the first result relates to the group velocity, and the second to the wave velocity. If instead of Struve's value of the constant of aberration we take the mean above spoken of, we get for the solar parallax 8″·78, allowing still less room for a difference between U and V.

Again, we may obtain a comparison without the aid of the eclipses of Jupiter's satellites by introducing, as otherwise known, the value of the solar parallax. Mr. Gill's

value from observations of Mars is 8″·78, agreeing exactly with Michelson's light velocity and the mean constant of aberration. Some other astronomers favour a higher value of the solar parallax, such as 8″·86; but whichever value we adopt, and whether we prefer Cornu's or Michelson's determination of the light velocity, the conclusion is that there can be no such difference between the group velocity and the wave velocity as 2 or 3 per cent., unless indeed the usual theory of aberration requires serious modification. These considerations appear to me to increase the already serious difficulties, which cause hesitation in accepting the views of Young and Forbes. The advent of further evidence will doubtless be watched with great interest by scientific men.

One other point I may refer to in conclusion. Speculations as to harmonic relations between various spectral rays emitted by a glowing gas proceed upon the assumption that the frequency of vibration is inversely proportional to the wave-length, or, in other words, that the velocity of propagation V is independent of the wave-length, the question now at issue. If the views of Young and Forbes are correct, calculations of this kind must be overhauled. On the other hand, the establishment of well-defined simple ratios between wave-lengths would tend to show that V does not vary. RAYLEIGH

August 15

ELECTRIC LIGHT IN COLLIERIES

AUGUST 9, 1881, witnessed the first practical application in the United Kingdom of the electric light to the illuminating of coal-mines. The Earnock Colliery, near Hamilton, Lanarkshire, belonging to Mr. J. Watson, has been fitted with Swan's incandescent lamps specially arranged with outer lanterns of stout glass, air-tight, and provided with steel guards. The workings in which the lamps were fixed are 118 fathoms, or 708 feet below the surface. Twenty-one brilliant little lights placed at the pit-bottom, in the roads, and at the actual face of the seam where active operations were in process, supply an illumination of a very different character from the dismal glimmer of an occasional Davy. The electricity was generated by a dynamo-electric machine at the surface worked by a special 12 horse-power engine, and conveyed by two cables, first along telegraph poles to the pit mouth, then down the shaft to the workings, in one section to a distance of half a mile. The overhead wires are naked copper wires of ⅜ inch diameter, while those below ground are carefully insulated, and in the shaft are protected with an outer tube of galvanised iron. At suitable points of the circuit safety air-tight switches, the invention of Messrs. Graham of Glasgow, are inserted to afford control over individual lamps. The mine was visited two days after the installation of the light by members of the Mining Institute of Scotland, with whom was Mr. W. Galloway, whose remarkable experiments on the explosive effects of coal-dust will be remembered in connection with the more recent report of Prof. Abel. The party were photographed in the workings. An experiment was made with a lamp to test whether in the event of its being broken by accident a surrounding atmosphere of explosive gas would or would not be kindled by the strip of red-hot carbon before it had had time to cool. Into a box containing about three cubic feet of explosive gas a single lamp, removed from its outer protecting case of stout glass, was placed, and the current was turned on. The fragile bulb inclosing the incandescent carbon thread was then purposely broken, when the gas inclosed in the box immediately exploded. No such occurrence could possibly happen if the protecting case of stout glass is properly constructed. The risk of accident must be considered as immensely less than that of the ordinary Davy lamp, especially when it is remembered that with the brilliant light of the electric lamps they need no longer be carried in the hand or set down upon the floor near the actual spot where the coal is being got, but will be fixed overhead at a safe distance against the wall of the mine. The ease with which the light can be turned out during the firing of a blast is another point in their favour. The proprietor of the Earnock Colliery is greatly to be congratulated on the step he has taken. In 1880 the death-roll of the slain by explosions of fire-damp in Great Britain reached the figure of 499 persons. We venture to predict that the universal adoption of electric lighting in fiery mines would reduce this figure to one-tenth of its terrible proportions. How many years will it be, we wonder, before the adoption of electric lighting will be made compulsory by Act of Parliament? And how many colliery owners will discover, we would ask, when driven to this course by compulsion, that in the long run they effect an economy by discarding the clumsy and unsafe "safety"-lamp, which will so soon be numbered with the "flint mill" amongst the relics of the past?

SINGULAR STONE HATCHETS[1]

MONSIEUR PITRE DE LISLE has lately called attention to a singular class of stone celts or hatchets which have for the most part, if not indeed only, been found in Brittany and North-Western France.

These hatchets, instead of tapering away to a more or less conical point at what has been termed the butt-end, suddenly expand close at that part, so as to present a somewhat button-like termination. In one instance, at least, the hatchet ends in a spheroidal ball not unlike that which one occasionally sees on the horns of cows which are inclined to make too free use of their natural arms of offence. In the case of the hatchets, however, the button is at the opposite end to that which was in use for cutting. These blades vary in length from about three inches to as much as fifteen inches, and are all made of rocks belonging to the family of Diorites, principally of Aphanite.

M. de Lisle has given to these instruments the name of "*haches à tête*" or "*haches à bouton*," and has pointed out the similarity which in some respects they bear to hatchets of Carib origin and to the *merés* of New Zealand. In these instances the object in view in forming a projecting rib round the end of the blade was no doubt to afford the means of preventing it from slipping out of the handle or hand which held it. He thinks that the same object led the makers of these French blades to adopt the same form, and that the hatchets, after passing through a transverse hole in their hafts, were secured by cords wound around them, which abutted against the projecting rims at their small end. In his opinion there is a representation of this method of hafting to be seen among the sculptures on the dolmen of Mané-Lud.

It is a remarkable circumstance that the hatchets of this particular form appear to be restricted to so small a district of France, and not to occur elsewhere. M. de Lisle is in consequence inclined to assign the development of this type to a late period in the Neolithic age, and has offered some reasons for inferring that in Brittany the use of bronze hardly found a home, and that stone was the principal material employed for cutting tools when first that part of Gaul was brought in contact with Roman civilisation. It seems probable enough that in that as in other countries there were districts which lay far away from the principal highways of progress and civilisation, and where old-world usages prevailed long after material advances had been made in more fortunate but not very distant regions.

We may however be allowed to doubt whether the country of the Veneti, the most enterprising maritime tribe of Gaul, whose ships in the days of Julius Cæsar were already provided with chain-cables of iron, were

[1] "Les Haches à Tête de la Bretagne, etc." (Nantes, 1880.)

among those in the rear of civilisation. However this may be, M. Pitre de Lisle has done good service to archæology in publishing his monograph upon this peculiar form of stone implement or weapon.

INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES

UNDER the authority of the Comité International, representing several countries of Europe, the United States, and South America, there has been recently published, by Gauthier-Villars of Paris, an important volume of Memoirs by Dr. Broch (Directeur du Bureau), and Drs. Pernet, René-Benoît, and Marek (Adjoints du Bureau), on the following subjects relating to the determination of units of measure and weight.

As the intensity of weight varies with geographical position and height above level of the sea, the Comité give in their first memoir tables of the ratio of the acceleration of weight at the level of the sea, for different latitudes, to its acceleration at latitude 45° (Paris), to which latitude the Comité recommend that all weighings might be referred. The tables are based on the formula of Laplace, the coefficients of which are corrected by Broch in accordance with recent deductions as to the figure of the earth. In the second memoir, which relates to the tension of aqueous vapour, certain corrections of hitherto accepted results are also indicated, particularly the errors of calculation in Regnault's tables as shown by Moritz, and new tables are given for tensions at all absolute barometer heights for normal degrees from $-30°$ to $+101°$ C.

With reference to the fixed points of mercurial thermometers, the Comité adopted the proposition that the point 0° of the Centigrade thermometer should be fixed at the pressure of 760 mm., when determined in 45° latitude, and at the mean level of the sea. Also at the Congress of Meteorologists at Rome in 1879 there was adopted the proposition of Dr. Pernet, to fix the boiling point of water, 100° C., under the above pressure, so as to render strictly comparable the temperatures observed at different places. Degrees of temperature between these points are termed normal-degrees.

Tables are also given, by which may be calculated the weight of a litre of pure air in different latitudes and at different altitudes. In London (lat. = 51° 30′, alt. = 6·7 metres) the weight is 1·2938 grammes. The Comité have adopted the term litre for expressing the volume of a kilogram of pure water, instead of the term cubic-decimetre.

In a report by M. Herr on the Austrian unit of weight (Vienna, 1870), the volume of pure water at various temperatures is stated from the means of observations by Muncke, Stampfer, Kopp, and Pierre, the maximum density of water being taken at 3°·92796 C. By this formula there have been calculated, under the directions of the Comité, tables of the volume and specific weight of water from 0° to 30° C.

One of the principal works executed during 1878-9 was the comparisons of the standard kilograms at Vienna, Paris, and London. An elaborate report on these comparisons is given by M. Marek, who, by improved methods and instruments, has obtained great accuracy. The probable error of his weighings is about 0·002 mgr., or 1-500,000,000th part of the whole weight. The results also show that the material of which the standards are made, 90 per cent. of platinum and 10 per cent. of iridium, is of all known bodies the least affected by time or atmospheric changes.

In a paper on Fizeau's apparatus for determining the rates of expansions of bodies by heat, by means of an optical method founded on the phenomena of interference, Dr. Benoit gives the results of his own experiences with a similar apparatus. The results show the wonderful delicacy of Fizeau's dilatometer, as the expansions by heat of small specimens of platinum are shown in a manner incontestable to millionths of a millimetre (0·00000004 inch).

An interesting account of the establishment and objects of the Bureau is given in a preface to this volume by the Secretary to the Comité, Dr. Ad. Hirsch; and it is hoped that the efforts made by the Comité to bring about international agreement on the scientific points above referred to will commend themselves to all engaged in accurate work.

H. J. CHANEY

A MODEL PUBLIC LIBRARY

ENGLISHMEN are fond of descanting upon the evils of too much centralisation, which they see displayed in some foreign systems of government, urging the amount of red tape rendered almost necessary, its inflexibility, and lack of adaptation to the infinitely-varying circumstances of different communities. But, on the other hand, the extravagant cost of working every undertaking by a separate organisation, especially in a community not large enough to make such undertakings great matters, must come forcibly home to many of those who are naturally selected to work upon several.

There has lately come under our notice an admirable case of a public library avoiding this waste, securing all the energy of private zeal, and at the same time increasing the working power of it by becoming, as a public library should become, the centre of all secondary education and the parent stem of many and various branches. If any of the smaller towns of England feel that a free library would not in their case stand by itself on account of small income, we commend this to their notice as a specimen of the advantages of co-operation.

Watford has a population of about 10,000, and the penny rate on last year's gross rental of 34,589*l.* brought in 144*l.* 2*s.* Yet this small amount has developed round it an expenditure of 700*l.* a year, equal to five times the largest rate collected, besides a large outlay on buildings at the beginning, costing some 3000*l.* subscribed, in addition to the gift of the land. Ten distinct sections are worked in connection with it. The accounts of each are shown in a separate balance-sheet each year, and the agenda paper, with notice of committee-meetings, shows how methodically the work of each section is carried out and overhauled.

Section A, the Library proper, contains about 7000 volumes; a payment of three shillings a year, or fourpence a month, is required for taking books home to read; the yearly issues accordingly amount to about 12,000. The only *free* part of the library, the reading-room, shows a something similar use of books; it is patronised chiefly by young men in the winter time, under the arrangements of Section D. The small subscription enables the book committee to spend about 50*l.* a year in new books; magazines and periodicals being supplied by a separate club, connected, of course, with the institution. We should be glad to see the troublesome and irksome system of guarantors dropped. Towns which have freed themselves from the labour and annoyance they entail, though containing far larger proportions of the "great unknown" than can a place of the size of Watford, have found no evil result. The subscription also, though small, seems to render it less necessary here.

Section B is the School of Science and Art, the latter division showing clearly that the public library at Watford by no means attends to the wants of the industrial classes only, for non-Government pupils may pay six guineas a year for drawing only. For the benefit of the evening classes, at which non-Government pupils pay a guinea and a half for the year's instruction, and Government students (whose income, that is, or parents' income, does not exceed 200*l.* a year) half that, "the subjects are

arranged to meet the requirements of artisans and those engaged in mechanical trades, and include mechanical drawing, building, construction, modelling, model drawing, outline drawing, and shading."

This section includes science teaching also, and classes were formed last year in inorganic chemistry, botany, principles of agriculture, fully illustrated by experiments; shorthand also, a useful help to all such studies.

Section C is Entertainments, and is set going or stopped as occasion offers; of course it is expected to be a help to the general fund.

Section D is the Youths' Institute, supported also by special honorary subscribers and by one penny a week paid by its members for admission to the reading-room, *because without that the room was too full.* Let us hope that such a very unsatisfactory statute of limitations may soon cease to be necessary. An extra rate would hardly be grudged in such a case, and it is a strong argument for Parliament authorising one.

Section E is a Private Subscription-room, supported by about a hundred members, who subscribe ten shillings a year each, spent in newspapers and periodicals, made available to the public after their use by the club.

Section F is a School of Music, where some of the best masters obtainable in London are engaged, two of whom take the combined instruction, and four take separate instruction. Nine different classes of lessons are arranged for, again not free, but all made available to those who wish for them, at very little expense or labour to themselves. It is now supported by 160 students, and by subscriptions from the vice-presidents.

Section G is the nucleus of a Museum; and how treasures of local interest are lost to a town for want of such a nucleus in trustworthy hands, the writer knows well! Central museums also can easily supply duplicate treasures to such institutions in almost perpetual succession.

Section H, the English Literature Club, meets weekly through the winter, at the library rooms, adding greatly to the care with which books are read, and, consequently, to the pleasure and information drawn from them. A very small subscription pays its, no doubt very small, expenses.

Even needlework in elementary schools, though spread about half the County of Herts, and patronised by a goodly company of influential ladies throughout the district, has its "head centre" in a committee of three gentlemen of the Watford Public Library. Fifty-four schools compete, and 1500 specimens are shown, in six classes of work, all having undergone a strict and very systematic examination.

The same association supports also a School of Cookery.

Other offshoots of the Library are the Herts Natural History Society, the Foresters' Club, Junior Foresters' Club, and the Shepherds' Club, each having its meeting-place at the library rooms.

Now among this variety of work there is probably none which is not carried on in nearly all the larger towns of England by *some* means. What we wish to set forth is the reasonableness of its all forming together the work of a single "committee of education," not necessarily elementary only by any means; and that a rate-supported public library should be the central institution, whose committee should set in co-ordinate motion all the parts of this local educational machine. Such a committee need not attempt to take into its hands the entire control of each separate branch, but should work all together with as little friction and loss of labour as possible, and especially should this be the case, as we have said, in our smaller towns. Very great is the economy of one institution working all together, in the matter of rooms, advertising, and printing; in one man receiving, as the librarian does at Watford, all the subscriptions and fees paid to these various societies, the 5 per cent. allowed him upon all giving him a tangible interest in increasing each, as such a central worker must have the means of doing, and in stirring all up to a friendly rivalry in well-doing. And the advantage can hardly be over-stated of the power of such an organisation to bring together earnest workers, who might otherwise have followed either a secluded path or one crossing that of other workers; in the one case, occurring most frequently in small communities, doing little for the advance of intelligence and information; and in the other case, to which large cities are most liable, wasting time and efforts which are often thwarted by mere local jealousies.

NOTES

IN June of the present year the freshwater jelly-fish (*Limnocodium Sowerbii*) reappeared in the Royal Botanical Society's Gardens, Regent's Park, though in no great numbers. At the suggestion of Mr. George Busk, F.R.S., and with the courteous assistance of Mr. W. Sowerby, a small number were captured and transferred to the Victoria tank in Number 10 House at the Royal Gardens, Kew. Nothing was known of their fate till about a week ago, when it was observed that the whole tank was swarming with the progeny of the small colony brought from London.

THE Local Committee at York have been making laudable exertions for the accommodation of those who intend to be present at the meeting of the British Association next week. They have prepared a long list of hotels and lodgings, with prices, at the same time stating that the prices of the lodgings are higher than will be eventually charged, "as there is abundance of good accommodation at reasonable rates." They have also issued a time-table of the arrivals and departures of trains at York station from the principal towns in the kingdom, with special tables for the local lines. A map of the city has besides been prepared, showing the situation of the principal buildings, the meeting-places of the various sections, and the principal hotels, of which there are fourteen.

THE *Times* Geneva correspondent gives some further particulars concerning Prof. Raoul Pictet's model steamer now in course of construction, with which he expects to reach a speed of forty miles an hour, and which will make a trial trip on the lake in November next. Her dimensions are—16 metres long and 3·50 metres wide. When lying at anchor she will draw 33 centimetres fore and 44 centimetres aft; at full speed 1 centimetre forward and 16 centimetres aft. The engine will be placed amidships, from which point to the stern the screw-shaft and the keel form an inclined plane; the bows are long, tapering, and wedge-shaped. Prof. Pictet reckons that his invention will lead to a great saving of fuel, inasmuch as a steamer built on his plan, after being started with say 100 horse-power, may be kept up full speed with an expenditure of force equal to thirty horses. The form of the hull, on which the maintenance of the ship's equilibrium will depend, cannot be explained without a diagram. Prof. Pictet is quite confident in the success of his invention, and his previous scientific achievements have been so remarkable that many people who cannot follow his reasoning have no hesitation in accepting his conclusions.

THE inhabitants of Havre are collecting money for raising a statue to Sauvage, who is considered in France as having applied the screw to the propelling of steamers.

A TELEGRAPHIC experiment of a singular description was tried last week at the Trocadéro. It consists merely in the reading of large silvered zinc letters, a square metre in size, fixed on a blackened board, by refracting telescopes. This method has succeeded very well from the Trocadéro to the Panthéon, a distance of about three miles. The inventor, an

officer in the French service, thinks he will succeed in reading messages at a distance of sixty miles under favourable circumstances.

M. EUGÈNE GODARD, the celebrated French aëronant, who has been making ascents at Gotha, has been made by the Grand Duke a knight of his order. M. Darmentiers, another French aëronaut, having ascended from Montpelier, has been less successful. He was driven by a strong wind out to the Mediterranean Sea, where he perished according to all probability, no news having been received from him up to the latest date.

AN advertising vehicle is circulating in the Paris streets lighted at night by voltaic electricity obtained by bichromate elements. It circulates all round the Boulevards.

THE triple granite concentric vaulting of the St. Gothard Tunnel, in the quicksand formation under Andermatt, is now completed, and as the rings previously constructed remain intact, it is confidently hoped that a difficulty at one time thought to be insuperable has been conquered, and that the great tunnel will be finished by the end of October.

FROM the Report in the last Technological Examination of the City and Guilds of London Institute we see that the number of candidates and centres for examination have largely increased. The results are generally better than in the previous year, though from the examiner's reports there is, in most subjects, much to be desired. No doubt in the course of a few years, when the Institute has been fairly at work, the improvement in technical knowledge in the country will be very marked.

AN official investigation shows that the phylloxera infests an area of over 8000 square metres of the vineyards at Heimersheim, near Remagen, on the Rhine. The diseased vines were imported from Austria. Energetic steps are being taken for the annihilation of the disease.

THE death is announced of Capt. Popelin, one of the Belgian African explorers, who had established the station at Karema, on the south-east shore of Lake Tanganyika, where, apparently, he has succumbed to fever. Capt. Popelin was only thirty-four years of age.

THE British Archæological Association is holding its meetings at Malvern, which forms an admirable centre for excursions to antiquities of all kinds.

THE "Polytechnic" expires this week, many of our readers will be sorry to hear. The age seems to have outgrown its toy science, though doubtless the institution did good in its day in paving the way for the popularisation of real science. In the memories of our older readers it will doubtless be associated with many a happy day.

MR. WILLIAM ARCHER, F.R.S., Librarian of the National Library of Ireland, has just issued a pamphlet which we commend to his fellow-librarians. The pamphlet consists of "suggestions as to public library buildings, their internal plan and construction, best adapted to effect economy of space (and, hence, saving of cost), and at same time most conducive to public, as well as administrative, convenience, with more especial reference to the National Library of Ireland." We cannot enter into the details discussed by Mr. Archer, but his leading principles as to arrangement may be thus summed up in his own words:—"Central reading-rooms and offices connected, by short and sufficiently numerous radii, with a continuous circuit of book-rooms around and beneath the same, the books in the book-rooms on a greater or less number of tiers of standing presses, these not more than eight feet high—thus securing the immense boon of the abolition of ladders and galleries, and saving at once space (and cost) as well as the time of the public." Mr. Archer draws attention to the fact that the question of space is gradually becoming more and more urgent as regards this, the only large public library in Ireland, and that if the providing of enlarged accommodation is to be delayed until a science and art museum and a metropolitan school of art should be built, so as to hand over the buildings at present occupied by these for library purposes, it looks very probable that the library must come to a "stand still." He states further that the number of readers was never before so great as during the past winter, the evening readers having especially increased. The National Library of Ireland, therefore, even in its present cramped and inadequate quarters, is fulfilling a mission of usefulness.

AN important discovery has been made in the vicinity of Kenneh, Upper Egypt. No less than thirty-six well-preserved sarcophagi have been brought to light. They almost exclusively belong to the kings and queens of the older Thebes Dynasty. They contained mummies, papyrus scrolls, Osiris statuettes (some thousands), ornaments, and talismans. The royal names of Raskenes, Amenophis I., Ahmes, Nofretari, Aahhotep, Totmes II. and III., Seti I., Ramses XII., Pinotem, and other Pharaohs are mentioned in the texts, and show the importance of the discovery. The sarcophagi were all found in one sepulchral chamber.

THE "Smithsonian Report" for 1879 affords ample evidence that the Smithsonian Institute continues to carry on its great work with increasing efficiency. Its grants of money have been devoted to the carrying out of anthropological researches and geographical explorations, and to the publication of a large number of works bearing on the progress of science. Large additions have been made to the various departments of the free museum of the Institute. The Appendix to the Report, occupying the larger half of the volume, contains a number of papers bearing on the anthropology of the North American Indians, one paper of considerable length being "A Study of the Savage Weapons at the Philadelphia Exhibition," by Mr. E. H. Knight. Other papers in the Appendix are: "On the Present Fundamental Principles of Physics," by Prof. Franz Joseph Pesko; "A Universal Meteorograph, designed for Detached Observatories," by Prof. E. S. Holden.

THE *Transactions* of the Norfolk and Norwich Naturalists Society for 1880–81 contains several useful papers. One of the most interesting is that of Mr. Southwell, "On the Extinction of Species by the Indirect Acts of Man." Mr. Southwell adduces a number of instances in point, showing that experimental acclimatisation sometimes leads to disastrous results. Mr. H. W. Feilden contributes some remarks on the Natural History of Franz-Josef Land. From the presidential address we are glad to see that the Society continues to prosper.

A SHORT time ago a colossal whale was captured and killed on a sandbank near Westerland-Lylt (Schleswig). It measured 52 feet in length, 26 feet in circumference, and its tail-fin spanned 7 feet across. The animal must have entered the Watten Sea during high tide and lost itself in shallow water, when the receding tide left it upon the sand.

THE Medical and Sanitary Exhibition, organised by the Committee of the Parkes Museum, was open for the last time on Saturday, August 13, when the number of visitors, exclusive of season-ticket holders, was 1,221, making a total of 24,333 visitors for the four weeks during which the Exhibition has been open, allowing only for one visit by each season-ticket holder. During the day the secretary, Mr. Mark Judge, visited the different exhibitors for the purpose of ascertaining their opinion as to the success of the Exhibition. The exhibitors generally expressed themselves as well satisfied with the result, some going so far as to say that they had done an exceptional amount of business owing to the fact that a very large proportion of the

visitors had been either medical men, architects, or engineers. The representatives of the exhibitors, who have been in daily attendance during the Exhibition, marked their appreciation of the arrangements made for their convenience by presenting on Saturday a small purse of gold to the superintendent, Mr. Smithson. The closing of the Exhibition was taken advantage of by the St. John Ambulance Association to give a demonstration of ambulance practice, and during the afternoon a large number of the visitors assembled in the conservatory to witness the practice, which was conducted by Major Duncan, Mr. Cantlie of Charing Cross Hospital, Mr. Furley, Dr. Crookshank, and Surgeon-major Baker. Prizes were competed for by squads of the Grenadier Guards, the Finsbury Rifles, and the Metropolitan Police. Mr. John Eric Erichsen (the chairman), Dr. Poore, Dr. Steele, Mr. George Godwin, Mr. Rogers Field, and other members of the Exhibition Committee were present during the day. It is expected that the prizes which have been awarded will be distributed at the Annual Meeting of the Parkes Museum in the autumn.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Lemur (*Lemur catta*) from Madagascar, presented by Mr. E. O. Brookfield; a White-collared Mangabey (*Cercocebus collaris*) from West Africa, presented by Mr. James Jameson; a Diana Monkey (*Cercopithecus diana*) from West Africa, presented by Mr. Louis Wyatt; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. R. Edge; two Vulpine Phalangers (*Phalangista vulpina*) from Australia, presented by Mr. George White; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from the Moluccas, presented by Mrs. Beard; two Wonga-wonga Pigeons (*Leucosarcia picata*) from New South Wales, presented by Mr. J. Burnham; a Royal Python (*Python regius*) from West Africa, presented by Mr. G. H. Garrett; a Grey Parrot (*Psittacus erithacus*) from West Africa, deposited; a Lesser White-nosed Monkey (*Cercopithecus petaurista*), two Pluto Monkeys (*Cercopithecus pluto*) from West Africa, purchased; a Zebu (*Bos indicus*), a Pampas Deer (*Cariacus campestris*), born in the Gardens. Amongst the additions to the Insectarium during the past week are pupæ of *Attacus aurota* (one of which has since emerged) and *Ceratocampa ixion*, from Brazil; larvæ of the Madder Hawk-Moth (*Deilephila galii*), the Death's-head Hawk-Moth (*Acherontia atropos*), the Poplar Hawk-Moth (*Smerinthus populi*), and Fox Moth (*Bombyx rubi*), and perfect insects of the Water Stick-insect (*Ranatra linearis*).

PHYSICAL NOTES

M. SAMUEL, of Ghent, has brought before the Belgium Academy a method of registering telegraphic signals received through the mirror galvanometer (*Bull.*, No. 5). On the screen receiving the light are fixed two selenium elements, one to the right, the other to the left. When either is illuminated its conductivity of course increases, and it acts as a relay on an electro-magnet, which causes a Morse dot or dash to be marked on paper. There are two local batteries, one having two circuits, which pass through the selenium pieces and the electro-magnets, while the other is for the electro-chemical writing. In this latter, a band of paper saturated with iodide of potassium passes continuously over a small copper cylinder which is connected with one pole of the second battery. Above the paper are the ends of the armatures of the electro-magnets; to one is attached a vertical platinum rod, to the other a small triangle with platinum base (horizontal). The rod and triangle are connected, through the armatures, with the other pole of the second battery, and they press the paper band on the cylinder each time the armatures are attracted, giving a dot or a dash as the case may be. The dashes, instead of being longitudinal, are at right angles to the length. If the ordinary lamp of the galvanometer be replaced by sunlight or lime-light, the electro-magnets (M. Samuel points out) may be actuated directly without use of a galvanometer relay; Bell's selenium elements having an average resistance of only 150 ohms in sunlight, and 300 ohms in darkness.

AN evaporimeter with constant level has been recently described by Prof. Fornloni (*Reale Ist. Lomb.*, vol. xiv. fasc. x.-xi). It consists of an oblong wooden case with a brass spiral descending into it from a micrometric screw. The spiral carries at its lower end a small glass vessel which acts as feeder. A glass siphon extends outwards horizontally from the feeder, and has at its outer end a small cup, in which the evaporation takes place. As the water evaporates in the cup the feeder is lightened, and rises by action of the spiral, thus keeping the level constant. A fine layer of oil in the feeder prevents evaporation from its water-surface. There are guides to control the vertical movements of the feeder, which, moreover, are indicated by means of a weighted thread, affecting an external index on a disk. The graduation of the instrument is expressed in millimetres of the height of water in the evaporating vessel.

SIGNOR MAURI (*Riv. Sci. Inst.* No. 11) obtains an economical and very compact battery carbon, intimately united with the electrode, as follows:—Finely-powdered graphite is mixed with an equal weight of sulphur (which should be free from carbonates), and the mixture is heated in an iron vessel until complete fusion of the sulphur. The temperature should not be raised beyond 200°. When the mass is fluid it is poured into a suitable metallic mould, and a thick copper wire, bent zigzag, is quickly inserted, a part being left projecting. The mass is let cool slowly: then it is easily drawn from the mould and is ready for use. These carbons have a conductivity practically equal to that of retort carbon, and are more electro-negative, consequently better adapted for electromotive force. Coke-powder cannot be substituted for graphite, because it has too little conducting power. By increasing the proportion of sulphur, the resistance may be increased at will, and strong resistances may be thus easily prepared in place of resistance-coils of copper wire. (S. Mauri further indicates a way of utilising graphite in construction of a miner's fuse.)

REPEATING Mercadier's experiments in which an intermittent beam meets smoked a surface within a glass tube containing aqueous or ammoniacal vapour, and furnished with an ear-tube, Prof. Mugna lately (*Riv. Sci. Ind.* No. 11) made the effects much better heard by attaching a small microphone to an elastic membrane closing the tube; and it was possible to operate at such distance from the interrupting apparatus, that its noise was no longer disturbing. Prof. Mugna further experimented by suspending horizontally from a cocoon-fibre, within a glass case, a short fine glass rod, with terminal laminæ of card or glass, or very fine metallic foil. An intermittent beam sent against one of the laminæ when they were in a position parallel to the wheel drove the system round in direction of the beam, indicating (the author considers) a direct action of the latter. An air-current due to thermal action should produce the opposite motion. Besides, the phenomenon is still better when the air is rarefied.

IN a note to the Vienna Academy, Dr. Margules calls attention to the beautiful figures that are produced in glycerine, when the liquid is moved in a regular way, by rotation of a disk in contact with it. These figures afford an insight into the form of the surfaces and paths of the currents. They are due to the water contained in the glycerine.

THE method described by Herren Kirchhoff and Hahnemann last year for determining the heat-conductivity of metals, has been applied by them (*Wied. Ann.*, No. 7) to three varieties of iron, and to lead, tin, zinc, and copper; and the electric conductivity of these metals has also been measured. The conclusion is that the ratio of these conductivities is in general constant in these different metals, with exception of iron, and it is thought the exception may be connected with magnetic properties. Herr H. F. Weber's result disagrees with this, for he finds the ratio to be a linear function of the product of specific heat and density. The authors are unable to discover the cause of this discrepance.

AN initial attempt to elucidate the ratio of the specific heat of liquid organic compounds to their composition has been made by Herr von Reis (*Wied. Ann.*, No. 7). It appears from his researches that the difference of the molecular heat of homologous compounds at 20° boiling point, and from 20° to 100°, is very regular: in the former case it is 8·0 and in the latter 7·5.

Alcohols form an exception, having a comparatively high specific heat; they gave the differences 9·7 and 8·5 respectively. Isomeric substances of similar composition have the same molecular heat, while those of unlike composition have a different. In the tables which give carbon and hydrogen differences there are exceptions along with regularity. For a right development of the theory Herr von Reis feels that more extended observation is necessary.

THE idea of qualitative analysis of substances by microscopical examination of crystalline forms is worked out to some extent by Herr Lehmann (*Wied. Ann.*, No. 7). A shallow watch-glass is substituted for the cover glass, and serves for turning over in various ways the crystals which form in the inclosed solution. The domain of regular forms is avoided as unsuitable, and only irregular forms observed—the so-called growth-forms, crystal skeletons, trichites, &c., produced by acceleration of crystallisation, viscosity, and so on. For details of Herr Lehmann's method and apparatus we must refer to his paper.

HERR SCHULLER has lately described to the Hungarian Academy of Sciences (*Wied. Ann.*, No. 7) a mercury air-pump which works automatically, and in which all greased glass combinations are dispensed with, the hermetic closure being effected with only glass and mercury. The evacuating power of the apparatus was not exactly measured; there are proofs that it is high.

MR. J. MILNE has written a careful account of the vertica and horizontal motions accompanying the earthquake of March 8, 1881, in Japan. This is believed to be the first earthquake in which a complete continuous record of both components of the motion has been obtained for a period exceeding twenty-five seconds. The actual maximum displacement appeared to be about 1·33 millims., recurring at the rate of about seven vibrations in five seconds. From the phenomena of this shock, and from some experiments on artificial earthquakes produced by letting an iron ball weighing about one ton fall from a height of about thirty-five feet, Mr. Milne argues that the waves that are felt are *transverse* to the line of propagation of the shock.

BIOLOGICAL NOTES

RELATIONS BETWEEN THE CRANIUM AND THE REST OF THE SKELETON.—These relations form the subject of a paper by M. Manouvrier, read at the last meeting of the French Association. The following are the author's conclusions:—1. The weight of the cranium varies, in a general way, with the weight of the skeleton, but not proportionally, like the weight of the brain. 2. The weight of the skeleton, less the cranium, in a given race, varies nearly in proportion to the weight of the femur. 3. The weight of the cranium is greater relatively to that of the femur, the lighter the latter is. 4. The weight of the cranium is much more considerable relatively to that of the femur in woman than in man. 5. This sexual difference is so pronounced that it constitutes one of the best secondary sexual characters. About 82 women in 100 have the cranium heavier than the two femurs, while 82 men in 100 have it lighter. 6. The lower jaw is heavier relatively to the cranium in the anthropoids than in man, is inferior than in civilised races, in man than in woman, and in the adult than in the child. 7. The weight of the cranium is smaller relatively to that of the lower jaw, the heavier the latter is, &c.

THE COLOUR CHANGES OF AXOLOTL.—Prof. Semper has lately examined axolotl with regard to the influence of light on its colour (*Würzburg Phys. med. Ges.*). When young axolotl are reared in darkness they become quite dark; nearly as dark in red light; in yellow, on the other hand, pretty bright; and brightest in bright daylight. The difference is connected not only with the chromatic function found in various degrees in all amphibia, but on pronounced formation of a peculiar diffuse yellowish green colouring matter, increase of white, and diminution of dark chromatophores. Further, when axolotl are exposed to daylight in white dishes covered with white paper, much less dark pigment forms in them than when they are kept in white dishes without a paper cover (other things equal); though in the latter case they are apparently exposed to the most intense light; these darker axolotl are, however, still much brighter than those reared in red light or in darkness. Since (as experiment showed) the white covering paper let through much light, but very little of the chemical rays, it appears that chemical rays play no part in the formation of pigment. But the causes of the whitening in bright daylight and the darkening in absence of light remain unknown as before.

SIREDON LICHENOIDES.—Mr. W. E. Carlin publishes in the June number of the *Proceedings* of the United States National Museum some very interesting details about this remarkable form. Its chief habitat is a body of water some two and a half miles in circumference called Como Lake. This has no known outlet, but is fed by a perennial stream of pure spring water. The lake is shallow, and its water very strongly impregnated with an alkali; it is very disagreeable to the taste. The Siredon never enter the freshwater stream; they abound in the alkaline waters of the lake in immense numbers. When about one hundred and fifty were placed in fresh water they seemed to suffer no inconvenience, but it had a remarkable effect in hastening their metamorphosis into the Amblystoma form. Of an equal number kept in fresh water and in the lake water, quite a change occurred with the former after twenty-four hours, while the latter showed no change after several days of captivity. Those that were kept well fed in jars usually began to show a slight change in from two to three weeks, and all of them completed the change into the Amblystoma inside of six weeks, while in some kept, but not specially fed, there were but three changes in three months. Specimens kept in captivity became quite tame, soon learning to know that tapping the jar in which they were, meant a fly, and, rising to the surface, would snap at whatever they saw first, pencil or fly.

FISH MORTALITY IN THE GULF OF MEXICO.—We glean a few more particulars as to this strange mortality from the June *Proceedings* of the United States National Museum. The fishing interest of Key West is an important one, supplying thousands with the means of subsistence. The fishermen state that a volcanic spring exists, the waters from which are of a high temperature. The polluted waters are of a red brick colour; their influence is seen for a distance of 200 miles. A scant supply of sea water from the Gulf of Mexico sent to Washington was examined by Mr. F. M. Endlich of the Smithsonian Institution. That in which the fish died (A) contained a large quantity of algæ and infusoria, and the pure water (B) had none. They gave the following analysis:—

	A.	B.
Spec. grav.	1·024	1·022
Solids per cent.	4·0780	4·1095
Ferric compounds per cent. ...	0·1106	0·0724
Injurious organic matters ...	ratio = 3	= 2

Even on spectroscopic analysis Mr. Endlich could not find in A any mineral constituent which could noxiously affect the fish, and he thinks that death must be caused by parasitic algæ, while Surgeon Glazier agrees with the prevalent opinion that the catastrophe is due to the salt water being impregnated with gases discharged from volcanic or geyser-like springs. During November last the waters of Tampa, Sarasota, and Charlotte Harbour were covered with thousands of dead fish, and the stench was quite overpowering.

THE BLOOD OF INSECTS.—Operating with the larva of *Oryctes nasicornis*, M. Fredericq has observed (*Bull. Belg. Acad.*) that the blood of the animal, drawn off in a small glass cannula, is a colourless liquid, but on exposure to the air presently takes a decided brown colour, and coagulates. The coloration he regards as a purely cadaveric phenomenon. The substance which becomes brown is probably formed in the moment of coagulation, and does not serve in the body as a vehicle between the external air and the tissues, like *hæmoglobin* in Vertebrates and many Annelids, *hæmocyanin* in Crustaceans, &c. When the larva is kept a quarter of an hour in hot water (50° to 55°), the blood extracted does not coagulate or become brown. Once the substance which browns is produced, even a boiling temperature does not prevent its browning. The brown substance once formed is very stable, not being decomposed either by acids or alkalies, and not made colourless by being submitted to vacuum or kept in a closed vessel. The existence of an intermediary in insects corresponding to hæmoglobin M. Fredericq thinks very problematical in view of the anatomical system, letting air penetrate into the heart of the tissues.

NEW PYCNOGONIDA.—The result of the examination of the collection of Pycnogons made during the cruise of the U.S. steamer *Blake* by Edmund B. Wilson, has just been published as No. 12, vol. viii. of the Harvard College Museum *Bulletin*. This collection was found to possess features of considerable interest, and though the species in it were few, some of them

were of remarkable size, quite colossal in comparison with shallow water or littoral forms. Of the three species of Colossendeis, two of which are described as new, the smallest has a span of 14 cm. between the tips of its outstretched legs, while the largest has an extent four times as great. A new genus (Scæorhynchus) has been established for a species with a span of 19 cm., a gigantic size as compared with the dimensions of its nearest allies. The most abundant species of Nymphon is the largest of that extensive genus, and one species of a new genus (Pallenopsis) is more than twice as large as any of the species of allied genera, such as Pallene, which are known only from the littoral zone. It is further interesting to note that in a number of forms the visual organs (ocelli) are either rudimentary and destitute of pigment, or are entirely absent. In Pallenopsis, however, the ocelli are relatively of unusually great size. The species of Scæorhynchus and Colossendeis show clearly from anatomical evidence the complete independence of the accessory legs and the first pair of ambulatory legs, as had been already proved by Dohrn from embryological data. In all cases the palpi and accessory legs are supplied with nerves from the same ganglion, and this latter shows in the adult no indication of being composed of two coalesced ganglia. But Dohrn states that there are in the larvæ of Achelia two ganglia. This question is of great interest, having a direct bearing on the affinities of these Pycnogons with the Arachnids. Mr. Wilson describes ten species, of which one half are given as new, and with figures.

New Zealand Desmids.—As a contribution to our knowledge of the pretty green unicellular algæ known as Desmids, which are to be met with in New Zealand, Mr. Maskell's paper in the recently-published volume of the *Transactions* of the New Zealand Institute is most welcome. It would seem to render more than probable the idea that these minute algæ are to a large extent cosmopolitan. The author is evidently under great disadvantage as to identifying the species he meets with, but this is to a great extent done away with by his fairly careful descriptions and accurate (as to outline) figures. He enumerates between sixty and sixty-five species, some of which are very noteworthy and fine additions to the list of Desmids; thus *Aptogonum undulatum* is a highly interesting new species. *Triploceras bidentatum* is not only a very distinct, but also a very noble, new species, and equally distinct as a species is his *Closterium selenæum*. Doubtless a more prolonged search in fresh localities will enable the author to add many old and new species to the list. He may feel sure that his further researches will be looked for with interest by those working at the freshwater algæ in Europe.

Protoplasm Stained whilst Living.—Mr. L. F. Henneguy publishes the result of some experiments made on living infusoria, in which he confirms the observations of Brandt, made in 1879, that an aqueous solution of aniline brown, known in commerce as Bismarck brown, will give an intense brownish-yellow colour to the protoplasm of the infusoria without in any way interfering with their enjoyment of life. The coloration first appears in the vacuoles of the protoplasm, then this latter is itself stained, the nucleus being most generally not at first coloured, and so being made more conspicuous. Experiments made on vegetable protoplasm seemed to exhibit the same result.

LARGE TELESCOPES[1]

THE small amount of work accomplished with large telescopes has often been the subject of unfavourable comment. This criticism applies with especial force in America, where there are nearly a dozen telescopes having an aperture of a foot or over, besides two of the largest size now in course of construction, and two of twenty-six and twenty-four inches aperture which are unmounted and have been for several years perfectly useless. Among so many it seems as if one might be spared for a trial of the following plan, which, if successful, would produce at a small expense far more work than could be obtained with a mounting of the usual form.

Suppose that the telescope is placed horizontally at right angles to the meridian, and that a plane reflector inclined to its axis by 45° is placed in front of it. This reflector may revolve around an axis coinciding with that of the telescope. Such a mounting has been used in transit instruments, and gives much satisfaction in the meridian photometer of the Harvard College Observatory. The principal difficulty with a large instrument would lie in the flexure of the reflector. This difficulty has however been overcome in a great measure in reflecting telescopes by various ingenious devices. In the present case, since the reflector rotates only around one axis instead of two, the problem is much simplified. A slight motion at right angles of perhaps 5° would be a great convenience, as will be shown below, and would probably be insufficient to materially affect the flexure. It may be said that it is more difficult to make a plane surface than one that is curved. But the principal effect of a slight curvature would be to change the focus of the telescope, the aberration being much less than the effect of the varying flexure. Let us admit, however, that the best definition cannot be obtained, in considering the purposes to which such an instrument could be applied without disadvantage.

Many advantages will be apparent on comparing such a mounting with an equatorial. Great steadiness would be secured, since the mirror would be the only portion moved, and this would be placed directly upon a low pier. Instead of a large and expensive dome which is moved with difficulty, the mirror would be protected by a small shed, of which the roof could be easily removed. It would therefore be opened and ready for use in a very short time, and would quickly take the temperature of the surrounding air. The object-glass would be mounted directly upon a second pier, and, as it would not be moved, would be in very little danger of accident. The tube could be made of tin or other inexpensive material, as its flexure is of no importance. It could easily be protected from the changes of the temperature so troublesome in the tube of a large equatorial. If preferred it might even be exhausted of air, or filled with hydrogen, and the effect of the changes of temperature thus greatly reduced.

The eyepiece could be mounted on a third pier, and would be so far distant horizontally from the mirror and object-glass that there is no reason that it should not be inclosed in a room which may be warmed. The comfort in winter of working in a warm room will be appreciated by those who have used a large telescope in a cold climate. The result is sure to be an increased precision in the observations, and a possibility of prolonging them over longer intervals. A similar effect is produced by the constant direction of the line of sight. No especial observing chair is needed. There is no limit to the size of the attachments which may be made to the eyepiece, since they need not be moved. This is a great advantage in certain spectroscopic and photometric measurements. A strong wind interferes seriously with many observations, as it is impossible to make a telescope so stiff that it will not be shaken by sudden gusts. In the plan here proposed the mirror alone is exposed, and its surface is too small to give trouble.

By means of a long handle the position of the mirror may be regulated from the eye-end, and the declination of the object observed read by small telescopes. If the mirror can be moved at right angles to the meridian 5° from its central position, an object at the equator may be followed for forty minutes, and other objects for a longer period. Without this motion an object may be followed for three or four minutes by moving the eyepiece alone. Clockwork may be applied to the mirror, or less easily to the eyepiece. The focal length may be increased almost indefinitely if desired, and certain advantages will be thus attained in the diminution in the defects of the object-glass, although those of the reflector will not be affected. If the telescope is to be erected at a great elevation the advantages of the present plan are at once apparent. Many nights of observation would be secured which otherwise would be lost owing to the wind and cold. The simplicity in the construction of the building would be a great advantage, as a large dome in so exposed a situation would be kept free from snow with much difficulty, and might be a source of danger in winter storms. If found impracticable to observe during the winter, it would be possible to have a duplicate mounting below, and remove the lens and mirror from one to the other.

It is evident that the saving of cost would be very great, not only in the observatory building and dome, but in the tube, observing chair, clockwork, &c.

If a reflector could be constructed whose surface was the portion of a paraboloid whose abscissa equalled that of the focus, the instrument could be much simplified. No object-glass would then be required, the reflector taking the place both of mirror and lens. All the light intercepted by the objective would thus

[1] By Edward C. Pickering, communicated by the author.

be saved, and but a single surface need be adjusted and corrected. With the advance in mechanical methods this does not seem wholly impracticable, especially with a mirror of long focus. Since the final correction must always be made by hand, it makes but little difference what is the exact form of the surface.

In any case it would be a great advantage that the mirror could be reground, repolished, or resilvered without moving it from its place. It would only be necessary to place it horizontally, and the grinding machinery could be kept permanently near it. If plane, the perfection of its form could also be tested at any time by setting it on edge, and viewing the image it reflected by a collimating eyepiece attached to the large telescope. Another method would be to place a heliotrope a few hundred yards to the north or south of it, and the light from this would form an excellent artificial star, available whenever the sun shone.

The greatest advantage is the rapidity with which observations could be made. No more time would be lost in identification than with a transit instrument, so that a large number of objects could be examined in the course of a single hour. Any one who has worked with a large telescope knows how much time is lost in opening and closing the dome and in finding and identifying minute objects.

Let us now consider to what purposes a large telescope mounted as suggested might be applied.

1. Sweeping. For the discovery of new objects this mounting presents especial advantages. It might be used for the detection of new double stars, of nebulæ, of red stars, or of objects having singular spectra, as planetary nebulæ, banded stars, and variables of long period. Suppose that the field of view had a diameter of somewhat over one minute of time, and that a small motor was attached to the mirror which would move it uniformly over 5° in declination in this time, and then bring it quickly back to its first position. The observer would then have presented to him a series of zones 5° long and one minute wide. The sweeps should overlap by a small amount, so that the entire region could be covered in a single evening. The observer could have a few seconds rest between each zone, while the motion of the mirror was reversed. If an object of interest was suspected, it could be located by merely noting the time at which it was seen. The right ascension would be given directly, and the declination would be found by interpolation from the time of beginning and ending the sweep. An examination of the object and a determination of its exact location should be made on another evening.

2. Measures of position. For many purposes positions could be determined with this instrument as in a transit circle. It would generally be better however to make the measures differential, leaving the mirror at rest and observing the transits of the object to be determined and of two or more companion stars. The method of the ring micrometer might be employed, or some modification of that with inclined lines. In the latter case the zero of position could be found by the transit of preceding stars, by setting the reticule by a divided position circle, or perhaps better by keeping it in a fixed position, determining the direction of the lines once for all, and applying a correction for the declination of the object observed. Stars could be compared differing nearly a degree in declination, as the eyepiece could be moved without danger of disturbing the reticule. For the same reason the star could be followed for three or four minutes, and its transit over a great number of wires observed. It is here assumed that the distortion produced by the mirror is not very great. A slight distortion would do little harm, as it would be the same for all stars of equal brightness. If the stars differ greatly in brightness, the observer should determine his personal equation between them in any case, and the same operation would eliminate the effect of the distortion. The large aperture of the instrument would permit the observation of stars quite beyond the reach of any meridian circle. The faintest asteroids could thus be readily measured, and could probably be followed in many cases on successive evenings to their stationary points. Zones of stars could be observed very conveniently for the formation of charts or catalogues, for the discovery of asteroids, stars with large proper motion, &c.

Probably the definition could not be sufficiently good for the measurement of the closer double stars, but if clockwork was attached, faint companions could be measured, or approximate positions of the coarser pairs determined very rapidly. The positions of nebulæ could also be observed with a view to detecting their proper motion. Stars having a large proper motion might be observed, and the observations so arranged that any very large parallax would be detected. A similar search for a large parallax of variable stars, short-period binaries, minute planetary nebulæ, or stars having singular spectra, might lead to interesting results. The argument that no ordinary star is very near does not apply to such objects.

3. Spectroscopy. The increased dimensions which could be given to the spectroscope, and its steadiness, would compensate in a great measure for a defect in definition. By Zöllner's reversion spectroscope the slit might be dispensed with, and also the necessity of clockwork. So many stars could be observed in a single evening that systematic errors could be in a great measure eliminated, and as the spectroscope would not be moved, we should have a great assurance that the deviations were real. Of the 6000 nebulæ hitherto discovered we know nothing of the spectrum of more than 300 or 400, while the observation of all the others with a large horizontal telescope would not be a very formidable undertaking. It would also be interesting to observe the spectra of all the clusters. It is possible that some may consist of stars having singular spectra, or even of disconnected nebulous masses, in fact forming clusters of planetary nebulæ. The interesting discovery by Dr. Copeland that Burnham's double nebula in Cygnus is gaseous, shows the same tendency to aggregation in these bodies as in stars. Observations of the spectra of all the red stars and variables would also probably lead to interesting results.

4. Photometry. Should the instrument be devoted to photometry numerous problems suggest themselves. Variable stars could be observed near their minimum when too faint to be identified with an equatorial without great loss of time. Faint stars in zones or faint companions to bright stars could be measured very rapidly. The relative light of all the asteroids would be an interesting problem. Many coarse clusters appear to consist of stars of nearly equal brightness. Their light compared with their distances apart might aid our study of their formation. Another useful investigation would be to measure the brightness of all the nebulæ.

In the application of physics to astronomy doubtless many other problems will suggest themselves. Thus no satisfactory results have been obtained in the attempt to measure the heat of the stars with the tasimeter. The use of this instrument would be vastly simplified if it was placed on a solid pier near the ground, was not moved during the observation, and could be perfectly protected from other changes of temperature than those which it was intended to measure.

As either of the problems proposed above would occupy the time of a telescope for at least one year, it is obvious that there could be no difficulty in keeping such an instrument occupied indefinitely.

The horizontal mounting is especially adapted to an elevated position, and would permit the use of a telescope where an equatorial mounting would be quite impracticable. On the other hand, to an amateur, or for purposes of instruction, an instrument which could be set quickly from one object to another, and where the observers need not be exposed to the cold, would offer many advantages. The impossibility of observing far from the meridian would be less important with a large instrument, where the number of objects to select from is very great.

There are certain purposes to which this mounting could not be advantageously applied. The study of close double stars and other objects requiring long examination and very perfect definition could be better left to other instruments. The sun, moon, and planets can also generally be better observed off the meridian. If, however, the entire time of an instrument can be employed to advantage, and it can collect several times as much material as an instrument of the usual form, it is no evidence against its trial that there are certain problems to which it cannot be advantageously applied.

The working force required for such an instrument should consist of at least one observer, an assistant to record, and a number of copyists and computers to prepare the working lists, reduce the observations, prepare them for the press, and read and check the proof-sheets. A large volume of valuable observations could thus be produced every year, which would require at least double the time and money to produce by the same telescope mounted equatorially. The difference in the amount of work will be evident when we compare the number of objects observed with a transit instrument per night, with those observed with an equatorial. A hundred objects in various declinations might be examined in a single evening, while it is seldom that the same number could be identified and measured by an equatorial in a week.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

Tests afforded by the Stars

WE will now see how the views which have been put forward are borne out by the facts which are presented to us by the stars. There is no need to occupy much time, in fact reference need only be made to Dr. Huggins' paper which was communicated to the Royal Society in the course of last year, and with that paper we may compare some earlier writings. It was as early as 1864 that Dr. Huggins, who was then associated with the late Dr. Miller, called attention to the intensely strong lines of hydrogen visible in the hottest stars.[2] In this paper they pointed out at the same time that other metallic lines associated with those lines of hydrogen were thin and faint. It has been already mentioned that, as we have independent evidence that these stars are hotter than our sun, we had strong grounds for believing that here we were in presence of a result brought about by a higher temperature, associated as it was with a simpler spectrum, and, therefore, presumably with simpler constituents. We need not stop now to discuss the objection which has been put forward by an ingenious person ignorant of the facts, that the broadening of these lines may not be due to an increase of temperature at all, but really to a very rapid equatorial rotation of the star. This is a fair sample of one of the classes of objections one has to meet. Of course it is at once put out of court by the fact, also stated by Dr. Huggins, that, associated with the thick lines, are excessively thin lines. Any enormous equatorial velocity of the star should have made all the lines thick, but this is not the fact. Now we have only two lines in the solar spectrum at all comparable in thickness with these hydrogen lines in the hottest stars, taking Sirius and α Lyræ as types.

In a paper communicated to the Royal Society in 1876[1] it was remarked that laboratory work indicated the possibility that line-spectra might, after all, really not result from the vibration of similar molecules; and at that time the evidence seemed to be so clear in the case of calcium that it was pointed out that the time had arrived when evidence touching calcium itself ought, if possible, to be obtained from the stars by means, of course, of photography, because the part of the spectrum in question—the region of H and K—is exceedingly faint in the case of the stars.

Why, it may be asked, was it important to get this evidence from the stars? I will read an extract from a book,[2] published some years ago, which puts this view forth:—"It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms, those that give us line-spectra, are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and, therefore, no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other similar atoms at all lower temperatures. Hence the association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end."

That was one point.

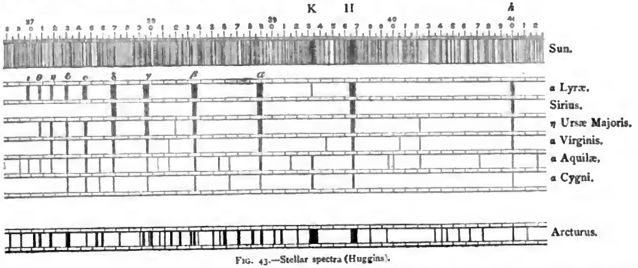


FIG. 43.—Stellar spectra (Huggins).

Here is the next point which made an appeal to the stars so necessary. "We are justified in supposing that our terrestrial calcium once formed is a distinct entity, whether it be an element or not, and, therefore, by working at terrestrial calcium alone we shall never know, even if its dissociation be granted, whether the temperature produces a simpler form, a more atomic condition of the same thing, or whether we are unable to break it up into $x + y$, because in our terrestrial calcium, assuming all calcium to be alike, neither x nor y will ever vary; but if calcium be the product of the condition of relatively low temperature, then in stars hot enough to enable its constituents to exist uncompounded we may expect these constituents to vary in quantity; there may be more x in one star, and more y in another. If this be so, then the H and K lines would vary in thickness, and the extremest limit of variation will be that we shall have only H represented, or x in one star, or only K represented or y in another, and intermediately between those extreme conditions we have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others."

What, then, are the results of this appeal to the stars which Dr. Huggins has made with such splendid success? We have in the hottest stars a spectrum so regular, so rhythmic, that it seems impossible not to consider it as produced either by the same substance or by substances closely allied. Is it by mere accident that some of the *least* refrangible lines coincide with those of hydrogen? is it by mere accident that the most refrangible lines have never been seen except in these stars? One of them coincides with H, one of the lines which still remain thick in the sun, and with which we find a fine line of hydrogen to be coincident. Fig. 43 is a copy of Dr. Huggins' diagram, to which reference has been made. At the top is a portion of the solar spectrum in the violet and ultra-violet, and next is the spectrum of the hottest star, α Lyræ. This spectrum, it will be seen, is simpler even than the spectrum of the solar prominences, and not only is there this wonderful simplicity, but note the exquisite rhythm by which the distance between the lines gradually increases as we go from one end of the spectrum to the other. Note also that the least refrangible line shown on the diagram is coincident with *h* in the violet part of the solar spectrum, and that the next line is coincident with the line H, to which reference has been made in the notes I have read. Note also the relative intensities of the lines H and K in the sun, in which their intensities are about equal, and in η Ursæ Majoris, in which K is altogether absent. These are the first points in this diagram to which attention must be drawn. There will be other points as we proceed further.

But in descending from the general to the particular Dr.

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 370.

[2] "On the Spectra of some of the Fixed Stars" (*Proc.* Roy. Soc., 1864, p. 242).

[1] "Preliminary Note on the Compound Nature of the Line Spectra of Elementary Bodies" (*Proc.* Roy. Soc., No. 168, 1876).

[2] "Studies in Spectrum Analysis," p. 196.

Huggins writes:[1]—"The spectrum of Vega may be taken conveniently as typical of the whole class of white stars, so that the distinctive features of the other stars of this class may be regarded as modifications or departures from this common typical form." He then adds: "There are principally three directions in which changes take place"; one of these consists "in the presence or absence of K, and if present, in its breadth and intensity relative to H." He goes on, "One of these modifications, which possess great suggestiveness, consists of the absence, or difference of character presented by the line K. In all the stars of this class this line is either absent or is very thin as compared with its appearance in the solar spectrum, at the same time that H remains very broad and intense. In the spectrum of Arcturus, a star which belongs to another class which includes our sun, this line K has passed beyond the condition in which it occurs in the solar spectrum, and even exceeds the solar K in breadth and intensity." Arcturus is given in the lower part of the diagram, and it will be seen that there K is relatively thicker than H; and also that with this relative increase in the thickness of K we get a considerable complexity of spectrum, very much more approaching the solar spectrum in the number of lines that we have to contend with. But at the same time I should point out that the positions of these lines vary from the positions of lines in the solar spectrum. "The spectra of these stars," Dr. Huggins continues, "may therefore be arranged in a continuous series, in which first we find this line to be absent. Then it appears as an exceedingly thin line. We then pass to another stage in which it is distinct and defined at the edges; in the solar spectrum it becomes broad and winged, and lastly in Arcturus there is further progress in the same direction, and the line, now a broad band, exceeds in intensity H." Absolute continuity we see in the story which Dr. Huggins brings us with reference to this concrete case of H and K in the details and in general. Well might Dr. Huggins ask after this: "Do these modifications not represent some of the stages through which our sun has passed?" In another part of his paper he uses the term "life changes." Now the life of a star is its temperature, and all these changes must have been produced by the running down of temperature, and I think the simplest view to take, limiting ourselves to the concrete change of H and K, is that the substance which produces K has been formed at the expense of the substance which produced H, and the reason that we see these two lines in calcium when a high temperature is employed is that we reveal the presence of these true root-forms. There may be very many more difficult explanations, but that I think is the simplest one to which one is driven by the logic of facts.

The appeal to the stars, then, I think, amply justifies the prediction which I based upon the comparison of solar with terrestrial phenomena, and, therefore, helps to show that the basis on which those predictions were founded had at all events some little glimmering of truth about it. I think also that it increases the dissociation stages through which we must assume the vapours of our so-called elements to pass when higher temperatures are employed in succession.

So much then for the tests which we have been able to apply to these views by means of Dr. Huggins' remarkably beautiful researches.

The wide departure of stars hotter presumably than the sun (taking the centre of gravity of the absorption, so to speak, as the indication of temperature) from the solar type shows that there is much more work to be done in this field. The success of my former prediction emboldens me to make another one. ***It will in all probability be found that the remaining thick lines in stars of the Sirius type are represented in many cases by the lines brightened in solar prominences.***

Tests afforded by the Phenomena of Fluted Spectra

So far we have dealt with line spectra, but we must not limit ourselves to a consideration of this class of spectra if we wish to test this view to the very bottom, as it is our bounden duty to do. We have therefore to ask ourselves the question with reference to other regions of spectrum analysis beyond that particular part which we have been discussing: Is the evidence to be got from those other regions the same? Does it tend in the same direction as the evidence which has been supplied from the consideration of the highest possible temperature in our coils and in the sun? I have no hesitation in saying that, so far as I know, the evidence is absolutely strengthened by a consideration of the low temperature phenomena observed spectroscopically.

[1] *Phil. Trans.*, 1880.

In fact the view was started very many years ago by observations at much lower temperatures than those we have been considering. Plücker and Hittorf, who worked at spectrum analysis before Kirchhoff and Bunsen, were bound to acknowledge that some of the substances with which they dealt had really two distinct spectra, which they called spectra of the first order, and spectra of the second order, which spectra changed as the temperature they employed changed; and although they came to the conclusion that these simply represented allotropic conditions, not molecular dissociation, I think when one comes to inquire into the subject thoroughly, one will find there cannot be any very great difference between those two considerations. In fact the question of double spectra, which has been fought for many years, but which I think is now nearly at rest, was started by the observation of Plücker and Hittorf. Of course the view they put forward was objected to very strongly, and was met by the assertion that they were misled by impurities in the substances which they experimented on. For instance, they found a second spectrum for hydrogen; this second spectrum, which had a very special character of its own, was referred to acetylene. Soon after, a part of the carbon spectrum which was entirely different from the second spectrum of hydrogen, was referred to acetylene. So that those gentlemen who saw in these phenomena nothing but impurities were perfectly content to give an explanation which would be quite right, provided hydrogen and carbon could only be supposed to have one spectrum; the *impurity* acetylene having two. Later work has shown that it is too *coarse* a view to think that the fluted spectra which represent the spectra of the first order of Plücker really represent the vibration of one molecule in the same way that the line-spectra were supposed to do. Evidence has been accumulated which indicates that in some cases where we get three or four flutings, those three or four flutings which can be seen one by one are inversely intensified, in precisely the same way that various lines can be seen one by one, or almost one by one, and inversely intensified. It seems as if even flutings cannot be considered to be simply due to the result of one special kind of allotropism, but probably represent several.

Let me give a figure or two to represent this point.

An application of the principles referred to in Figs. 32 and 33 will readily enable us to understand that a substance may give us a particular fluting at the lowest temperature, represented by the furnace C, a different fluting in the furnace B, and finally a line at the highest temperature afforded by the furnace A, as shown in Fig. 44, and that at intermediate temperatures its spectrum may consist of mixtures in varying proportions of each of these constituents, and it will also be seen that the line produced by the highest temperature can never be seen together with the lowest temperature fluting, unless the fluting produced by the intermediate temperature is present also. Fig. 45 shows the facts actually observed when the spectrum of carbon is photographed under various conditions of temperature.

The results are strikingly suggestive, as we have a compound origin to the two sets of flutings shown.

But there is a lower region yet, a region in which much work has been done which seems to show that before the substance is fit to give us flutings, that it can still record for us—in a very feeble sort of way—it can yet record for us its vibrations by absorption at one end of the spectrum or at the other, so that the story of simplification is really intensified when we leave the high temperature spectrum, and it seems as if the first effect produced by the action of heat on any substance is to give us general absorption which breaks up into absorption in the red and absorption in the blue, and then we get a series of flutings more or less complex according to the temperature of the body; and then when we have passed from this stage we get the series of line spectra to which I have drawn attention. Again, in passing from a low temperaure to a high temperature, so far as I can see there is absolutely no break, nor is there any difference of kind that we are acquainted with in the passage from a compound body and the passage from a known form of, let us say, gold or silver, at a low temperature, to that same substance at a higher temperature.

If we assume that these various spectra are really due to different molecular aggregations, we shall have the following series, going from the more simple to the more complex:—

First stages of complexity of molecule	Line-spectra.
Second stages...	Channelled space or fluted spectra.

Third stage	Continuous absorption at the blue end not reaching to the less refrangible end. (This absorption may break up into channelled spaces.)
Fourth stage	Continuous absorption at the red end not reaching to the more refrangible end. (This absorption may break up into channelled spaces.)
Fifth stage	Unique continuous absorption.

So that the story is one of absolute conformity, absolute continuity from one end of the series to the other; but on this subject I need not say more, because my friend Capt. Abney will have a great deal to say about the red molecules and blue molecules when he comes to deal with the red end of the spectrum, and I may safely leave this part of the subject in the hands of one who has so brilliantly distinguished himself by his investigations upon it.

Replies to Objections

Now I think it is time that I should reply, or attempt to reply, to some objections that have been made to these views. So far as I can gather, the serious objections which have been made are not many, but some of them are objections to which considerable value should be attached. The chief one now urged is that one is misled in the conclusions that one draws from these observations of spots and storms by the fact that the solar lines corresponding with the lines which we consider to be common to two substances are really double, and that the lines common to two substances appear common simply because we have not sufficient dispersion to separate them. Now that is a very important objection indeed, but let us examine it. It has been pointed out that of the 62 iron lines which remained as the result of the purification of the first part of the map (between wave-lengths 39 and 40) only 18 were left; all the rest being found common, not only to two substances, but in a great many cases to four or six substances, and we found also that our rough observation-book, as we went on, suggested that the solar line was double; but if we had gone on in that way we should not have been able to produce a map at all, because there would have been few lines which were not complex, so that it would have been a piece of cowardice to remain there and not attempt to find out what it meant. Now let us suppose a great many of the solar lines are double. It is fair to assume that these double lines would be irregularly distributed throughout the spectrum. We cannot imagine some spiteful freak of nature choosing out to be double a particular set of solar lines which some one should eventually find common to two substances; we must imagine an impartial distribution of double lines if we are to consider them as double. Now the argument we can bring against that is founded on this, that two things always hang together, the alleged complex nature of the line and the fact that this particular line is picked out for special prominence in spots and storms. For instance, take the line 4923·2 referred to in Fig. 41. If that line is double, and is one of two lines seen in flames, the probability that that line should be double, supposing that the solar line is double, would be as 2 to 1, but that line is picked out as 50 to 1 in the case of prominences. The betting in favour of the coincidence is not so great in the case of the spots, but when we come to the prominences, when we are dealing with 100 lines with the probability therefore of 50 double lines if they are equally distributed, and that every other line is double—when we come from 100 lines to 1, it is 50 to 1 against this particular line being double; and yet this is the particular line which we always find to be common to two substances when we really discuss the observations of the flames. I think then that the idea that these basic lines are simple creatures of the imagination, simply chance coincidences, will really not stand at all.

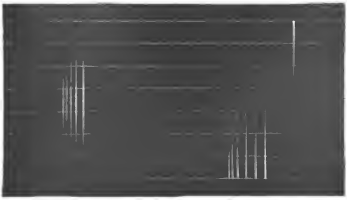


FIG. 44.—Diagram showing the action of three different temperatures on a hypothetical substance assuming three stages of complete dissociation; and also of intermediate temperatures at which the vapours are only partially dissociated.

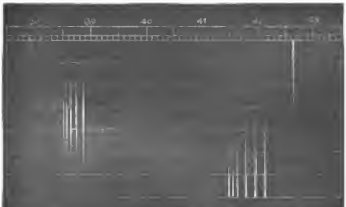


FIG. 45.—The photographed spectra of some carbon compounds.

Prof. Young has lately brought forward this objection, although in 1872 he was the first to point out the very extraordinary fact as it appeared to him, and as it still appears to every one else, that an enormous number of coincident lines which he got from the tables extant at that time seemed to cluster round the bright lines seen in his observations. The credit of that is

undoubtedly due to Prof. Young, and although he has lately seen cause to withdraw somewhat from this first view of his, he is still driven to think that two of the most important lines in the solar spectrum, H and K, are really due to two substances—hydrogen and calcium. There is one more thing which must be said with special reference to this: the work is not to be negatived by a mere assertion that the line is double; it must in a great many cases be shown that it is a double-double or a treble-treble line. For instance, take the case of *b*4, which may very well be double for aught we know; that line is coincident, so far as we can make out with the means at our command, with magnesium, iron, nickel, uranium, chromium, and cerium. It will not do to limit oneself to the statement that these lines are double—they must really be perfect families of lines in order to prevent this explanation being, as I think it is, the more probable.

Another objection, again, a very important objection, is of a somewhat different nature, and may shortly be called the bell hypothesis. It is to the effect that these molecules, or atoms, are very extraordinary things indeed, as we can well imagine them to be, and that the spectrum which they produce depends entirely upon the manner in which they are struck; so that in fact it would seem at first useless to construct a map of any spectrum at all, for fear the substances we wish to observe in our laboratories should be struck in different ways and should render the map perfectly useless. I say the idea is really that the molecules struck differently would give us different spectra. Now if the difference were only slight, that would not much matter; it would be very difficult to withstand that hypothesis. But it must be remembered that in this work we are dealing with this extraordinary fact, that over the region which we have been specially studying there are no lines of iron common to spots and flames; so that if we had not any iron at all to experiment with we should be perfectly justified in asserting the iron lines in flames were produced by one substance and those in spots by another, because no two lines agree in *these* two spectra. The spectra are as distinct as the spectrum of magnesium and the spectrum of iodine, or any other two bodies. Now if the bell hypothesis is to explain that, it explains too much, because if it is true of any two bodies, it must be true of all bodies, and therefore all spectra are the result of the same thing struck differently, and spectrum analysis would then cease to be spectrum analysis, for it would simply record changes rung on the same molecule by the various methods of striking. Then again there is another thing to be said, that no statement of this bell hypothesis which I have heard gets us out of the difficulty that we are sinning against the law of continuity in advancing it, for the reason that if you begin with a known compound body, let us say, a salt of calcium, the change from a salt of calcium to calcium is the same in kind and about the same in degree as the change from one form of calcium to the other, if we can talk of different forms of calcium on the mere strength of spectroscopic work. I mean that there are more important changes to be got out of the observations of the metal calcium than there are to be got by passing from a salt of calcium to calcium; so that if the bell-hypothesis proves anything it proves that a compound body is a simple one.

It will be seen that the special import of these considerations lies in the question of the short lines; leaving all considerations dependent on long lines, by which the presence of impurities may be recognised, out of consideration altogether. But it may be further said that a method of purifying spectra and eliminating any spectroscopic defects which were due to the presence of impurities has been before the scientific world for some years, and so far as I know its validity has never been called in question. Where that method has been employed I believe it has been employed honestly, so that until that method is called in question by better work—and the work I know will be severe—I think we are bound to accept the results which have been obtained by it.

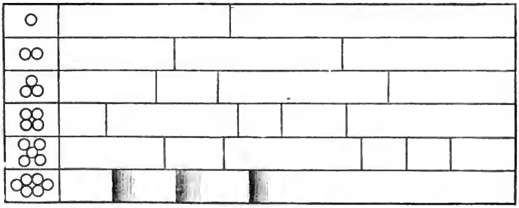


FIG. 46.—Diagram showing how the evolution of chemical forms may be indicated by their spectra.

Then there are some theories which I might be permitted to say a few words about, but in reference to them I need only call attention to Dr. Schuster's admirable report on the progress of spectrum analysis, given to the British Association at the meeting at Swansea. It will be seen from that report that none of the theories which have been put forward really account for all the facts observed. It is shown that phenomena have been recorded which are not to be explained away by the bell theory or any other. Such a phenomenon, and a perfectly distinct one, is the change due to the thickness in the vapour. Changes also due to varying temperature and other causes have been seen, for which the theories in question do not account. Now changes in temperature may probably affect large reaches of the spectrum, but in the cases we have studied we got most diverse effects in lines so close together in the spectrum that it requires a considerable amount of dispersion to find out that they are really not single lines.

The New Theory of Chemical Evolution

What then is the view of the evolution of chemical species to which we are led by our study of the sun and stars? I think that after all it is but a slight expansion of the present chemical view. Chemists regard matter as composed of atoms and molecules, about which more presently. The view now brought forward simply expands the series into a larger number of terms, and suggests that the molecular grouping of a chemical substance may be simplified almost without limit if the temperature be increased. A diagram (Fig. 46) will show exactly what I mean. If we assume a very great difference in the temperature which can be brought to bear upon a substance we may assume that at the highest temperature we have, for simplicity's sake say, a certain line represented by a single circle; let us imagine the temperature reduced, we shall then get another spectrum, which we can represent by a double circle, if we like to assume that the evolution is one which proceeds by constant additions of the original unit. Coming lower down, we get another substance formed with a more complex spectrum represented by three circles; lower down still we have one represented by four circles, another by five, another by six, and so on. We might take another supposition, easier perhaps to some minds, and suppose that evolution proceeded, not by the addition of the initial unit, but by the constant doubling of the substance of the molecule itself. Instead, therefore, of our circles increasing by one, we shall have one, two, four, eight, sixteen, thirty-two, and it will be readily understood that if there are a considerable number of stages of temperature, both within our ken and beyond our ken, and if some substances form themselves perpetually by doubling, then the unit with which we can experiment at low temperature, call it the chemical atom or the chemical molecule, or what you will, must be a very complex thing indeed. If the lower spectrum represents that of a complex

body such as iron, or a salt of calcium, the upper spectra will represent those due to the finer groupings brought about by higher temperatures. We pass continuously, as in the sun and the stars, from complexity to simplicity, if we begin at the lower stages, and from simplicity to complexity if we begin at the higher stages of temperature.

Now, two questions arise here which I think it is important to discuss. Are we playing fast and loose, in such an hypothesis as this, with the ordinary course of nature's operations, or are we in harmony with her? Again, is it contrary to the view expressed by the greatest minds which have studied chemical phenomena? I think really the view is not inharmonious with those other views which we have gathered from other regions of thought and work; in fact, I think it derives its whole force from the fact that along many lines it runs parallel with the evolutionary processes in the different kingdoms of nature. I have another diagram which will show what I mean (Table I.). This diagram deals with a very simple case of evolution, and it deals with this evolutionary process, going along a single line. Of course we know very well that in the organic kingdom evolution always proceeds along many lines, but to simplify the problem I have dealt with one of the simplest that I can think of. Let us assume that in a certain hottest star there shall be two substances, which we will call a and b. They will first at the transcendental temperature which I assume, exist as separate entities; the temperature being then reduced, they probably will combine, and, instead of two atoms, a and b, we shall have one group of $a + b$. If the temperature is still further reduced, we shall get b combining with b; in that case we shall have a grouping consisting of $a + 2b$. Let the same operation be performed again, we shall then have $a + 4b$, combining into two groups of 2; we shall have what we can represent, in short, in chemical language ab_4. Now, having got our ab_2, having got our temperature reduced, let us assume that ab_2 is now the substance linked on to give a greater complexity, instead of b or $2b$ merely. We then have this series given in the table.

TABLE I.

$a \quad b$
$a + b$
$a + bb$
$a + (bb)(bb)$
$= \ldots ab_4$
$+ ab_2 = \ldots\ldots a_2b_6$
$+ ab_2 = \ldots\ldots a_3b_8$
$+ ab_2 = \ldots\ldots a_4b_{10}$
$+ ab_2 = \ldots\ldots a_5b_{12}$
$+ ab_2 = \ldots\ldots a_6b_{14}$
$+ ab_2 = \ldots\ldots a_7b_{16}$
$+ ab_2 = \ldots\ldots a_8b_{18}$
$+ ab_2 = \ldots\ldots a_9b_{20}$
$+ ab_2 = \ldots\ldots a_{10}b_{22}$
$+ ab_2 = \ldots\ldots a_{11}b_{24}$
$+ ab_2 = \ldots\ldots a_{12}b_{26}$
$+ ab_2 = \ldots\ldots a_{13}b_{28}$
$+ ab_2 = \ldots\ldots a_{14}b_{30}$
$+ ab_2 = \ldots\ldots a_{15}b_{32}$
$+ ab_2 = \ldots\ldots a_{16}b_{34}$

Now, that is an ideal scale. The question is, Is it absurd? How can we honestly answer that question? By asking whether we are or are not on the lines on which nature works in the region of the known, in the region which we can get at?

TABLE II.

$C \quad H$
$C + H$
$C + HH$
$C + (HH)(HH)$
$= \ldots CH_4$
$+ CH_2 = \ldots\ldots C_2H_6$
$+ CH_2 = \ldots\ldots C_3H_8$
$+ CH_2 = \ldots\ldots C_4H_{10}$
$+ CH_2 = \ldots\ldots C_5H_{12}$
$+ CH_2 = \ldots\ldots C_6H_{14}$
$+ CH_2 = \ldots\ldots C_7H_{16}$
$+ CH_2 = \ldots\ldots C_8H_{18}$
$+ CH_2 = \ldots\ldots C_9H_{20}$
$+ CH_2 = \ldots\ldots C_{10}H_{22}$
$+ CH_2 = \ldots\ldots C_{11}H_{24}$
$+ CH_2 = \ldots\ldots C_{12}H_{26}$
$+ CH_2 = \ldots\ldots C_{13}H_{28}$
$+ CH_2 = \ldots\ldots C_{14}H_{30}$
$+ CH_2 = \ldots\ldots C_{15}H_{32}$
$+ CH_2 = \ldots\ldots C_{16}H_{34}$

We will now refer to another diagram; we will pass from the ideal to the concrete, and it will be seen that there is, if one can invert the term in such a way, a distinct precedent for such a table as the last; for here are the absolute facts with regard to one evolutionary series of the combination of carbon and hydrogen. We have the gases CH_4, C_2H_6, C_3H_8; we have as liquids from C_4H_{10} to $C_{15}H_{32}$; each of them formed, not by the addition of my hypothetical ab_2, but by a concrete CH_2, and we have connected with that a beautiful order and exquisite regularity in the way in which the boiling-points and specific gravities of these things increase.

TABLE III.—*Hydrocarbon Series*

			Boiling point.	Specific gravity.
Gaseous	CH_4	Marsh Gas.		
	C_2H_6	Ethane.		
	C_3H_8	Propane.		
Liquid	C_4H_{10}	Tetrane or Diethyl ...	1° ...	·600 at 0°
	C_5H_{12}	Pentane	38 ...	·628 at 17
	C_6H_{14}	Hexane or Dipropyl..	71 ...	·669 at 16
	C_7H_{16}	Heptane	99 ...	·699 at 15
	C_8H_{18}	Octane	124 ...	·726 at 15
	C_9H_{20}	Nonane	148 ...	·728 at 13·5
	$C_{10}H_{22}$	Decane	166 ...	·739 at 13·5
	$C_{11}H_{24}$	Endecane	180 ...	·765 at 16
	$C_{12}H_{26}$	Dodecane or Dihexyl.	202 ...	·774 at 17
	$C_{13}H_{28}$	Tridecane	218 ...	·792 at 20
	$C_{14}H_{30}$	Tetradecane	230 ...	—
	$C_{15}H_{32}$	Pentadecane	258 ...	·825 at 16
Solid ...	$C_{16}H_{34}$	Hexdecane or Dioctyl	278 Melts at 21°	

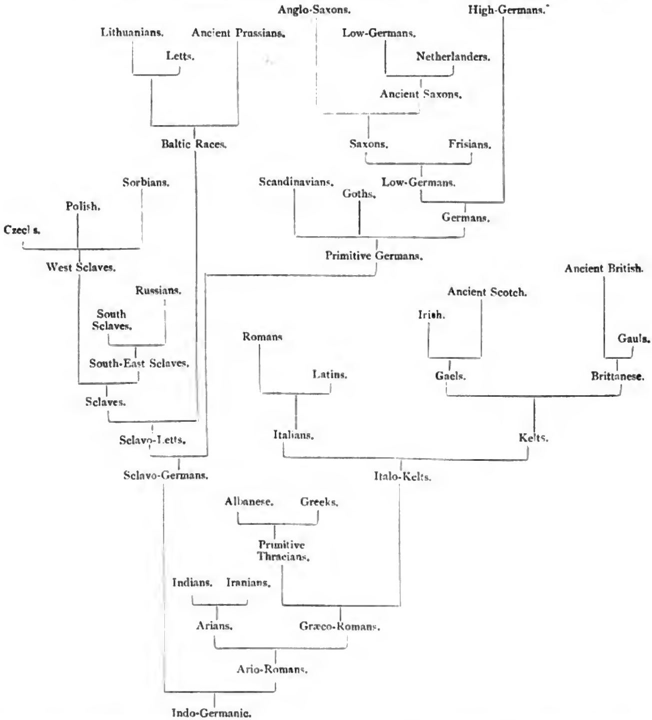
There is no break in the general line of increase, and after we have gone through the gaseous stage, which stops at C_3H_8, and through the liquid stage, which stops at $C_{15}H_{32}$, we get the solid state, and there again the same series is represented. So that I think one is justified in saying that, dealing with this one simple case (and the only reason I have taken the simple case is that it is a line which has been thoroughly worked out by organic chemists), taking this simple case we are justified in saying that if nature, in the regions which we cannot get at, works in the same way as she does in the regions which we can get at, the view is not absurd, and in fact any one who wishes to dispute the view in such a case as this has, I think, the *onus probandi* thrown upon him. He must show that either in a certain latitude or longitude, or at a certain temperature, or under some unknown condition the laws of nature are absolutely changed, and give place to new ones. That has not yet been found in any other region of natural philosophy. Indeed I think one might go further and say that all these evolutionary processes, obtained from different regions of thought, have such a oneness about them that to my mind one of the best mental images we can get of the causes which produce the lines picked out for special prominence in solar spots and solar flames, is to consider the molecular groupings that produce them as resembling the roots of the present European languages which our ancestors brought from the cradle of the race in Asia.

The accompanying diagram (p. 396) is taken from Haeckel's book. We begin with the European languages, including our own, High-German, and so on, and philologists have traced them down into Indo-German. We have at present in our language few and far between, the root-words which have existed almost unchanged from the time when this language only was spoken. There has been an evolution in language as there has been an evolution brought about by the reduction of temperature wherever chemical substances have been allowed to intermingle with each other, but side by side with the new chemical forms produced by the low temperature, and represented by the later languages, remain true root-forms, which may be traced to the hottest regions within our ken in the same way as the root-forms in our own language can be traced through these other forms of language back to the first one with which we are acquainted.

Now comes the second question, to which reference has been made. What is the opinion of those who have given the greatest attention to chemical philosophy? I do not mean to chemistry, I mean to chemical philosophy. We begin with Dalton. He says, "We do not know that any one of the bodies denominated elementary is absolutely indecomposable." I go on to Graham: "It is conceivable that the various kinds of matter now recognised in different elementary substances may possess one

and the same element or atomic molecule existing in different conditions of movability. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies." I now come to Sir Benjamin Brodie, whom we have so lately lost, the last of the great English triumvirate to whom I mean to refer. His views I have already stated in his own words, but I may here again state that as early as 1867, almost, we may say, before the spectroscope had been applied to the sun, except in the general way, which was started by Fraunhofer and Kirchhoff, he prophesied that the solar facts would be as I think we have found them. That is to say, he stated his belief that at the solar temperature the constituents of our elementary bodies would be found existing in independent forms. The greatest chemical philosopher now living, M. Dumas, so

PEDIGREE OF THE INDO-GERMANIC LANGUAGES.



long ago as 1836 published a series of lectures in which his views were very clearly stated indeed, and any one who reads them will see how convinced he was then of the considerable amount of evidence that had already been accumulated in favour of the non-elementary nature of a great number of substances then classed as elements.

Then again we can pass to another chemical philosopher, Kopp. In his researches on specific heats he also gives evidence to show that that relationship is not to be depended upon to establish the received view. If, then, the three greatest English chemists we can name, and the most eminent chemical philosophers in France and Germany, give their opinion in behalf of the compound nature of the chemical elements, can these simpler forms be any other than those we detect by means of the spectroscope? By the conditions of the problem and the absence of knowledge they are not decomposable in the laboratory; if they

were they would cease to be elementary bodies at once, and would be wiped out of our tables. Nor do I think it possible that in the present stage of our knowledge they can be revealed to us in any other way than by the spectroscope. It is unfortunate that none of these chemists who have given us this view have helped us by showing in what way the possibility, which all of them suggest, and which many of them intensely believe in, could be absolutely demonstrated, but it is obvious that it dissociation is the thing which time out of mind has made compound bodies simpler, in their minds the condition of higher temperature must have been present. The only difficulty was the way in which the effects of that high temperature could be measured and weighed, and I think that if the spectroscope had been introduced earlier they would probably have left some hints behind them which would have been of the greatest value to those who work with that instrument.

Passing from the chemists to the physicists, there is one, at all events, who has appreciated exactly how this decomposibility of the terrestrial elements could be established. I refer to the lamented Clerk Maxwell. In his article on atoms in the "Encyclopædia Britannica," he says: "The discovery of a particular line in a celestial spectrum which does not coincide with any line in a terrestrial spectrum indicates either that a substance exists in the heavenly body not yet detected by chemistry on earth, or" (and it is to the "or" I wish to draw attention) "that the temperature of the heavenly body is such that some substance undecomposable by our methods is there split up into components unknown to us in their separate states." Absolutely nothing could be clearer than this.

In endeavouring to discuss the bearing of this application of the hypothesis of evolution of chemical forms upon modern chemistry, we must draw a very wide distinction between chemical theory and chemical fact.

When we compare the laws given in average chemical text-books with the laws which lie at the root, let us say, of astronomy, the candid mind cannot fail to be struck by the difficulty which chemists must have encountered in endeavouring to reduce the facts of their science to order on the hypothesis they bring before us. An outsider, for instance, thinks that the basis of chemistry, or a large part of the basis of chemistry at all events, lies in the fact that the chemist has determined the existence of a certain number of elementary bodies, each of these elementary bodies having a certain atomic weight, and that this atomic weight determines all the constants of that body. Yet we read in chemical text-books that this atomic weight is fixed according to no invariable rule; indeed, with Kepler's laws and Newton's laws in one's mind one comes to the conclusion that it is not too much to say that it is determined by a series of compromises. An outsider would think that if any one of these elementary bodies were taken as a standard, the weight of an equal volume of vapour of another substance under equal conditions would bear some relationship of a definite character to the atomic weight. This however is not the case. Again, among the questions to be considered as determining the atomic weights taken, is an assumed limitation of combination power, a so-called atomicity, according to which one substance is a monad, because it will combine with that same relative proportion of hydrogen which exists in half a water-molecule. Another substance is called a dyad, because it will combine with the same relative proportion of hydrogen which exists in a whole water-molecule, and so on. When we thus begin to class the substances into monads, dyads, hexads, and so forth; in fact, when we thus effect a re-classification of elementary bodies, the solidarity at once breaks down; we find that the classification after all is useless, because the same substance may behave as a dyad, a tetrad, a hexad, a pseudo-tryad, a pseudo-octad; in fact, one feels one is dealing with something that is more like a moral than a physical attribute—a sort of expression of free will on the part of the molecules. We are, I think, justified in asking whether these various attempts to formulate a science do not break down after a certain point, because they attempt to give a fixity to what is in truth variable.

When we pass to the *facts* of the science, the key-note of which is variability from one end of the scale to the other, we find that the view of successive dissociations, the view of variable molecular groupings brought about under different conditions, is really more or less in accordance with the facts where the laws based on the fixity of the facts break down entirely. Thus, for instance, let us take the question of vapour densities. The view accounts fully for the so-called anomalous vapour densities, and in this way: it suggests that the elements may really be complex groups which break up into their constituent groups under suitable conditions of temperature, like phosphoric chloride and many other bodies do when obtained in the condition of vapour. We have dissimilar groups in the one case, and possibly similar groups in the other. In this way, that contradiction in terms, the "monatomic molecule," really becomes the evidence of a higher law.

Let us pass to allotropic conditions. The explanation of these is that there are bodies which have a large molecular range within the ordinary temperatures at our command. The substances in which allotropism is most marked are all metalloids which have not been found in the sun, and the allotropic forms give us in many cases different spectra—spectra indicating a considerable complexity of the molecules which produce them—spectra of continuous absorption, continuous radiation in the blue, continuous absorption in the red, fluted spectra, and the like. In the passage from one allotropic condition to the other, energy, without any known exception, is absorbed or given out. What becomes of this energy; what is it doing; unless it is in some way or other controlling the passage from one molecular group to another? These allotropic conditions, occurring very obviously to us in certain limits at our ordinary temperature and pressure are, I hold, but special cases of group-condensation common to all bodies, represented by Dalton's law of multiple proportions. We can indeed imagine a condition of things in which the difference between iron in Fe_6, and the iron in Fe_2Cl_6, would be as obvious as the difference between ordinary and amorphous phosphorus.

In certain classes of so-called organic substances this grouping of simpler groups to more complex actually takes place, and is recognised under the term polymerism—for instance, with cyanogen compounds of oxygen we have a simple thing like CNO say, which will form a series of compounds, and we have its so-called polymers, $C_2N_2O_2$, or $C_3N_3O_3$, which will each form a series of compounds, these groups of simpler nature forming by their combination group individuals with related but not identical properties with the simplest or fundamental group.

In many cases the amount of this condensation may be determined by the vapour densities. In others, again, a dissociation takes place at a certain limit of temperature, a simpler or fundamental group being the resolution product.

The resemblance between these cases of polymerism and especially those elementary bodies which exhibit allotropism, is at least striking.

In the one case, the organic complex bodies, the range of existence is in most cases within our easy attainment; in the so-called elementary stuffs it is less frequently the case. We can certainly convert ordinary phosphorus and sulphur into allotropic and most likely polymeric forms, but we do not know as yet how many atoms more are contained in the polymeric forms of these substances than in their simpler states.

And in other substances this range of condition of formation passes gradually out of our reach, but the phenomena are the same in kind up to the temperature of the sun. And again, when we can obtain the spectra of bodies like amorphous phosphorus we can prophesy that the relative grouping of the atoms of phosphorus in this to the ordinary form will be exhibited.

This brings us to the next point—atomicity. What are the associated phenomena? Lowest melting-point, simplest spectrum, lowest atomicity. Therefore we are justified, I think, in assuming that atomicity may after all be but the measure of the molecular groupings at work. In this way we can associate various atomicities, not with moral phenomena as regards the "*behaviour*" of the same molecule, but with different physical states—different complexities of the same substance. Thus in the same substance the more complex or allotropic the molecular grouping, the higher the atomicity. Hence the substances in which the highest atomiticities appear should, as a rule, be formed and broken up at the lowest temperature. This, I am informed, is really what happens in the majority of cases.

New Analogies between Organic and Inorganic Bodies

I have ventured in these few remarks to touch upon the relations of the new view to modern chemical facts, because I think such a discussion shows us that there are several chemical regions in which the views can be tested from a chemical point of view, although I have, from a set purpose in my lectures, dealt with them absolutely from the physical side. In fact, one such

step of the highest interest has already been taken by my colleague, Capt. Abney. I will read what it is, and the language of Prof. Roscoe, the President of the Chemical Society, is so clear and so admirably put, that it is impossible for any one to improve upon it.[1] Referring to the work which Capt. Abney and Col. Festing have done together, he says: This work "is no less than a distinct physical test of the existence in organic compounds, of the organic radicals, and a means of recognising the chemical structure of an organic compound by means of the spectroscope." This result "is accomplished by photographing the absorption spectra of organic compounds in the infra-red part of the spectrum. In these invisible portions characteristic and distinct absorption lines and bands occur for each organic radical. The ethyl compounds all show one special ethyl band; the methyl compounds a special methyl band; and thus, just as a glance at the luminous portion of the spectrum satisfies us of the presence of calcium, lithium, and rhubidium, so a simple inspection of these infra-red photographs enables us to ascertain the presence of the various organic radicals. This invention is still in its infancy, but one of greater importance to chemists has seldom if ever been communicated to the Society." I have been the more anxious to give these results in Prof. Roscoe's own words, because it will be seen that, *mutatis mutandis*, these remarks touching the spectra of organic radicals are precisely the statement I have been endeavouring to make with regard to inorganic radicals. It cannot therefore be said that the nature of the principle I bring forward is one with which chemists are not familiar. In this beautiful work, then, we have an analogy between the behaviour of known compounds and assumed elementary bodies.

A new method of laboratory work which I have recently started may, I think, in the course of time furnish us with another analogy, and in connection with it there is an experiment to which reference may advantageously be made, because it will show what kind of results we expect to get. It is simply referred to as an indication of the probable fruit which will come from many new kinds of experimentation which might be adopted, provided always we bear in view the idea which it has been my duty to bring forward. This experiment is founded on the behaviour of compound bodies when they are distilled at different temperatures. If we take, for instance, a mixture of hydrocarbons, some of them very complex in their nature, and others more simple; when a low temperature is employed it is found that the simpler hydrocarbon comes over in the shape of vapour. If therefore we were fortunate enough to be able to observe the spectra of these different vapours, assume that that series of hydrocarbons, for instance, shown in the accompanying diagram (Fig. 47), had each of them a distinct spectrum, we should be able to follow spectroscopically the effect of each change of temperature, and we could in that way associate the known fact of the greatest density of the vapour which comes over at a higher temperature with a spectrum of a certain kind.

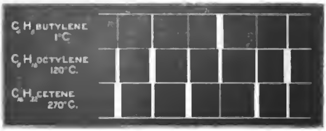


FIG. 47.—Hypothetical spectra obtained on distilling at successively increasing temperatures a mixture of light and heavy hydrocarbons.

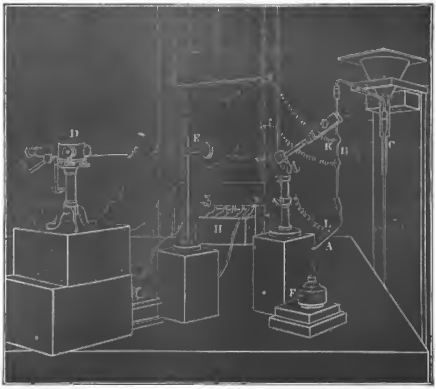


FIG. 48.—Fractional distillation of potassium. A, hard glass tube containing the potassium and connected with a Sprengel pump C by a tube B, having two bulbs with platinum electrodes sealed into them, between which an induced current may be made to pass; F, spirit lamp; H, battery; D, spectroscope; E, lens to focus image of spark on slit of spectroscope.

Now in our experiment we deal not with a compound body in the ordinary sense, but with the so-called elementary body, potassium, which we have in a hard glass tube of the form shown in Fig. 48. By means of a Sprengel pump the tube is very perfectly exhausted, and then gently warmed with a spirit lamp, the exhaustion going on during the whole process. On passing a current between the platinum electrodes we see a beautiful green glow in the tube, and obtain a certain spectrum. On replacing the spirit-lamp by a Bunsen burner we find as the result of this increased temperature that the colour in the tube is blood-red, and the resulting spectrum is entirely different. The spectrum of potassium is one which requires a very great deal of study, for the reason that it varies very much under different experimental conditions. If the

[1] *Journal* of the Chemical Society, May, 1881.

potassium, for instance, is thrown into a Bunsen burner, the chief line that one gets is a red one. Kirchhoff, in the early days of solar chemical investigation, pointed out that this red line is not to be found among the Fraunhofer lines. The flame also gives us a line in the blue. If we examine the spectrum of potassium by means of an induction-coil we find the blue line which we also see in the flame, but it is intensified in the spark. We also see some strong lines in the green and yellow, which are barely visible in the flame—which are in fact not generally recorded in the flame spectrum of potassium, although they are really visible when considerable dispersion is employed. These lines in the yellow and green I say become prominent lines. Now, it so happens that some of these lines in the green do, it is believed, correspond with Fraunhofer lines, and we are, therefore, justified in assuming that they represent a something, whatever it may be, in the potassium, which can withstand the heat of the sun, while the red lines represent something which

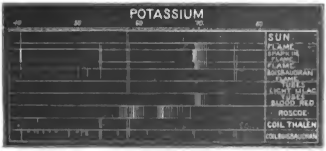


FIG. 49.—Spectra of potassium obtained under different conditions.

is broken up at the temperature of those regions of which we can determine the absorption. The interesting point of the experiment, therefore, is this: assuming for a moment that the red line does represent a complex something which cannot withstand the temperature of the sun, and that the yellow line represents a something finer which can withstand the temperature of the sun, what happens when we try to drive off the vapour of this potassium *at the lowest temperature* at which we can get it to volatilise at all, is that if the experiment is carefully performed it gives precisely those lines which are reversed in the solar spectrum alone, and of that line, which is the strongest line at the temperature of the Bunsen burner we see absolutely nothing at all. Referring to the spectrum which we get in the lilac and yellow-green part of the tube, two out of the three lines visible at all events are seen in the sun, whereas the other lines which we get in the flame and some of them which we get in the coil are not represented in that fine vapour which was produced at the lowest possible temperature.

The Bunsen burner produces some very exquisite colour-effects in the tube, and especially develops a beautiful blood-red colour which might be imagined to be the product of that molecule which gives the red line in the Bunsen burner; but that is not the fact. The line seen in the Bunsen burner is not visible as a rule in the vapour when heated in this way, the lines actually seen being more refrangible. Fig. 49 is a map of the spectrum of potassium under these various conditions. I give it simply as an indication that it is possible when other laboratory and chemical experiments are made with this view in mind that other analogies than those already obtained will be forthcoming.

The experiment then comes to this. If we assume the potassium to be a compound body and that its finer constituent molecules are those which resist the solar temperature, then it behaves exactly like a mixture of hydrocarbons is known to do, that is, the finer vapours come off in greatest quantity at the lowest temperature, and the more complex ones as the temperature is raised.

Conclusion

In concluding my lectures in this course on Solar Physics I would ask attention to the fact that the views which I have ventured to put forward, as being what I honestly believe to be the true outcome of the twenty years' work which has been applied to this subject, depend for their strength upon the convergence of very various lines of thought and work. No doubt the future progress of science will show that we, after all, are looking through a glass darkly, and that we are not yet face to face with the truth, and the whole truth. We must all of us be content to have our work criticised and expanded by future work, by researches carried on with greater skill, with more elaborate methods and higher views. But with all these reservations I do wish to draw attention to the fact that the convergence of many lines of work and many lines of thought suggest the ideas which I have put forward. Depend upon it, that we shall get a much higher and much richer truth out of further inquiries; and I quite acknowledge, although I have had a hand in the work myself, that the outcome of the work is so important that it ought to be considered honestly and carefully from every point of view. Still I consider that I am in honour bound to say, as the result of the work on solar physics, in that small branch of the inquiry into solar matters with which I am more personally connected, that my belief is that the late work has changed the views which were held say twenty years ago to this extent: whereas twenty years ago we imagined ourselves to be in full presence in the sun of chemical forms with which we are familiar here, I think in this present year we are bound to consider that that view may be modified to a certain extent, and that we are justified in holding the view that, not these chemical forms, with which we are acquainted here, but their germs really, are revealed to us in the hottest regions of the sun.

J. NORMAN LOCKYER

NOTES FROM THE MALAY ARCHIPELAGO

A CORRESPONDENT in Java sends us the following:—

In 1879 I saw, at Tabu Breio, Padang Panjang, west coast of Sumatra, a child aged about one and a half years, with four legs. It was a female child with perfect organs, only the feet were clubbed and the legs bent. The added-on pair of legs were less perfect and their circulation evidently not in order, for they were not so sensitive to pain (pinches, &c.). They looked as if part of an embryo male child. The child was subject to fits; it could not walk, but crawled, using its female legs, the male (?) legs being dragged along. The spine was much dragged out of position.

During about six months of 1880 there was a child at Surabaya, Java, with two distinct heads joined to one neck. It is now with the Regent (a Javanese) of Surabaya, in spirits. Photographs of this are sold. The brains were quite independent of each other, for the one would sleep whilst the other was awake. I have not heard whether the one could articulate whilst the other slept.

Bornean Rhinoceros.—Mr. Bartlett writes to me: "We now know for certain that the Bornean is the same as the Sumatran. This comes of course from Hart Everett, and I do not doubt it for a moment." But I have strong grounds for believing that there are two kinds:—1. A Government official who recently spent a year in the deepest recesses of the island says the natives told him there were two kinds. 2. About eight years ago a small rhinoceros was killed at Bunut, about 150 miles above Sintang, on the Pontianak. This is certain, it had only one horn. I have recently spoken to an officer who spent a year and a half in the interior, and he says he always understood the animal had only one horn. Anyhow it is very rare indeed. No European I have met—and many have been a long way into Borneo—has seen it. That may be because they are phlegmatic Dutch, and not inquiring English. But the natives who killed the one at Bunut had never seen one before. At the first sight they fled in terror at such a beast. It might have been a young *R. Sum.*, as the horn was very small, and perhaps the trifling development of bud horn escaped notice.

A Dutch ship, the *Batavia*, has at length reached the point where the 141st degree cuts the west coast of New Guinea. This is considered a great feat; why, I can't precisely say. There has been a good deal of talk about sending explorers to the Dutch end of New Guinea, but directly money is asked for silence reigns. They had much better finish with Sumatra before going to New Guinea.

The cattle plague has been raging in the west end of Java, Bantam, the Preanger, and Batavia residences—during the west monsoon (now finishing) with redoubled vigour. It has now abated a little (after four years it may well do so, from want of victims) in these parts, but is extending eastward, its appearance in Krawang being the most alarming. The authorities have decided upon making a double fence right across Java at its narrowest part. This means a line from somewhere about Cheribon due south. In the interval—a considerable one—

between the two fences, no cattle will be allowed to pass or exist.

There is a bird (native name Jallak) which follows the buffaloes about and perches on their backs. Query, can this bird have anything to do with the spreading of the plague? If so I don't see what Government can do. They can't fence him out.

In all the parts where the cattle-plague has raged the most awful fevers have been the result amongst the native population. In Bantam alone 50,000 died in 1880. In the Preanger and Batavia the death-rate was also very high. There is no doubt whatever that this is due to the imperfect interment of the carcases. The Government says it is due to the wet season; but this is a lame excuse, for why is there no fever elsewhere? In the wet season it is, of course, worse, for the heavy rains cause more miasma.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, July.—The direct manufacture of iron from ore, puddling, heating furnaces, and forge cinders, &c., by Mr. Du Puy. Discussion on steel rails.—Experiments with screw-propellers of different material and dimensions, applied to the steamer *Lookout*, with the hull coppered and not coppered, by Chief-Engineer Isherwood.—Percussion rock-drills, by Mr. Grimshaw.—Radio-dynamics, by Dr. Pliny Earle Chase.

Annalen der Physik und Chemie, No. 7.—On the forces acting on the interior of magnetically or dielectrically polarised bodies, by H. Helmholtz.—On the conductivities of metals for heat and electricity, by G. Kirchhoff and G. Hansemann.—On the same, by L. Lorenz.—The specific heat of liquid organic compounds and its relation to their molecular weight, by M. A. von Reis.—Contribution to the doctrine of induced magnetism, by E. Riecke.—On crystal analysis, by O. Lehmann.—On the contraction observed in formation of haloid salts in comparison with their heat of formation, by W. Müller-Erzbach.—Automatic mercury air-pump, by A. Schuller.—The theory of the law of saturation, by M. Planck.—The so-called self-exciting influence-machine, by P. Riess.—On K. Strecker's paper on the specific heat of chlorine, &c., by L. Boltzmann.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 5.—On curves of the third order, by M. le Paige.—On the structure of the reproductive apparatus of Teleosteans (second paper), by Mr. MacLeod.—On a registering apparatus for signals of the mirror galvanometer, by M. Samuel.

Journal de Physique, July.—Experimental researches on the capacity of voltaic polarisation, by M. Blondlot.—Fundamental equations of induced magnetism, according to Maxwell, by M. Bouty.—Pumping machines and pneumatic apparatus, by M. de Romilly.—Researches on the specific heat of mixtures of heat and the three primary alcohols $C_2H_4O_2$, $C_4H_6O_2$ and $C_6H_8O_2$, by Dr. Zetterman.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, Vol. xiv., fasc. x.-xi.—Contribution to the study of Amœba, by Dr. Grassi.—On an evaporimeter with constant level, by Prof. Fornioni.—Some researches on the distillation of cadaveric alkaloids, by S. Soldaini.

Memorie della Società degli Spettroscopisti Italiani, May.—Solar observations at Palermo Observatory during the first quarter of 1881, by Prof. Ricco.—On photographic photometry, by M. Janssen.—Scientific monument to P. Secchi in Reggio Emilia.—Spectroscopy applied to investigation of some colouring matters introduced into red wines, by S. Macagno.

Atti della R. Accademia dei Lincei, vol. v. fasc. 14.—Pharmacological researches on unstriped muscles, and particularly on the bladder, by P. Pellacani.—On some componnds of the furfuric series, by G. L. Ciamician and M. Dennstedt.—On some derivatives of pyrocol, by G. L. Ciamician and L. Danesi.—Action of nascent hydrogen on apoatropine, by L. Pesci.—On the saccharifying action of neutral salts, by F. Selmi.—Some theorems in geometry of *n* dimensions, by S. Veronese.

Rivista Scientifico-Industriale, No. 11, June 15.—Theory of siphons, by Prof. Rovelli.—New application of powdered graphite, by S. Mauri.—On Elban poltuce, by S. Corsi.—On radiant matter, by Prof. Mugna.

Archives des Sciences Physiques et Naturelles, No. 7, July 15.—Essay on the periodic variations of glaciers, by F. A. Forel.—Researches on the influence of heat on respiration, by W. Marcet.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 8.—M. Wurtz in the chair.—The following papers were read:—On the heat of formation of perchlorate of potass, by MM. Berthelot and Vieille.—Specific heats and heats of dilution of perchloric acid, by M. Berthelot.—Note on the communication to last meeting, by M. Boulley, on M. Toussaint's experiments on the infection produced by the juices of heated viands, by M. Chevreul.—Researches on the anhydrous chlorides of gallium, by M. Lecoq de Boisbaudran.—The standards of weights and measures of the Observatory, and the apparatus used in their construction, their origin, history, and present state, by M. C. Wolf.—On the Fuchsian functions, by M. H. Poincaré.—On the imitation, by means of hydrodynamics, of electrical and magnetic actions, by M. C. A. Bjerknes.—On the compressibility of carbonic acid and air under weak pressure and at a high temperature, by M. E. H. Amagat.—On the action of oxygen on mercury, by M. E. Amagat.—On the heating of waggons, carriages, &c., by means of the crystallised acetate of soda, by M. A. Ancelin.—Researches on the conditions of manufacture of magnets, by M. G. Trouvé.—Dissociation comparison of formulæ by experiment, by M. G. Lemoine.—Action of sulphuric acid on bromic amylene, by M. Chatin.—On a solution, of density 3·28, suitable for the immediate analysis of rocks, by M. D. Klein.—Tuberculous infection by the liquids of secretion and the serosity of vaccine pustules, by M. H. Toussaint.—Note on hydrophobia, by M. H. Duboué.

August 16.—M. Wurtz in the chair.—On cometary appearances, by M. J. Jamin.—Researches on the anhydrous chlorides of gallium, by M. Lecoq de Boisbaudran.—Singular effects of a gust of south-west wind, by M. G. A. Hirn.—Report of the place of Claude de Jouffroy in the discovery of steam-navigation, by M. de Lesseps.—The alcamines, by M. A. Ladenburg.—On the solubility of carbonate of magnesia in water charged with carbonic acid, by MM. Engel and Ville.—On the cobaltamines, by M. Porumbaru.—On the seat of gustation in dipterous insects: anatomical constitution and physiological value of the epipharynx and hypopharynx, by MM. Künckel and Gazagnaire.—On parasitism and tuberculosis, by M. H. Toussaint.—On the shooting stars of August, 1881, by M. Chapelas.

CONTENTS

THURSDAY, SEPTEMBER 1, 1881

THE BRITISH ASSOCIATION

THE Fifty-first Annual Meeting of the British Association was opened yesterday under the presidency of Sir John Lubbock, Bart., M.P., F.R.S., at York, the birthplace of the Association fifty years ago (September 27, 1831). Almost as easily might we compare the first meeting of the Accademia del Cimento when Roberval and Mersennus and Torricelli discussed the nature of the vacuum with the last meeting of the Nuovo Cimento, as compare the meeting of the British Association of 1831 with that of 1881. Railways, telegraphs, telephones, and electric lighting were unknown; the doctrines of evolution and the conservation of energy had not been developed; geology, palæontology, and petrology were in their infancy; the modern applications of spectroscopy were scarcely thought of; the mechanical equivalent of heat had not been determined. Several sciences, which at that time consisted of a mere collection of ill-arranged facts, have since, by the application of logical methods, had conferred upon them an individuality which they never before possessed. Science schools have arisen in all directions; the State yearly examines some thousands of its subjects; the Universities have created new professorships, have vitalised the old ones, and have placed science scholarships on an equality with those which formerly were only given for classics and mathematics. The Universities having opened their doors to the new culture, and it has become a necessary part of elementary education; while technical schools in all our large centres instruct thousands of artisans in the rudiments of natural knowledge. Has the British Association kept pace with this prodigious development?

What were the ideas of its founders? William Vernon Harcourt, "the lawgiver and proper founder of the British Association," said at the opening meeting that its objects should be "to give a stronger impulse and more systematic direction to scientific inquiry, to obtain a greater degree of national attention to the objects of science, and a removal of those disadvantages which impede its progress, and to promote the intercourse of the cultivators of science with one another and with foreign philosophers." By its reports, committees, recommendations, and grants, the Association has to some extent succeeded in each of these objects. But Mr. Vernon Harcourt planned the Association on a wider basis than that upon which it rests. "I propose to you," he said, "to found an association, including all the strength of Great Britain, which shall employ a short period of every year in pointing out the lines of direction in which the researches of science should move; in indicating the particulars which most immediately demand investigation; in stating problems to be solved and data to be fixed; in assigning to every class of mind a definite task; and suggesting to its members that there is here a shore of which the soundings should be more accurately taken, and there a line of coast along which a voyage of discovery should be made." We venture to think that this course of action might be more closely followed with advantage. It is true that a few committees are appointed to report upon, and sometimes to experiment upon, certain defined objects, but if each section could give a list of the most important questions awaiting answer in its particular science—somewhat in the form of a modernised *Inquisitio de Naturâ Calidi*—energy would less often be expended about the mint, the anise, and the cumin, and more often applied to the weightier matters of the sciences. Men would then more frequently forge connections in the mighty chain, in place of separate links which sometimes rust away before a place is found for them.

The earlier presidents delighted to find in the Association the development of Bacon's idea of the "New Atlantis." But we venture with great deference to submit that it never has and never can approach the character of that academy of universal science. A nearer approach to it was to be found in the old Gresham College, and may now be met with in any one of the new colleges of sciences. Bacon's idea was to have a vast inclosure containing "elaboratories chymicall and phisicall," anatomical and metallurgical, observatories of every kind, botanical gardens, museums, and operatories for every science. Connected with these there was to be a staff of workers and a staff of thinkers; also a kind of scientific society, or collection of societies, in which the results should be discussed. There are a thousand workers in the domains of the sciences now where there was one fifty years ago; discoveries and inventions multiply, and scientific literature is assuming vast proportions; but at present we are as far from the lofty and majestic ideal of the New Atlantis as we were in 1831.

But let us not for a moment underrate the valuable work which the Association has accomplished. Many of the Reports of committees or individuals are classical, and the suggestions which they furnish have led to considerable results. Take one example: the establishment of magnetic observatories all over the world is mainly due to the action of the Association. "By no sudden impulse or accidental circumstance," said Prof. Phillips in the Birmingham presidential address in 1865, "rose to its high importance that great system of magnetic observations on which for more than a quarter of a century the British Association and the Royal Society, acting in concert, have been intent. First we had reports on the mathematical theory, and experimental researches of magnetism by Christie, 1833; Whewell, 1835; and Sabine, 1835. Afterwards a magnetic survey of the British Islands; then the establishment of a complete observatory at Dublin, with newly arranged instruments, by Dr. Lloyd in 1838. On all this gathered experience we founded a memorial to Her Majesty's Government, made a grant of 400*l*. from our funds for preliminary expenses, and presented to the meeting of this Association in Birmingham in 1839 a report of progress signed by Herschel and Lloyd. From that time how great the labour, how inestimable the fruits! Ross sails to the magnetic pole of the south; America and Russia co-operate with our observers at Kew, Toronto, and St. Helena; and General Sabine, by combining all this united labour, has the happiness of seeing results established of which no man dreamed—laws of harmonious variation affecting the magnetic elements of the globe, indefinite relation to the earth's

movement, the position of the sun and moon, the distribution of temperature, and the situation in latitude and longitude."

We must bear in mind, however, that the great mass of members at any one meeting are not made up of scientific men who can appreciate the full development of a train of ideas or results, but of people who have not the advantages of attending the meetings of the London scientific societies, or of being *au courant* with scientific progress, and we may fitly inquire by what means their interests are best served. The President's address is perhaps the most powerful stimulus. Such addresses usually belong to one of three classes:—they are either distinguished by a fine display of oratory; or by the discussion of some leading theory concerning which the president has a right to speak *ex cathedrâ;* or they give a *résumé* of the scientific progress of the year. This last is of the greatest utility to the general run of members. Sometimes the three classes are judiciously combined, and these addresses are commonly the best of all. In former years the Presidential Address was very short, and chiefly discussed the results obtained by the Committees, and the Reports thereon. Occasionally an unscientific nobleman has opened the proceedings by a *quasi* after-dinner speech, while anon we have a sophistical declamation dealing with some of the burning questions of the hour, and disposing of them bravely.

Many cities have received the Association twice, but few three times. York will now be one of the latter, but it is thirty-six years since the last meeting was held there. Murchison called it the "cradle of the Association," and at the second York meeting the tickets bore the inscription, *Antiquam exquirite Matrem.* If the Association carries out the ideas of its founders, we may fairly hope that a centennial and even a millennial meeting will be held in the place of its birth. The city has many objects of interest: it possesses convenient accommodation for all the sections, and a number of important manufactories can be easily visited from it. The local committee have issued an extremely useful programme of their arrangements, which not only contains all the necessary information concerning trains, posts, lodgings, and the places of meeting, but also articles on the zoology, botany, and geology of the neighbourhood, and a description of the various excursions. An interesting article on "The York Founders of the Association" is contributed by Archdeacon Hey. An exhibition of art and industrial produce, and a collection of scientific apparatus, will be open during the week. Four excursions are organised for Saturday, September 3: to Scarborough; to Castle Howard; to Helmsley and Rievaulx; and to Brimham Rocks and Harrogate. On the following Thursday there will be seven excursions: to Bolton Abbey and the Strid; to Cleveland; a coast excursion; to Gristhorpe, Speeton, and Scarborough; to Whitby; to Wensleydale; and to Aldborough and Boroughbridge. Among the more important manufactories which will be visited are the telescope works of Messrs. Cooke and Sons, the workshops of the North Eastern Railway, the York glass works, and some extensive confectionery works. Naturalists will be glad to learn that the county possesses a fauna which comprises 513 out of the 717 British Vertebrata, viz. 46 mammals, 307 birds, 12 reptiles, and 148 fishes. It also furnishes 71 per cent. of the British flowering-plants and ferns. Geologically the county consists of rounded Chalk Hills, Oolite overlying the Lias, Trias covered with glacial drift and alluvial deposit, and a narrow band of Permian strata. Many opportunities will be afforded to members of studying the geology of the district.

The famous Kirkdale Cave, which was the first to be scientifically examined, gave rise to the Yorkshire Philosophical Society. The numerous remains found in it became the basis of a museum, and to it was attached the scientific society of which John Phillips was one of the secretaries. The idea of the Association was broached by Brewster in a letter to Phillips. The Council of the Yorkshire Society issued the first invitations, and its president, vice-president, treasurer, and secretaries filled the same offices at the first meeting of the Association. The writer of an able article in the *Times* of last Friday points out that in place of the few philosophical societies of fifty years ago there are now a hundred or two scattered all over the country often doing good work, which is to a great extent lost or wasted because inaccessible to the scientific world, and he suggests that the Association should act as a bond of union between these societies, proposing methods of work and special kinds of research suitable to the particular district. This might surely be done with great advantage in the case of the natural history sciences and geology; and we think the idea is worthy the attention of the Association. If, furthermore, it could publish a *résumé* of the more important results obtained by the several local societies during each year, it would be a boon to scientific literature.

As might have been expected, the "Jubilee Meeting" of the Association is likely to attract an unusually large gathering. On Tuesday upwards of 1500 names had been enrolled. The special character of the meeting is likely to have an influence not only on the presidential addresses, but on the nature of the entire proceedings.

Inaugural Address by Sir John Lubbock, Bart., M.P., F.R.S., D.C.L., LL.D., President

In the name of the British Association, which for the time I very unworthily represent, I beg to tender to you, my Lord Mayor, and through you to the City of York, our cordial thanks for your hospitable invitation and hearty welcome.

We feel, indeed, that in coming to York we are coming home: gratefully as we acknowledge and much as we appreciate the kindness we have experienced elsewhere, and the friendly relations which exist between this Association and most—I might even say, all—our great cities, yet Sir R. Murchison truly observed at the close of our first meeting in 1831, that to York, "as the cradle of the Association, we shall ever look back with gratitude; and whether we meet hereafter on the banks of the Isis, the Cam, or the Forth, to this spot we shall still fondly revert." Indeed, it would have been a matter of much regret to all of us, if we had not been able on this, our fiftieth anniversary, to hold our meeting in our mother city.

My Lord Mayor, before going further, I must express my regret, especially when I call to mind the illustrious men who have preceded me in this chair, that it has not fallen to one of my eminent friends around me, to preside on this auspicious occasion. Conscious, however, as I am of my own deficiencies, I feel that I must not waste time in dwelling on them, more especially as in doing so I should but give them greater prominence. I will, therefore, only make one earnest appeal to your kind indulgence.

The connection of the British Association with the City of York does not depend merely on the fact that our first meeting was held here. It originated in a letter addressed by Sir D. Brewster to Prof. Phillips, as Secretary to your York Philosophical Society, by whom the idea was warmly taken up. The first meeting was held on September 26, 1831, the chair being taken by Lord Milton, who delivered an address, after which Mr. William Vernon Harcourt, Chairman of the Committee of Management, submitted to the meeting a code of rules which had been so maturely considered, and so wisely framed, that they have remained substantially the same down to the present day.

The constitution and objects of the Association were so ably described by Mr. Spottiswoode, at Dublin, and are so well known to you, that I will not dwell on them this evening. The excellent President of the Royal Society, in the same address, suggested that the past history of the Association would form an appropriate theme for the present meeting. The history of the Association, however, is really the history of science, and I long shrank from the attempt to give even a panoramic survey of a subject so vast and so difficult; nor should I have ventured to make any such attempt, but that I knew I could

rely on the assistance of friends in every department of science.

Certainly, however, this is an opportunity on which it may be well for us to consider what have been the principal scientific results of the last half-century, dwelling especially on those with which this Association is more directly concerned, either as being the work of our own members, or as having been made known at our meetings. It is of course impossible within the limits of a single address to do more than allude to a few of these, and that very briefly. In dealing with so large a subject I first hoped that I might take our annual volumes as a text-book. This, however, I at once found to be quite impossible. For instance, the first volume commences with a Report on Astronomy by Sir G. Airy; I may be pardoned, I trust, for expressing my pleasure at finding that the second was one by my father, on the Tides, prepared like the preceding at the request of the Council; then comes one on Meteorology by Forbes, Radiant Heat by Baden Powell, Optics by Brewster, Mineralogy by Whewell, and so on. My best course will therefore be to take our different Sections one by one, and endeavour to bring before you a few of the principal results which have been obtained in each department.

The Biological Section is that with which I have been most intimately associated, and with which it is, perhaps, natural that I should begin.

Fifty years ago it was the general opinion that animals and plants came into existence just as we now see them. We took pleasure in their beauty; their adaptation to their habits and mode of life in many cases could not be overlooked or misunderstood. Nevertheless, the book of Nature was like some richly illuminated missal, written in an unknown tongue; the graceful forms of the letters, the beauty of the colouring, excited our wonder and admiration; but of the true meaning little was known to us; indeed we scarcely realised that there was any meaning to decipher. Now glimpses of the truth are gradually revealing themselves; we perceive that there is a reason—and in many cases we know what that reason is—for every difference in form, in size, and in colour; for every bone and every feather, almost for every hair. Moreover, each problem which is solved opens out vistas, as it were, of others perhaps even more interesting. With this great change the name of our illustrious countryman, Darwin, is intimately associated, and the year 1859 will always be memorable in science as having produced his great work on "The Origin of Species." In the previous year he and Wallace had published short papers, in which they clearly state the theory of natural selection, at which they had simultaneously and independently arrived. We cannot wonder that Darwin's views should have at first excited great opposition. Nevertheless from the first they met with powerful support, especially, in this country, from Hooker, Huxley, and Herbert Spencer. The theory is based on four axioms:—

"1. That no two animals or plants in nature are identical in all respects.

"2. That the offspring tend to inherit the peculiarities of their parents.

"3. That of those which come into existence, only a small number reach maturity.

"4. That those, which are, on the whole, best adapted to the circumstances in which they are placed, are most likely to leave descendants."

Darwin commenced his work by discussing the causes and extent of variability in animals, and the origin of domestic varieties; he showed the impossibility of distinguishing between varieties and species, and pointed out the wide differences which man has produced in some cases—as, for instance, in our domestic pigeons, all unquestionably descended from a common stock. He dwelt on the struggle for existence (which has since become a household word), and which, inevitably resulting in the survival of the fittest, tends gradually to adapt any race of animals to the conditions in which it occurs.

While thus, however, showing the great importance of natural selection, he attributed to it no exclusive influence, but fully admitted that other causes—the use and disuse of organs, sexual selection, &c.—had to be taken into consideration. Passing on to the difficulties of his theory he accounted for the absence of intermediate varieties between species, to a great extent, by the imperfection of the geological record.

But if the geological record be imperfect, it is still very instructive. The further palæontology has progressed the more it has tended to fill up the gaps between existing groups and species, while the careful study of living forms has brought into prominence the variations dependent on food, climate, habitat, and other conditions, and shown that many species long supposed to be absolutely distinct are so closely linked together by intermediate forms that it is difficult to draw a satisfactory line between them.

The principles of classification point also in the same direction, and are based more and more on the theory of descent. Biologists endeavour to arrange animals on what is called the "natural system." No one now places whales among fish, bats among birds, or shrews with mice, notwithstanding their external similarity; and Darwin maintained that "community of descent was the hidden bond which naturalists had been unconsciously seeking." How else, indeed, can we explain the fact that the framework of bones is so similar in the arm of a man, the wing of a bat, the fore-leg of a horse, and the fin of a porpoise—that the neck of a giraffe and that of an elephant contain the same number of vertebræ?

Strong evidence is, moreover, afforded by embryology; by the presence of rudimentary organs and transient characters, as, for instance, the existence in the calf of certain teeth which never cut the gums, the shrivelled and useless wings of some beetles, the presence of a series of arteries in the embryos of the higher Vertebrata exactly similar to those which supply the gills in fishes, even the spots on the young blackbird, the stripes on the lion's cub; these, and innumerable other facts of the same character, appear to be incompatible with the idea that each species was specially and independently created; and to prove, on the contrary, that the embryonic stages of species show us more or less clearly the structure of their ancestors.

Darwin's views, however, are still much misunderstood. I believe there are thousands who consider that according to his theory a sheep might turn into a cow, or a zebra into a horse. No one would more confidently withstand any such hypothesis, his view being, of course, not that the one could be changed into the other, but that both are descended from a common ancestor.

No one, at any rate, will question the immense impulse which Darwin has given to the study of natural history, the number of new views he has opened up, and the additional interest which he has aroused in, and contributed to, Biology. When we were young we knew that the leopard had spots, the tiger was striped, and the lion tawny; but why this was so it did not occur to us to ask; and if we had asked no one would have answered. Now we see at a glance that the stripes of the tiger have reference to its life among jungle-grasses; the lion is sandy, like the desert; while the markings of the leopard resemble spots of sunshine glancing through the leaves.

The science of embryology may almost be said to have been created in the last half-century. Fifty years ago it was a very general opinion that animals which are unlike when mature, were dissimilar from the beginning. It is to Von Baer, the discoverer of the mammalian ovum, that we owe the great generalisation that the development of the egg is in the main a progress from the general to the special, in fact, that embryology is the key to the laws of animal development.

Thus the young of existing species resemble in many cases the mature forms which flourished in ancient times. Huxley has traced up the genealogy of the horse to the Miocene Anchitherium. In the same way Gaudry has called attention to the fact that just as the individual stag gradually acquires more and more complex antlers; having at first only a single prong, in the next year two points, in the following three, and so on; so the genus, as a whole, in Middle Miocene times, had two pronged horns; in the Upper Miocene, three; and that it is not till the Upper Pliocene that we find any species with the magnificent antlers of our modern deer. It seems to be now generally admitted that birds have come down to us through the Dinosaurians, and, as Huxley has shown, the profound break once supposed to exist between birds and reptiles has been bridged over by the discovery of reptilian birds and bird-like reptiles; so that, in fact, birds are modified reptiles. Again, the remarkable genus Peripatus, so well studied by Moseley, tends to connect the annulose and articulate types.

Again, the structural resemblances between Amphioxus and the Ascidians had been pointed out by Goodsir; and Kowalevsky in 1866 showed that these were not mere analogies, but indicated a real affinity. These observations, in the words of Allen Thomson, "have produced a change little short of revolutionary in embryological and zoological views, leading as they do to

the support of the hypothesis that the Ascidian is an earlier stage in the phylogenetic history of the mammal and other vertebrates."

The larval forms which occur in so many groups, and of which the Insects afford us the most familiar examples, are, in the words of Quatrefages, embryos, which lead an independent life. In such cases as these external conditions act upon the larvæ as they do upon the mature form; hence we have two classes of changes, adaptational or adaptive, and developmental. These and many other facts must be taken into consideration; nevertheless naturalists are now generally agreed that embryological characters are of high value as guides in classification, and it may, I think, be regarded as well-established that, just as the contents and sequence of rocks teach us the past history of the earth, so is the gradual development of the species indicated by the structure of the embryo and its developmental changes.

When the supporters of Darwin are told that his theory is incredible, they may fairly ask why it is impossible that a species in the course of hundreds of thousands of years should have passed through changes which occupy only a few days or weeks in the life-history of each individual?

The phenomena of yolk-segmentation, first observed by Prevost and Dumas, are now known to be in some form or other invariably the precursors of embryonic development; while they reproduce, as the first stages in the formation of the higher animals, the main and essential features in the life-history of the lowest forms. The "blastoderm" as it is called, or first germ of the embryo in the egg, divides itself into two layers, corresponding, as Huxley has shown, to the two layers into which the body of the Cœlenterata may be divided. Not only so, but most embryos at an early stage of development have the form of a cup, the walls of which are formed by the two layers of the blastoderm. Kowalevsky was the first to show the prevalence of this embryonic form, and subsequently Lankester and Hæckel put forward the hypothesis that it was the embryonic repetition of an ancestral type, from which all the higher forms are descended. The cavity of the cup is supposed to be the stomach of this simple organism, and the opening of the cup the mouth. The inner layer of the wall of the cup constitutes the digestive membrane, and the outer the skin. To this form Hæckel gave the name Gastræa. It is perhaps doubtful whether the theory of Lankester and Hæckel can be accepted in precisely the form they propounded it; but it has had an important influence on the progress of embryology. I cannot quit the science of embryology without alluding to the very admirable work on "Comparative Embryology" by our new general secretary, Mr. Balfour, and also the "Elements of Embryology" which he had previously published in conjunction with Dr. M. Foster.

In 1842, Steenstrup published his celebrated work on the "Alternation of Generations," in which he showed that many species are represented by two perfectly distinct types or broods, differing in form, structure, and habits; that in one of them males are entirely wanting, and that the reproduction is effected by fission, or by buds, which, however, are in some cases structurally indistinguishable from eggs. Steenstrup's illustrations were mainly taken from marine or parasitic species, of very great interest, but not generally familiar, excepting to naturalists. It has since been shown that the common Cynips or Gallfly is also a case in point. It had long been known that in some genera belonging to this group, males are entirely wanting, and it has now been shown by Bassett, and more thoroughly by Adler, that some of these species are double-brooded; the two broods having been considered as distinct genera.

Thus an insect known as *Neuroterus lenticularis*, of which females only occur, produces the familiar oak-spangles so common on the under sides of oak leaves, from which emerge, not *Neuroterus lenticularis*, but an insect hitherto considered as a distinct species, belonging even to a different genus, *Spathegaster baccarum*. In Spathegaster both sexes occur; they produce the currant-like galls found on oaks, and from these galls Neuroterus is again developed. So also the King Charles oak-apples produce a species known as *Teras terminalis*, which descends to the ground, and makes small galls on the roots of the oak. From these emerge an insect known as *Biorhiza aptera*, which again gives rise to the common oak-apple.

It might seem that such inquiries as these could hardly have any practical bearing. Yet it is not improbable that they may lead to very important results. For instance, it would appear that the fluke which produces the rot in sheep, passes one phase of its existence in the black slug, and we are not without hopes that the researches, in which our lamented friend Prof. Rolleston was engaged at the time of his death, which we all so much deplore, will lead, if not to the extirpation, at any rate to the diminution, of a pest from which our farmers have so grievously suffered.

It was in the year 1839 that Schwann and Schleiden demonstrated the intimate relation in which animals and plants stand to each other, by showing the identity of the laws of development of the elementary parts in the two kingdoms of organic nature.

As regards descriptive biology, by far the greater number of species now recorded have been named and described within the last half-century.

Dr. Günther has been good enough to make a calculation for me. The numbers, of course, are only approximate, but it appears that while the total number of animals described up to 1831 was not more than 70,000, the number now is at least 320,000.

Lastly, to show how large a field still remains for exploration, I may add that Mr. Waterhouse estimates that the British Museum alone contains not fewer than 12,000 species of insects which have not yet been described, while our collections do not probably contain anything like one-half of those actually in existence. Further than this, the anatomy and habits even of those which have been described offer an inexhaustible field for research, and it is not going too far to say that there is not a single species which would not amply repay the devotion of a lifetime.

One remarkable feature in the modern progress of biological science has been the application of improved methods of observation and experiment; and the employment in physiological research of the exact measurements employed by the experimental physicist. Our microscopes have been greatly improved. The use of chemical re-agents in microscopical investigations has proved most instructive, and another very important method of investigation has been the power of obtaining very thin slices by imbedding the object to be examined in paraffin or some other soft substance. In this manner we can now obtain, say, fifty separate sections of the egg of a beetle, or the brain of a bee.

At the close of the last century, Sprengel published a most suggestive work on flowers, in which he pointed out the curious relation existing between these and insects, and showed that the latter carry the pollen from flower to flower. His observations, however, attracted little notice until Darwin called attention to the subject in 1862. It had long been known that the cowslip and primrose exist under two forms, about equally numerous, and differing from one another in the arrangements of their stamens and pistils; the one form having the stamens on the summit of the flower and the stigma half-way down; while in the other the relative positions are reversed, the stigma being at the summit of the tube and the stamens half-way down. This difference had, however, been regarded as a case of mere variability; but Darwin showed it to be a beautiful provision, the result of which is that insects fertilise each flower with pollen brought from a different plant; and he proved that flowers fertilised with pollen from the other form yield more seed than if fertilised with pollen of the same form, even if taken from a different plant.

Attention having been thus directed to the question, an astonishing variety of most beautiful contrivances have been observed and described by many botanists, especially Hooker, Axel, Delpino, Hildebrand, Bennett, Fritz Müller, and above all Hermann Müller and Darwin himself. The general result is that to insects, and especially to bees, we owe the beauty of our gardens, the sweetness of our fields. To their beneficent, though unconscious action, flowers owe their scent and colour, their honey—nay, in many cases, even their form. Their present shape and varied arrangements, their brilliant colours, their honey, and their sweet scent are all due to the selection exercised by insects.

In these cases the relation between plants and insects is one of mutual advantage. In many species, however, plants present us with complex arrangements adapted to protect them from insects; such, for instance, are in many cases the resinous glands which render leaves unpalatable; the thickets of hairs and other precautions which prevent flowers from being robbed of their honey by ants. Again, more than a century ago, our countryman, Ellis, described an American plant, Dionæa, in which the leaves are somewhat concave, with long lateral spines and a joint in the middle; close up with a jerk, like a rat-trap, the moment any

unwary insect alights on them. The plant, in fact, actually captures and devours insects. This observation also remained as an isolated fact until within the last few years, when Darwin, Hooker, and others have shown that many other species have curious and very varied contrivances for supplying themselves with animal food.

Some of the most fascinating branches of botany—morphology, histology, and physiology—scarcely existed before 1830. In the two former branches the discoveries of von Mohl are pre-eminent. He first observed cell-division in 1835, and detected the presence of starch in chlorophyll-corpuscles in 1837, while he first described protoplasm, now so familiar to us, at least by name, in 1846. In the same year Amici discovered the existence of the embryonic vesicle in the embryo sac, which develops into the embryo when fertilised by the entrance of the pollen-tube into the micropyle. The existence of sexual reproduction in the lower plants was doubtful, or at least doubted by some eminent authorities, as recently as 1853, when the actual process of fertilisation in the common bladderwrack of our shores was observed by Thuret, while the reproduction of the larger fungi was first worked out by De Bary in 1863.

As regards lichens, Schwendener proposed, in 1869, the startling theory, now however accepted by some of the highest authorities, that lichens are not autonomous organisms, but commensal associations of a fungus parasitic on an alga. With reference to the higher Cryptogams it is hardly too much to say that the whole of our exact knowledge of their life-history has been obtained during the last half-century. Thus in the case of ferns the male organs, or antheridia, were first discovered by Nägeli in 1844, and the archegonia, or female organs, by Suminski in 1848. The early stages in the development of mosses were worked out by Valentine in 1833. Lastly, the principle of Alternation of Generations in plants was discovered by Hofmeister. This eminent naturalist also, in 1851-4, pointed out the homologies of the reproductive processes in mosses, vascular cryptogams, gymnosperms, and angiosperms.

Nothing could have appeared less likely than that researches into the theory of spontaneous generation should have led to practical improvements in medical science. Yet such has been the case. Only a few years ago Bacteria seemed mere scientific curiosities. It had long been known that an infusion—say, of hay—would, if exposed to the atmosphere, be found, after a certain time, to teem with living forms. Even those few who still believe that life would be spotaneously generated in such an infusion, will admit that these minute organisms are, if not entirely, yet mainly, derived from germs floating in our atmosphere; and if precautions are taken to exclude such germs, as in the careful experiments especially of Pasteur, Tyndall, and Roberts, every one will grant that in ninety-nine cases out of a hundred no such development of life will take place.

These facts have led to most important results in Surgery. One reason why compound fractures are so dangerous, is because, the skin being broken, the air obtains access to the wound, bringing with it innumerable germs, which too often set up putrefying action. Lister first made a practical application of these observations. He set himself to find some substance capable of killing the germs, without being itself too potent a caustic, and he found that dilute carbolic acid fulfilled these conditions. This discovery has enabled many operations to be performed which would previously have been almost hopeless.

The same idea seems destined to prove as useful in Medicine as in Surgery. There is great reason to suppose that many diseases, especially those of a zymotic character, have their origin in the germs of special organisms. We know that fevers run a certain definite course. The parasitic organisms are at first few, but gradually multiply at the expense of the patient, and then die out again. Indeed, it seems to be thoroughly established that many diseases are due to the excessive multiplication of microscopic organisms, and we are not without hope that means will be discovered by which, without injury to the patient, these terrible, though minute, enemies may be destroyed, and the disease thus stayed. The interesting researches of Burdon Sanderson, Greenfield, Koch, Pasteur, Toussaint, and others, seem to justify the hope that we may be able to modify these and other germs, and then by appropriate inoculation to protect ourselves against fever and other acute diseases.

The history of Anæsthetics is a most remarkable illustration of how long we may be on the very verge of a most important discovery. Ether, which, as we all know, produces perfect insensibility to pain, was discovered as long ago as 1540. The anæsthetic property of nitrous oxide, now so extensively used, was observed in 1800 by Sir H. Davy, who actually experimented on himself, and had one of his teeth painlessly extracted when under its influence. He even suggests that "as nitrous oxide gas seems capable of destroying pain, it could probably be used with advantage in surgical operations." Nay, this property of nitrous oxide was habitually explained and illustrated in the chemical lectures given in hospitals, and yet for fifty years the gas was never used in actual operations.

Few branches of science have made more rapid progress in the last half-century than that which deals with the ancient condition of man. When our Association was founded it was generally considered that the human race suddenly appeared on the scene, about 6000 years ago, after the disappearance of the extinct mammalia, and when Europe, both as regards physical conditions and the other animals by which it was inhabited, was pretty much in the same condition as in the period covered by Greek and Roman history. Since then the persevering researches of Layard, Rawlinson, Botta and others have made known to us, not only the statues and palaces of the ancient Assyrian monarchs, but even their libraries; the cuneiform characters have been deciphered, and we can not only see, but read in the British Museum, the actual contemporary records, on burnt clay cylinders, of the events recorded in the historical books of the Old Testament and in the pages of Herodotus. The researches in Egypt also seem to have satisfactorily established the fact that the pyramids themselves are at least 6000 years old, while it is obvious that the Assyrian and Egyptian monarchies cannot suddenly have attained to the wealth and power, the state of social organisation, and progress in the arts, of which we have before us, preserved by the sand of the desert from the ravages of man, such wonderful proofs.

In Europe, the writings of the earliest historians and poets indicated that, before iron came into general use, there was a time when bronze was the ordinary material of weapons, axes, and other cutting implements, and though it seemed *à priori* improbable that a compound of copper and tin should have preceded the simple metal iron, nevertheless the researches of archæologists have shown that there really was in Europe a "Bronze Age," which at the dawn of history was just giving way to that of "Iron."

The contents of ancient graves, buried in many cases so that their owner might carry some at least of his wealth with him to the world of spirits, left no room for doubt as to the existence of a Bronze Age; but we get a completer idea of the condition of Man at this period from the Swiss lake-villages, first made known to us by Keller. Along the shallow edges of the Swiss lakes there flourished, once upon a time, many populous villages or towns, built on platforms supported by piles, exactly as many Malayan villages are now. Under these circumstances innumerable objects were one by one dropped into the water; sometimes whole villages were burnt, and their contents submerged; and thus we have been able to recover, from the waters of oblivion in which they had rested for more than 2000 years, not only the arms and tools of this ancient people, the bones of their animals, their pottery and ornaments, but the stuffs they wore, the grain they had stored up for future use, even fruits and cakes of bread.

But this bronze-using people were not the earliest occupants of Europe. The contents of ancient tombs give evidence of a time when metal was unknown. This also was confirmed by the evidence then unexpectedly received from the Swiss lakes. By the side of the bronze-age villages were others, not less extensive, in which, while implements of stone and bone were discovered literally by thousands, not a trace of metal was met with. The shell-mounds or refuse-heaps accumulated by the ancient fishermen along the shores of Denmark, fully confirmed the existence of a "Stone Age."

No bones of the reindeer, no fragment of any of the extinct mammalia, have been found in any of the Swiss lake-villages or in any of the thousands of tumuli which have been opened in our own country or in Central and Southern Europe. Yet the contents of caves and of river-gravels afford abundant evidence that there was a time when the mammoth and rhinoceros, the musk-ox and reindeer, the cave lion and hyena, the great bear and the gigantic Irish elk wandered in our woods and valleys, and the hippopotamus floated in our rivers; when England and France were united, and the Thames and the Rhine had a common estuary. This was long supposed to be before the advent of Man. At length, however, the discoveries of Boucher de Perthes in the

valley of the Somme, supported as they are by the researches of many Continental naturalists, and in our own country of MacEnery and Godwin-Austen, Prestwich and Lyell, Vivian and Pengelly, Christy, Evans, and many more, have proved that Man formed a humble part of this strange assembly.

Nay, even at this early period there were at least two distinct races of men in Europe; one of them—as Boyd Dawkins has pointed out—closely resembling the modern Esquimaux in form, in his weapons and implements, probably in his clothing, as well as in so many of the animals with which he was associated.

At this stage Man appears to have been ignorant of pottery, to have had no knowledge of agriculture, no domestic animals, except perhaps the dog. His weapons were the axe, the spear, and the javelin; I do not believe he knew the use of the bow, though he was probably acquainted with the lance. He was, of course, ignorant of metal, and his stone implements, though skilfully formed, were of quite different shapes from those of the second Stone age, and were never ground. This earlier Stone period, when man co-existed with these extinct mammalia, is known as the Palæolithic, or Early Stone Age, in opposition to the Neolithic, or Newer Stone Age.

The remains of the mammalia which co-existed with Man in pre-historic times have been most carefully studied by Owen, Lartet, Rütimeyer, Falconer, Busk, Boyd Dawkins, and others. The presence of the mammoth, the reindeer, and especially of the musk-ox, indicates a severe, not to say an arctic, climate, the existence of which, moreover, was proved by other considerations; while, on the contrary, the hippopotamus requires considerable warmth. How then is this association to be explained?

While the climate of the globe is, no doubt, much affected by geographical conditions, the cold of the glacial period was, I believe, mainly due to the excentricity of the earth's orbit, combined with the obliquity of the ecliptic. The result of the latter condition is a period of 21,000 years, during one half of which the northern hemisphere is warmer than the southern, while during the other 10,500 years the reverse is the case. At present we are in the former phase, and there is, we know, a vast accumulation of ice at the south pole. But when the earth's orbit is nearly circular, as it is at present, the difference between the two hemispheres is not very great; on the contrary, as the excentricity of the orbit increases the contrast between them increases also. This excentricity is continually oscillating within certain limits, which Croll and subsequently Stone have calculated out for the last million years. At present the excentricity is ·016 and the mean temperature of the coldest month in London is about 40°. Such has been the state of things for nearly 100,000 years; but before that there was a period, beginning 300,000 years ago, when the excentricity of the orbit varied from ·26 to ·57. The result of this would be greatly to increase the effect due to the obliquity of the orbit; at certain periods the climate would be much warmer than at present, while at others the number of days in winter would be twenty more, and of summer twenty less than now, while the mean temperature of the coldest month would be lowered 20°. We thus get something like a date for the last glacial epoch, and we see that it was not simply a period of cold, but rather one of extremes, each beat of the pendulum of temperature lasting for no less than 21,000 years. This explains the fact that, as Morlot showed in 1854, the glacial deposits of Switzerland, and, as we now know, those of Scotland, are not a single uniform layer, but a succession of strata indicating very different conditions. I agree also with Croll and Geikie in thinking that these considerations explain the apparent anomaly of the co-existence in the same gravels of arctic and tropical animals; the former having lived in the cold, while the latter flourished in the hot, periods.

It is, I think, now well established that man inhabited Europe during the milder periods of the glacial epoch. Some high authorities indeed consider that we have evidence of his presence in pre-glacial and even in Miocene times, but I confess that I am not satisfied on this point. Even the more recent period carries back the record of man's existence to a distance so great as altogether to change our views of ancient history.

Nor is it only as regards the antiquity and material condition of man in prehistoric times that great progress has been made. If time permitted I should have been glad to have dwelt on the origin and development of language, of custom, and of law. On all of these the comparison of the various lower races still inhabiting so large a portion of the earth's surface, has thrown much light; while even in the most cultivated nations we find survivals, curious fancies, and lingering ideas; the fossil remains as it were of former customs and religions embedded in our modern civilisation, like the relics of extinct animals in the crust of the earth.

In Geology the formation of our Association coincided with the appearance of Lyell's "Principles of Geology," the first volume of which was published in 1830, and the second in 1832. At that time the received opinion was that the phenomena of Geology could only be explained by violent periodical convulsions, and a high intensity of terrestrial energy culminating in repeated catastrophes. Hutton and Playfair had indeed maintained that such causes as those now in operation would, if only time enough were allowed, account for the geological structure of the earth; nevertheless the opposite view generally prevailed, until Lyell, with rare sagacity and great eloquence, with a wealth of illustration and most powerful reasoning, convinced geologists that the forces now in action are powerful enough, if only time be given, to produce results quite as stupendous as those which science records.

As regards Stratigraphical Geology, at the time of the first meeting of the British Association at York, the strata between the Carboniferous Limestone and the Chalk had been mainly reduced to order and classified, chiefly through the labours of William Smith. But the classification of all the strata lying above the Chalk and below the Carboniferous Limestone respectively, remained in a state of the greatest confusion. The year 1831 marks the period of the commencement of the joint labours of Sedgwick and Murchison, which resulted in the establishment of the Cambrian, Silurian, and Devonian systems. Our pre-Cambrian strata have recently been divided by Hicks into four great groups of immense thickness, and implying, therefore, a great lapse of time; but no fossils have yet been discovered in them. Lyell's classification of the Tertiary deposits, the result of the studies which he carried on with the assistance of Deshayes and others, was published in the third volume of the "Principles of Geology" in 1833. The establishment of Lyell's divisions of Eocene, Miocene, and Pliocene, was the starting-point of a most important series of investigations by Prestwich and others of these younger deposits; as well as of the post-Tertiary, Quaternary, or drift beds, which are of special interest from the light they have thrown on the early history of Man.

As regards the physical character of the earth, two theories have been held: one, that of a fluid interior covered by a thin crust; the other, of a practically solid sphere. The former is now very generally admitted, both by astronomers and geologists, to be untenable. The prevailing feeling of geologists on this subject has been well expressed by Prof. Le Conte, who says, "the whole theory of igneous agencies—which is little less than the whole foundation of theoretic geology—must be reconstructed on the basis of a solid earth."

In 1837 Agassiz startled the scientific world by his "Discours sur l'ancienne extension des Glaciers," in which, developing the observation already made by Charpentier and Venetz, that boulders had been transported to great distances, and that rocks far away from, or high above, existing glaciers, are polished and scratched by the action of ice, he boldly asserted the existence of a "glacial period," during which Switzerland and the North of Europe were subjected to great cold and buried under a vast sheet of ice.

The ancient poets described certain gifted mortals as privileged to descend into the interior of the earth, and have exercised their imagination in recounting the wonders there revealed. As in other cases, however, the realities of science have proved more varied and surprising than the dreams of fiction. Of the gigantic and extraordinary animals thus revealed to us by far the greatest number have been described during the period now under review. For instance, the gigantic Cetiosaurus was described by Owen in 1838, the Dinornis of New Zealand by the same distinguished naturalist in 1839, the Mylodon in the same year, and the Archæopteryx in 1862.

In America, a large number of remarkable forms have been described, mainly by Marsh, Leidy, and Cope. Marsh has made known to us the Titanosaurus, of the American (Colorado) Jurassic beds, which is, perhaps, the largest land animal yet known, being a hundred feet in length, and at least thirty in height, though it seems possible that even these vast dimensions were exceeded by those of the Atlantosaurus. Nor must I omit the Hesperornis, described by Marsh in 1872, as a carnivorous, swimming ostrich, provided with teeth, which he regards as a character inherited from reptilian ancestors; the Ichthyornis,

stranger still, with biconcave vertebræ, like those of fishes, and teeth set in sockets.

As giving, in a few words, an idea of the rapid progress in this department, I may mention that Morris's "Catalogue of British Fossils," published in 1843, contained 5300 species; while that now in preparation by Mr. Etheridge enumerates 15,000.

But if these figures show how rapid our recent progress has been, they also very forcibly illustrate the imperfection of the geological record, and give us, I will not say a measure, but an idea, of the imperfection of the geological record. The number of all the described recent species is over 300,000, but certainly not half are yet on our lists, and we may safely take the total number of recent species as being not less than 700,000. But in former times there have been at the very least twelve periods, in each of which by far the greater number of species were distinct. True, the number of species was probably not so large in the earlier periods as at present; but if we make a liberal allowance for this, we shall have a total of more than 2,000,000 species, of which about 25,000 only are as yet upon record; and many of these are only represented by a few, some only by a single specimen, or even only by a fragment.

The progress of palæontology may also be marked by the extent to which the existence of groups has been, if I may so say, carried back in time. Thus I believe that in 1830 the earliest known quadrupeds were small marsupials belonging to the Stonesfield slates; the most ancient mammal now known is *Microlestes antiquus* from the Keuper of Würtemberg: the oldest bird known in 1831 belonged to the period of the London Clay, the oldest now known is the Archæopteryx of the Solenhofen slates, though it is probable that some at any rate of the foot-steps on the Triassic rocks are those of birds. So again the Amphibia have been carried back from the Trias to the Coal-measures; Fish from the Old Red Sandstone to the Upper Silurian; Reptiles to the Trias; Insects from the Cretaceous to the Devonian; Mollusca and Crustacea from the Silurian to the Lower Cambrian. The rocks below the Cambrian, though of immense thickness, have afforded no relics of animal life, if we except the problematical *Eozoon Canadense*, so ably studied by Dawson and Carpenter. But if palæontology as yet throws no light on the original forms of life, we must remember that the simplest and the lowest organisms are so soft and perishable that they would leave "not a wrack behind."

Passing to the science of Geography, Mr. Clements Markham has recently published an excellent summary of what has been accomplished during the half-century.

But the progress in our knowledge of geography is, and has been, by no means confined to the improvement of our maps, or to the discovery and description of new regions of the earth, but has extended to the causes which have led to the present configuration of the surface. To a great extent indeed this part of the subject falls rather within the scope of geology, but I may here refer, in illustration, to the distribution of lakes, the phenomena of glaciers, the formation of volcanic mountains, and the structure and distribution of coral islands.

The origin and distribution of lakes is one of the most interesting problems in physical geography. That they are not scattered at random, a glance at the map is sufficient to show. They abound in mountain districts, are comparatively rare in equatorial regions, increasing in number as we go north, so that in Scotland and the northern parts of America they are sown broadcast.

Perhaps *à priori* the first explanation of the origin of lakes which would suggest itself, would be that they were formed in hollows resulting from a disturbance of the strata, which had thrown them into a basin-shaped form. Lake-basins, however, of this character are, as a matter of fact, very rare; as a general rule lakes have not the form of basin-shaped synclinal hollows, but, on the contrary, the strike of the strata often runs right across them. My eminent predecessor, Prof. Ramsay, divides lakes into three classes:—(1) Those which are due to irregular accumulations of drift, and which are generally quite shallow. (2) Those which are formed by moraines, and (3) those which occupy true basins scooped by glacier-ice out of the solid rock. To the latter class belong most of the great Swiss and Italian lakes. Prof. Ramsay attributes their excavation to glaciers, because it is of course obvious that rivers cannot make basin-shaped hollows surrounded by rock on all sides. Now the Lake of Geneva, 1230 feet above the sea, is 984 feet deep, the Lake of Brienz is 1850 feet above the sea, and 2000 feet deep, so that its bottom is really below the sea-level. The Italian lakes are even more remarkable. The Lake of Como, 700 feet above the sea, is 1929 feet deep. Lago Maggiore, 685 feet above the sea, is no less than 2625 feet deep. It will be observed that these lakes, like many others in mountain regions, those of Scandinavia, for instance, lie in the direct channels of the great old glaciers. If the mind is at first staggered at the magnitude of the scale, we must remember that the ice which scooped out the valley in which the Lake of Geneva now reposes, was once at least 2700 feet thick; while the moraines were also of gigantic magnitude, that of Ivrea, for instance, being no less than 1500 feet in height. Prof. Ramsay's theory seems, therefore, to account beautifully for a large number of interesting facts.

Passing from lakes to mountains, two rival theories with reference to the structure and origin of volcanoes long struggled for supremacy.

The more general view was that the sheets of lava and scoriæ which form volcanic cones—such, for instance, as Ætna or Vesuvius—were originally nearly horizontal, and that subsequently a force operating from below, and exerting a pressure both upwards and outwards from a central axis towards all points of the compass, uplifted the whole stratified mass and made it assume a conical form, giving rise at the same time, in many cases, to a wide and deep circular opening at the top of the cone, called by the advocates of this hypothesis a "crater of elevation."

This theory, though, as it seems to us now, it had already received its death-blow from the admirable memoirs of Scrope, was yet that most generally adopted fifty years ago, because it was considered that compact and crystalline lavas could not have consolidated on a slope exceeding 1° or 2°. In 1858, however, Sir C. Lyell conclusively showed that in fact such lavas could consolidate at a considerable angle, even in some cases at more than 30°, and it is now generally admitted that though the beds of lava, &c., may have sustained a slight angular elevation since their deposition, still in the main, volcanic cones have acquired their form by the accumulation of lava and ashes ejected from one or more craters.

The problems presented by glaciers are of very great interest. In 1843 Agassiz and Forbes proved that the centre of a glacier, like that of a river, moves more rapidly than its sides. But how and why do glaciers move at all? Rendu, afterwards Bishop of Annecy, in 1841 endeavoured to explain the facts by supposing that glacier ice enjoys a kind of ductility. The "viscous theory" of glaciers was also adopted, and most ably advocated, by Forbes, who compared the condition of a glacier to that of the contents of a tar-barrel poured into a sloping channel. We have all, however, seen long narrow fissures, a mere fraction of an inch in width, stretching far across glaciers—a condition incompatible with the ordinary idea of viscosity. The phenomenon of regelation was afterwards applied to the explanation of glacier motion. An observation of Faraday's supplied the clue. He noticed in 1850 that when two pieces of thawing ice are placed together they unite by freezing at the place of contact. Following up this suggestion, Tyndall found that if he compressed a block of ice in a mould it could be made to assume any shape he pleased. A straight prism, for instance, placed in a groove and submitted to hydraulic pressure, was bent into a transparent semicircle of ice. These experiments seem to have proved that a glacial valley is a mould through which the ice is forced, and to which it will accommodate itself, while as Tyndall and Huxley also pointed out, the "veined structure of ice" is produced by pressure, in the same manner as the cleavage of slate rocks.

It was in the year 1842 that Darwin published his great work on "Coral Islands." The fringing reefs of coral presented no special difficulty. They could be obviously accounted for by an elevation of the land, so that the coral which had originally grown under water, had been raised above the sea-level. The circular or oval shape of so many reefs, however, each having a lagoon in the centre, closely surrounded by a deep ocean, and rising but a few feet above the sea-level, had long been a puzzle to the physical geographer. The favourite theory was that these were the summits of submarine volcanoes on which the coral had grown. But as the reef-making coral does not live at greater depths than about twenty-five fathoms, the immense number of these reefs formed an almost insuperable objection to this theory. The Laccadives and Maldives, for instance—meaning literally the "lac of islands" and the "thousand islands"—are a series of such

atolls, and it was impossible to imagine so great a number of craters, all so nearly of the same altitude. Darwin showed, moreover, that so far from the ring of corals resting on a corresponding ridge of rock, the lagoons, on the contrary, now occupy the place which was once the highest land. He pointed out that some lagoons, as, for instance, that of Vanikoro, contain an island in the middle; while other islands, such as Tahiti, are surrounded by a margin of smooth water, separated from the ocean by a coral reef. Now, if we suppose that Tahiti were to sink slowly, it would gradually approximate to the condition of Vanikoro; and if Vanikoro gradually sank, the central island would disappear, while on the contrary the growth of the coral might neutralise the subsidence of the reef, so that we should have simply an atoll, with its lagoon. The same considerations explain the origin of the "barrier reefs," such as that which runs, for nearly one thousand miles, along the north-east coast of Australia. Thus Darwin's theory explained the form and the approximate identity of altitude of these coral islands. But it did more than this, because it showed us that there were great areas in process of subsidence, which though slow, was of great importance in physical geography.[1]

Much information has also been acquired with reference to the abysses of the ocean, especially from the voyages of the *Porcupine* and the *Challenger*. The greatest depth yet recorded is near the Ladrone Islands, where a sounding of 4575 fathoms was obtained.

Ehrenberg long ago pointed out the similarity of the calcareous mud now accumulating in our recent seas to the Chalk, and showed that the green sands of the geologist are largely made up of casts of foraminifera. Clay, however, had been looked on, until the recent expeditions, as essentially a product of the disintegration of older rocks. Not only, however, are a large proportion of siliceous and calcareous rocks either directly or indirectly derived from material which has once formed a portion of living organisms, but Sir Wyville Thomson maintains that this is the case with some clays also. In that case the striking remark of Linnæus, that "fossils are not the children but the parents of rocks," will have received remarkable confirmation. I should have thought it, I confess, probable that these clays are, to a considerable extent, composed of volcanic dust.

It would appear that calcareous deposits resembling our chalk do not occur at a greater depth than 3000 fathoms; they have not been met with in the abysses of the ocean. Here the bottom consists of exceedingly fine clay, sometimes coloured red by oxide of iron, sometimes chocolate by manganese oxide, and containing with Foraminifera occasionally large numbers of siliceous Radiolaria. These strata seem to accumulate with extreme slowness; this is inferred from the comparative abundance of whales' bones and fishes' teeth; and from the presence of minute spherical particles, supposed by Mr. Murray to be of cosmic origin—in fact, to be the dust of meteorites, which in the course of ages have fallen on the ocean. Such particles no doubt occur over the whole surface of the earth, but on land they soon oxidise, and in shallow water they are covered up by other deposits. Another interesting result of recent deep-sea explorations has been to show that the depths of the ocean are no mere barren solitudes, as was until recent years confidently believed, but, on the contrary, present us many remarkable forms of life. We have, however, as yet but thrown here and there a ray of light down into the ocean abysses:—

> "Nor can so short a time sufficient be,
> To fathom the vast depths of Nature's sea."

In Astronomy, the discovery in 1845 of the planet Neptune, made independently and almost simultaneously by Adams and by Le Verrier, was certainly one of the very greatest triumphs of mathematical genius. Of the minor planets four only were known in 1831, whilst the number now on the roll amounts to 220. Many astronomers believe in the existence of an intra-mercurial planet or planets, but this is still an open question. The Solar System has also been enriched by the discovery of an inner ring to Saturn, of satellites to Mars, and of additional satellites to Saturn, Uranus and Neptune.

The most unexpected progress, however, in our astronomical knowledge during the past half-century has been due to Spectrum Analysis.

The dark lines in the spectrum were first seen by Wollaston, who noticed a few of them; but they were independently discovered by Fraunhofer, after whom they are justly named, and who, in 1814, mapped no fewer than 576. The first steps in "spectrum analysis," properly so called, were made by Sir J. Herschel, Fox Talbot, and by Wheatstone, in a paper read before this Association in 1835. The latter showed that the spectrum emitted by the incandescent vapour of metals was formed of bright lines, and that these lines, while, as he then supposed, constant for each metal, differed for different metals. "We have here," he said, "a mode of discriminating metallic bodies more readily than that of chemical examination, and which may hereafter be employed for useful purposes." Nay, not only can bodies thus be more readily discriminated, but, as we now know, the presence of extremely minute portions can be detected, the $\frac{1}{180000000}$th of a grain being in some cases easily perceptible.

It is also easy to see that the presence of any new simple substance might be detected, and in this manner already several new elements have been discovered, as I shall mention when we come to Chemistry.

But spectrum analysis has led to even grander and more unexpected triumphs. Fraunhofer himself noticed the coincidence between the double dark line D of the solar spectrum and a double line which he observed in the spectra of ordinary flames, while Stokes pointed out to Sir W. Thomson, who taught it in his lectures, that in both cases these lines were due to the presence of sodium. To Kirchhoff and Bunsen, however, is due the independent conception and the credit of having first systematically investigated the relation which exists between Fraunhofer's lines and the bright lines in the spectra of incandescent metals. In order to get some fixed measure by which they might determine and record the lines characterising any given substance, it occurred to them that they might use for comparison the spectrum of the sun. They accordingly arranged their spectroscope so that one-half of the slit was lighted by the sun, and the other by the luminous gases they proposed to examine. It immediately struck them that the bright lines in the one corresponded with the dark lines in the other—the bright line of sodium, for instance, with the line or rather lines D in the sun's spectrum. The conclusion was obvious. There was sodium in the sun! It must indeed have been a glorious moment when that thought flashed across them, and even by itself well worth all their labour.

Kirchhoff and Bunsen thus proved the existence in the sun of hydrogen, sodium, magnesium, calcium, iron, nickel, chromium, manganese, titanium, and cobalt; since which Ångström, Thalén, and Lockyer have considerably increased the list.

But it is not merely the chemistry of the heavenly bodies on which light is thrown by the spectroscope; their physical structure and evolutional history are also illuminated by this wonderful instrument of research.

It used to be supposed that the sun was a dark body enveloped in a luminous atmosphere. The reverse now appears to be the truth. The body of the sun, or photosphere, is intensely brilliant; round it lies the solar atmosphere of comparatively cool gases, which cause the dark lines in the spectrum; thirdly, a chromosphere,—a sphere principally of hydrogen, jets of which are said sometimes to reach to a height of 100,000 miles or more, into the outer coating or corona, the nature of which is still very doubtful.

Formerly the red flames which represent the higher regions of the chromosphere could be seen only on the rare occasions of a total solar eclipse. Janssen and Lockyer, by the application of the spectroscope, have enabled us to study this region of the sun at all times.

It is, moreover, obvious that the powerful engine of investigation afforded us by the spectroscope is by no means confined to the substances which form part of our system. The incandescent body can thus be examined, no matter how great its distance, so long only as the light is strong enough. That this method was theoretically applicable to the light of the stars was indeed obvious, but the practical difficulties were very great. Sirius, the brightest of all, is, in round numbers, a hundred millions of millions of miles from us; and, though as big as sixty of our suns, his light when it reaches us, after a journey of sixteen years, is at most one two-thousand-millionth part as bright. Nevertheless as long ago as 1815 Fraunhofer recognised the fixed lines in the light of four of the stars, and in 1863 Miller and Huggins in our own country, and Rutherford in America, succeeded in determining the dark lines in the spectrum of some of the brighter stars, thus showing that these beautiful and mysterious lights contain many of the material substances with

[1] I ought to mention that Darwin's views have recently been questioned by Semper and Murray.

which we are familiar. In Aldebaran, for instance, we may infer the presence of hydrogen, sodium, magnesium, iron, calcium, tellurium, antimony, bismuth, and mercury; some of which are not yet known to occur in the sun. As might have been expected, the composition of the stars is not uniform, and it would appear that they may be arranged in a few well-marked classes, indicating differences of temperature, or in other words, of age. Some recent photographic spectra of stars obtained by Huggins go very far to justify this view.

Thus we can make the stars teach us their own composition with light which started from its source before we were born—light older than our Association itself.

But spectrum analysis has even more than this to tell us. The old methods of observation could determine the movements of the stars so far only as they were transverse to us; they afforded no means of measuring motion either directly towards or away from us. Now Döppler suggested in 1841 that the colours of the stars would assist us in this respect, because they would be affected by their motion to and from the earth, just as a steam-whistle is raised or lowered as it approaches or recedes from us. Every one has observed that if a train whistles as it passes us, the sound appears to alter at the moment the engine goes by. This arises, of course, not from any change in the whistle itself, but because the number of vibrations which reach the ear in a given time are increased by the speed of the train as it approaches, and diminished as it recedes. So like the sound, the colour would be affected by such a movement; but Döppler's method was practically inapplicable, because the amount of effect on the colour would be utterly insensible; and even if it were otherwise the method could not be applied, because, as we did not know the true colour of the stars, we have no datum line by which to measure.

A change of refrangibility of light, however, does occur in consequence of relative motion, and Huggins successfully applied the spectroscope to solve the problem. He took in the first place the spectroscope of Sirius, and chose a line known as F, which is due to hydrogen. Now, if Sirius was motionless, or rather if it retained a constant distance from the earth, the line F would occupy exactly the same position in the spectrum of Sirius, as in that of the sun. On the contrary, if Sirius were approaching or receding from us, this line would be slightly shifted either towards the blue or red end of the spectrum. He found that the line had moved very slightly towards the red, indicating that the distance between us and Sirius is increasing at the rate of about twenty miles a second. So also Betelgeux, Rigel, Castor, and Regulus are increasing their distance; while, on the contrary, that of others, as for instance of Vega, Arcturus, and Pollux, is diminishing. The results obtained by Huggins on about twenty stars have since been confirmed and extended by Mr. Christie, now Astronomer-Royal in succession to Sir G. Airy, who has long occupied the post with so much honour to himself and advantage to science.

To examine the spectrum of a shooting star would seem even more difficult; yet Alexander Herschel has succeeded in doing so, and finds that their nuclei are incandescent solid bodies; he has recognised the lines of potassium, sodium, lithium, and other substances, and considers that the shooting stars are bodies similar in character and composition to the stony masses which sometimes reach the earth as aërolites.

No element has yet been found in any meteorite, which was not previously known as existing in the earth, but the phenomena which they exhibit indicate that they must have been formed under conditions very different from those which prevail on the earth's surface. I may mention, for instance, the peculiar form of crystallised silica, called by Maskelyne, Asmanite; and the whole class of meteorites, consisting of iron generally alloyed with nickel, which Daubrée terms Holosiderites. The interesting discovery, however, by Nordenskjöld, in 1870, at Ovifak, of a number of blocks of iron alloyed with nickel and cobalt, in connection with basalts containing disseminated iron, has, in the words of Judd, "afforded a very important link, placing the terrestrial and extra-terrestrial rocks in closer relations with one another."

We have as yet no sufficient evidence to justify a conclusion as to whether any substances exist in the heavenly bodies which do not occur in our earth, though there are many lines which cannot yet be satisfactorily referred to any terrestrial element. On the other hand, some substances which occur on our earth have not yet been detected in the sun's atmosphere.

Such discoveries as these seemed, not long ago, entirely beyond our hopes. M. Comte, indeed, in his "Cours de Philosophie Positive," as recently as 1842, laid it down as an axiom regarding the heavenly bodies, that "Nous concevons la possibilité de déterminer leurs formes, leurs distances, leurs grandeurs et leurs mouvements, tandis que nous ne saurions jamais étudier par aucun moyen leur composition chimique ou leur structure minéralogique." Yet within a few years this supposed impossibility has been actually accomplished, showing how unsafe it is to limit the possibilities of science.

It is hardly necessary to point out that, while the spectrum has taught us so much, we have still even more to learn. Why should some substances give few, and others many, lines? Why should the same substance give different lines at different temperatures? What are the relations between the lines and the physical or chemical properties?

We may certainly look for much new knowledge of the hidden actions of atoms and molecules from future researches with the spectroscope. It may even, perhaps, teach us to modify our views of the so-called simple substances. Prout long ago, struck by the remarkable fact that nearly all atomic weights are simple multiples of the atomic weight of hydrogen, suggested that hydrogen must be the primordial substance. Brodie's researches also naturally fell in with the supposition that the so-called simple substances are in reality complex, and that their constituents occur separately in the hottest regions of the solar atmosphere. Lockyer considers that his researches lend great probability to this view. The whole subject is one of intense interest, and we may rejoice that it is occupying the attention, not only of such men as Abney, Dewar, Hartley, Liveing, Roscoe and Schuster in our own country, but also of many foreign observers.

When geology so greatly extended our ideas of past time, the continued heat of the sun became a question of greater interest than ever. Helmholtz has shown that, while adopting the nebular hypothesis, we need not assume that the nebulous matter was originally incandescent; but that its present high temperature may be, and probably is, mainly due to gravitation between its parts. It follows that the potential energy of the sun is far from exhausted, and that with continued shrinking it will continue to give out light and heat, with little, if any, diminution for several millions of years.

Like the sand of the sea, the stars of heaven have ever been used as effective symbols of number, and the improvements in our methods of observation have added fresh force to our original impressions. We now know that our earth is but a fraction of one out of at least 75,000,000 worlds.

But this is not all. In addition to the luminous heavenly bodies, we cannot doubt that there are countless others, invisible to us from their greater distance, smaller size, or feebler light; indeed we know that there are many dark bodies which now emit no light or comparatively little. Thus in the case of Procyon, the existence of an invisible body is proved by the movement of the visible star. Again I may refer to the curious phenomena presented by Algol, a bright star in the head of Medusa. This star shines without change for two days and thirteen hours; then, in three hours and a half, dwindles from a star of the second to one of the fourth magnitude; and then, in another three and a half hours, reassumes its original brilliancy. These changes seem certainly to indicate the presence of an opaque body, which intercepts at regular intervals a part of the light emitted by Algol.

Thus the floor of heaven is not only "thick inlaid with patines of bright gold," but studded also with extinct stars; once probably as brilliant as our own sun, but now dead and cold, as Helmholtz tells us that our sun itself will be, some seventeen millions of years hence.

The general result of astronomical researches has been thus eloquently summed up by Proctor:—"The sidereal system is altogether more complicated and more varied in structure than has hitherto been supposed; in the same region of the stellar depths co-exist stars of many orders of real magnitude; all orders of nebulæ, gaseous or stellar, planetary, ring-formed, elliptical, and spiral, exist within the limits of the galaxy; and lastly, the whole system is alive with movements, the laws of which may one day be recognised, though at present they appear too complex to be understood."

We can, I think, scarcely claim the establishment of the undulatory theory of light as falling within the last fifty years; for though Brewster, in his "Report on Optics," published in our first volume, treats the question as open, and expresses himself

still unconvinced, he was, I believe, almost alone in his preference for the emission theory. The phenomena of interference, in fact, left hardly any—if any—room for doubt, and the subject was finally set at rest by Foucault's celebrated experiments in 1850. According to the undulatory theory the velocity of light ought to be greater in air than in water, while if the emission theory were correct the reverse would be the case. The velocity of light—186,000 miles in a second—is, however, so great that, to determine its rate in air, as compared with that in water, might seem almost hopeless. The velocity in air was, nevertheless, determined by Fizeau in 1849, by means of a rapidly revolving wheel. In the following year Foucault, by means of a revolving mirror, demonstrated that the velocity of light is greater in air than in water—thus completing the evidence in favour of the undulatory theory of light.

The idea is now gaining ground, that, as maintained by Clerk-Maxwell, light itself is an electro-magnetic disturbance, the luminiferous ether being the vehicle of both light and electricity.

Wünsch, as long ago as 1792, had clearly shown that the three primary colours were red, green, and violet; but his results attracted little notice, and the general view used to be that there were seven principal colours—red, orange, yellow, green, blue, indigo, and violet; four of which—namely orange, green, indigo, and violet—were considered to arise from mixtures of the other three. Red, yellow, and blue were therefore called the primary colours, and it was supposed that in order to produce white light these three colours must always be present.

Helmholtz, however, again showed, in 1852, that a colour to our unaided eyes identical with white, was produced by combining yellow with indigo. At that time yellow was considered to be a simple colour, and this, therefore, was regarded as an exception to the general rule, that a combination of three simple colours is required to produce white. Again, it was, and indeed still is, the general impression that a combination of blue and yellow makes green. This, however, is entirely a mistake. Of course we all know that yellow paint and blue paint make green paint; but this results from absorption of light by the semi-transparent solid particles of the pigments, and is not a mere mixture of the colours proceeding unaltered from the yellow and the blue particles: moreover, as can easily be shown by two sheets of coloured paper and a piece of window glass, blue and yellow light, when combined, do not give a trace of green, but if pure would produce the effect of white. Green, therefore, is after all not produced by a mixture of blue and yellow. On the other hand Clerk-Maxwell proved in 1860 that yellow could be produced by a mixture of red and green, which put an end to the pretension of yellow to be considered a primary element of colour. From these and other considerations it would seem, therefore, that the three primary colours—if such an expression be retained—are red, green, and violet.

The existence of rays beyond the violet, though almost invisible to our eyes, had long been demonstrated by their chemical action. Stokes, however, showed in 1852 that their existence might be proved in another manner, for that there are certain substances which, when excited by them, emit light visible to our eyes. To this phenomenon he gave the name of fluorescence. At the other end of the spectrum Abney has recently succeeded in photographing a large number of lines in the infra-red portion, the existence of which was first proved by Sir William Herschel.

From the rarity, and in many cases the entire absence, of reference to blue, in ancient literature, Geiger—adopting and extending a suggestion first thrown out by Mr. Gladstone—has maintained that, even as recently as the time of Homer, our ancestors were blue blind. Though for my part I am unable to adopt this view, it is certainly very remarkable that neither the Rigveda, which consists almost entirely of hymns to heaven, nor the Zendavesta, the Bible of the Parsees or fire-worshippers, nor the Old Testament, nor the Homeric poems, ever allude to the sky as blue.

On the other hand, from the dawn of poetry, the splendours of the morning and evening skies have excited the admiration of mankind. As Ruskin says, in language almost as brilliant as the sky itself, the whole heaven, "from the zenith to the horizon, becomes one molten, mantling sea of colour and fire; every black bar turns into massy gold, every ripple and wave into unsullied shadowless crimson, and purple, and scarlet, and colours for which there are no words in language, and no ideas in the mind—things which can only be conceived while they are visible; the intense hollow blue of the upper sky melting through it all, showing here deep, and pure, and lightness; there, modulated by the filmy, formless body of the transparent vapour, till it is lost imperceptibly in its crimson and gold."

But what is the explanation of these gorgeous colours? why is the sky blue? and why are the sunrise and sunset crimson and gold? It may be said that the air is blue, but if so how can the clouds assume their varied tints? Brücke showed that very minute particles suspended in water are blue by reflected light. Tyndall has taught us that the blue of the sky is due to the reflection of the blue rays by the minute particles floating in the atmosphere. Now if from the white light of the sun the blue rays are thus selected, those which are transmitted will be yellow, orange, and red. Where the distance is short the transmitted light will appear yellowish. But as the sun sinks towards the horizon the atmospheric distance increases, and consequently the number of the scattering particles. They weaken in succession the violet, the indigo, the blue, and even disturb the proportions of green. The transmitted light under such circumstances must pass from yellow through orange to red, and thus, while we at noon are admiring the deep blue of the sky, the same rays, robbed of their blue, are elsewhere lighting up the evening sky with all the glories of sunset.

Another remarkable triumph of the last half-century has been the discovery of photography. At the commencement of the century Wedgwood and Davy observed the effect produced by throwing the images of objects on paper or leather prepared with nitrate of silver, but no means were known by which such images could be fixed. This was first effected by Niepce, but his processes were open to objections which prevented them from coming into general use, and it was not till 1839 that Daguerre invented the process which was justly named after him. Very soon a further improvement was effected by our countryman Talbot. He not only fixed his "Talbotypes" on paper—in itself a great convenience—but, by obtaining a negative, rendered it possible to take off any number of positive, or natural, copies from one original picture.

We owe to Wheatstone the conception that the idea of solidity is derived from the combination of two pictures of the same object in slightly different perspective. This he proved in 1833 by drawing two outlines of some geometrical figure or other simple object, as they would appear to either eye respectively, and then placing them so that they might be seen, one by each eye. The "stereoscope," thus produced, has been greatly popularised by photography.

For 2000 years the art of lighting had made little if any progress. Until the close of the last century, for instance, our lighthouses contained mere fires of wood or coal, though the construction had vastly improved. The Eddystone lighthouse, for instance, was built by Smeaton in 1759; but for forty years its light consisted in a row of tallow candles stuck in a hoop. The Argand lamp was the first great improvement, followed by gas, and in 1863 by the electric light.

Just as light was long supposed to be due to the emission of material particles, so heat was regarded as a material, though ethereal, substance, which was added to bodies when their temperature was raised.

Davy's celebrated experiment of melting two pieces of ice by rubbing them against one another in the exhausted receiver of an air-pump had convinced him that the cause of heat was the motion of the invisible particles of bodies, as had been long before suggested by Newton, Boyle, and Hooke. Rumford and Young also advocated the same view. Nevertheless, the general opinion, even until the middle of the present century, was that heat was due to the presence of a subtle fluid known as "caloric," a theory which is now entirely abandoned.

The determination of the mechanical equivalent of heat is mainly due to the researches of Mayer and Joule. Mayer, in 1842, pointed out the mechanical equivalent of heat as a fundamental datum to be determined by experiment. Taking the heat produced by the condensation of air as the equivalent of the work done in compressing the air, he obtained a numerical value of the mechanical equivalent of heat. There was, however, in these experiments, one weak point. The matter operated on did not go through a cycle of changes. He assumed that the production of heat was the only effect of the work done in compressing the air. Joule had the merit of being the first to meet this possible source of error. He ascertained that a weight of 1 lb. would have to fall 772 feet in order to raise the temperature of 1 lb. of water by 1° Fahr. Hirn

subsequently attacked the problem from the other side, and showed that if all the heat passing through a steam-engine was turned into work, for every degree Fahr. added to the temperature of a pound of water, enough work could be done to raise a weight of 1 lb. to a height of 772 feet. The general result is that, though we cannot create energy, we may help ourselves to any extent from the great storehouse of nature. Wind and water, the coal-bed and the forest, afford man an inexhaustible supply of available energy.

It used to be considered that there was an absolute break between the different states of matter. The continuity of the gaseous, liquid, and solid conditions was first demonstrated by Andrews in 1862.

Oxygen and nitrogen have been liquefied independently and at the same time by Cailletet and Raoul Pictet. Cailletet also succeeded in liquefying air, and soon afterwards hydrogen was liquefied by Pictet under a pressure of 650 atmospheres, and a cold of 170° Cent. below zero. It even became partly solidified, and he assures us that it fell on the floor with "the shrill noise of metallic hail." Thus then it was shown experimentally that there are no such things as absolutely permanent gases.

The kinetic theory of gases, now generally accepted, refers the elasticity of gases to a motion of translation of their molecules, and we are assured that in the case of hydrogen at a temperature of 60° Fahr. they move at an average rate of 6225 feet in a second; while as regards their size, Loschmidt, who has since been confirmed by Stoney and Sir W. Thomson, calculates that each is at most 1-50000000th of an inch in diameter.

We cannot, it would seem at present, hope for any increase of our knowledge of atoms by any improvement in the microscope. With our present instruments we can perceive lines ruled on glass of 1-90,000th of an inch apart. But, owing to the properties of light itself, the fringes due to interference begin to produce confusion at distances of 1-74,000. It would seem then that, owing to the physical characters of light, we can, as Sorby has pointed out, scarcely hope for any great improvement so far as the mere visibility of structure is concerned, though in other respects no doubt much may be hoped for. At the same time, Dallinger and Royston Pigott have shown that, so far as the mere presence of simple objects is concerned, bodies of even smaller dimensions can be perceived.

Sorby is of opinion that in a length of 1-80,000th of an inch there would probably be from 500 to 2000 molecules—500, for instance, in albumen and 2000 in water. Even, then, if we could construct microscopes far more powerful than any we now possess, they would not enable us to obtain by direct vision any idea of the ultimate molecules of matter. Sorby calculates that the smallest sphere of organic matter which could be clearly defined with our most powerful microscopes would contain many millions of molecules of albumen and water, and it follows that there may be an almost infinite number of structural characters in organic tissues, which we can at present foresee no mode of examining.

Electricity in the year 1831 may be considered to have just been ripe for its adaptation to practical purposes; it was but a few years previously, in 1819, that Oersted had discovered the deflective action of the current on the magnetic needle, that Ampère had laid the foundation of electro-dynamics, that Schweizzer had devised the electric coil or multiplier, and that Sturgeon had constructed the first electro-magnet. It was in 1831 that Faraday, the prince of pure experimentalists, announced his discoveries of voltaic induction and magneto-electricity, which with the other three discoveries constitute the principles of nearly all the telegraph instruments now in use; and in 1834 our knowledge of the nature of the electric current had been much advanced by the interesting experiment of Sir Charles Wheatstone, proving the velocity of the current in a metallic conductor to approach that of the wave of light.

Practical applications of these discoveries were not long in coming to the fore, and the first telegraph line on the Great Western Railway from Paddington to West Drayton was set up in 1838. In America Morse is said to have commenced to develop his recording instrument between the years 1832 and 1837.

In 1851, submarine telegraphy became an accomplished fact through the successful establishment of telegraphic communication between Dover and Calais. Submarine lines followed in rapid succession, crossing the English Channel and the German Ocean, threading their way through the Mediterranean, Black and Red Seas, until in 1866, after two abortive attempts telegraphic communication was successfully established between the Old and New Worlds, beneath the Atlantic Ocean.

Duplex and quadruplex telegraphy, one of the most striking achievements of modern telegraphy, the result of the labours of several inventors, should not be passed over in silence. It not only serves for the simultaneous communication of telegraphic intelligence in both directions, but renders it possible for four instruments to be worked irrespectively of one another, through one and the same wire connecting to distant places.

Another more recent and perhaps still more wonderful achievement in modern telegraphy is the invention of the telephone and microphone, by means of which the human voice is transmitted through the electric conductor, by mechanism that imposes through its extreme simplicity. In this connection the names of Reiss, Graham Bell, Edison, and Hughes are those chiefly deserving to be recorded.

By the electric transmission of power, we may hope some day to utilise at a distance such natural sources of energy as the Falls of Niagara, and to work our cranes, lifts, and machinery of every description by means of sources of power arranged at convenient centres. To these applications the brothers Siemens have more recently added the propulsion of trains by currents passing through the rails, the fusion in considerable quantities of highly refractory substances, and the use of electric centres of light in horticulture as proposed by Werner and William Siemens. By an essential improvement by Faure of the Planté Secondary Battery, the problem of storing electrical energy appears to have received a practical solution, the real importance of which is clearly proved by Sir W. Thomson's recent investigation of the subject

It would be difficult to assign the limits to which this development of electrical energy may not be rendered serviceable for the purposes of man.

As regards Mathematics I have felt that it would be impossible for me, even with the kindest help, to write anything myself. Mr. Spottiswoode, however, has been so good as to supply me with the following memorandum.

In a complete survey of the progress of science during the half-century which has intervened between our first and our present meeting, the part played by mathematics would form no insignificant feature. To those indeed who are outside its enchanted circle it is difficult to realise the intense intellectual energy which actuates its devotees, or the wide expanse over which that energy ranges.

In the extension of mathematics it has happened more than once that laws have been established so simple in form, and so obvious in their necessity, as scarcely to require proof. And yet their application is often of the highest importance in checking conclusions which have been drawn from other considerations, as well as in leading to conclusions which, without their aid, might have been difficult of attainment. The same thing has occurred also in physics; and notably in the recognition of what has been termed the "Law of the Conservation of Energy."

Energy has been defined to be "The capacity, or power, of any body, or system of bodies, when in a given condition, to do a measurable quantity of work." Such work may either change the condition of the bodies in question, or it may affect other bodies; but in either case energy is expended by the agent upon the recipient in performance of the work. The law then states that the total amount of energy in the agents and recipients taken together remains unaltered by the changes in question.

Now the principle on which the law depends is this: "that every kind of change among the bodies may be expressed numerically in one standard unit of change," viz., work done, in such wise that the result of the passage of any system from one condition to another may be calculated by mere additions and subtractions, even when we do not know how the change came about.

The history of a discovery, or invention, so simple at first sight, is often found to be more complicated the more thoroughly it is examined. That which at first seems to have been due to a single mind proves to have been the result of the successive action of many minds. Attempts more or less successful in the same direction are frequently traced out; and even unsuccessful efforts may not have been without influence on minds turned towards the same object. Lastly also, germs of thought, originally not fully understood, sometimes prove in the end to have been the first stages of growth towards ultimate fruit. The history of the law of the conservation of energy forms no

exception to this order of events. There are those who discern even in the writings of Newton expressions which show that he was in possession of some ideas which, if followed out in a direct line of thought, would lead to those now entertained on the subjects of energy and of work. But however this may be, and whosoever might be reckoned among the earlier contributors to the general subject of energy, and to the establishment of its laws, it is certain that within the period of which I am now speaking, the names of Séguin, Clausius, Helmholtz, Mayer, and Colding, on the Continent, and those of Grove, Joule, Rankine, and Thomson, in this country, will always be associated with this great work.

Prof. Frankland has been so good as to draw up for me the following account of the progress of Chemistry during the last half-century.

Most of the elements had been discovered before 1830, the majority of the rarer elements since the beginning of the century. In addition to these the following five have been discovered, three of them by Mosander, viz.:—lanthanum in 1839, didymium in 1842, and erbium in 1843. Ruthenium was discovered by Claus in 1843, and niobium by Rose in 1844. Spectrum analysis has added five to the list, viz.:—Cæsium and rubidium, which were discovered by Bunsen and Kirchhoff in 1860; thallium, by Crookes in 1861; indium, by Reich and Richter in 1863; and gallium, by Lecoq de Boisbaudran in 1875.

In organic chemistry the views most generally held about the year 1830 were expressed in the radical theory of Berzelius. This theory, which was first stated in its electro-chemical and dualistic form by its author in 1817, received a further development at his hands in 1834 after the discovery of the benzoyl-radical by Liebig and Wöhler. In the same year (1834), however, a discovery was made by Dumas, which was destined profoundly to modify the electro-chemical portion of the theory, and even to overthrow the form of it put forth by Berzelius. Dumas showed that an electro-negative element, such as chlorine, might replace, atom for atom, an electro-positive element like hydrogen, in some cases without much alteration in the character of the compound. This law of substitution has formed a necessary portion of every chemical theory which has been proposed since its discovery, and its importance has increased with the progress of the science.

Chemists have been engaged in determining, by means of decompositions, the molecular architecture, or *constitution* as it is called, of various compounds, natural and artificial, and in verifying by synthesis the correctness of the views thus arrived at.

It was long supposed that an impassable barrier existed between inorganic and organic substances: that the chemist could make the former in his laboratory, while the latter could only be produced in the living bodies of animals or plants,—requiring for their construction not only chemical attractions, but a supposed "vital force." It was not until 1828 that Wöhler broke down this barrier by the synthetic production of urea, and since his time this branch of science in the hands of Hofmann has made great strides.

In connection with the rectification of the atomic weights it may be mentioned that a so called natural system of the elements has been introduced by Mendelejeff (1869), in which the properties of the elements appear as a periodic function of their atomic weights. By the aid of this system it has been possible to predict the properties and atomic weights of undiscovered elements, and in the case of known elements to determine many atomic weights which had not been fixed by any of the usual methods. Several of these predictions have been verified in a remarkable manner. A periodicity in the atomic weights of elements belonging to the same class had been pointed out by Newlands about four years before the publication of Mendelejeff's memoir.

In Mechanical Science the progress has not been less remarkable than in other branches. Indeed to the improvements in mechanics we owe no small part of our advance in practical civilisation, and of the increase of our national prosperity during the last fifty years.

This immense development of mechanical science has been to a great extent a consequence of the new processes which have been adopted in the manufacture of iron, for the following data with reference to which I am indebted to Captain Douglas Galton. About 1830, Neilson introduced the Hot Blast in the smelting of iron. At first a temperature of 600° or 700° Fahrenheit was obtained, but Cowper subsequently applied Siemens' regenerative furnace for heating the blast, chiefly by means of fumes from the black furnace, which were formerly wasted; and the temperature now practically in use is as much as 1400° or even more: the result is a very great economy of fuel and an increase of the output.

Bessemer, by his brilliant discovery, which he first brought before the British Association at Cheltenham in 1856, showed that Iron and Steel could be produced by forcing currents of atmospheric air through fluid pig metal, thus avoiding for the first time the intermediate process of puddling iron, and converting it by cementation into steel. These changes, by which steel can be produced direct from the blast furnace instead of by the more cumbersome processes formerly in use, have been followed by improvements in manipulation of the metal.

The inventions of Cort and others were known long before 1830, but we were then still without the most powerful tool in the hands of the practical metallurgist, viz., Nasmyth's steam hammer.

Steel can be produced as cheaply as iron was formerly; and its substitution for iron as railway material and in shipbuilding, has resulted in increased safety in railway travelling, as well as in economy, from its vastly greater durability.

The introduction of iron, has, moreover, had a vast influence on the works of both the civil and military engineer. Before 1830, Telford had constructed an iron suspension turnpike-road bridge of 560 feet over the Menai Straits; but this bridge was not adapted to the heavy weights of locomotive engines. At the present time, with steel at his command, Mr. Fowler is engaged in carrying out the design for a railway bridge over the Forth, of two spans of 1700 feet each; that is to say, of nearly one-third of a mile in length.

But it is in railroads, steamers, and the electric telegraph, that the progress of mechanical science has most strikingly contributed to the welfare of man. To the latter I have already referred.

As regards railways, the Stockton and Darlington Railway was opened in 1825, but the Liverpool and Manchester Railway, perhaps the first truly passenger line, dates from 1830, while the present mileage of railways is over 200,000 miles, costing nearly 4,000,000,000*l*. sterling. It was not until 1838 that the *Sirius* and *Great Western* first steamed across the Atlantic. The steamer, in fact, is an excellent epitome of the progress of the half-century; the paddle has been superseded by the screw; the compound has replaced the simple engine; wood has given place to iron, and iron in its turn to steel. The saving in dead weight, by this improvement alone, is from 10 to 16 per cent. The speed has been increased from 9 knots to 15, or even more. Lastly, the steam-pressure has been increased from less than 5 lbs. to 70 lbs. per square inch, while the consumption of coal has been brought down from 5 or 6 lbs. per horse-power to less than 2. It is a remarkable fact that not only is our British shipping rapidly on the increase, but it is increasing relatively to that of the rest of the world. In 1860 our tonnage was 5,700,000 against 7,200,000; while it may now be placed as 8,500,000 against 8,200,000; so that considerably more than half the whole shipping of the world belongs to this country.

If I say little with reference to Economic Science and Statistics, it is because time, not materials, are wanting.

I scarcely think that in the present state of the question I can be accused of wandering into politics if I observe that the establishment of the doctrine of free trade as a scientific truth falls within the period under review.

In Education some progress has been made towards a more rational system. When I was at a public school, neither science, modern languages, nor arithmetic formed any part of the school system. This is now happily changed. Much, however, still remains to be done. Too little time is still devoted to French and German, and it is much to be regretted that even in some of our best schools they are taught as dead languages. Lastly, with few exceptions, only one or two hours on an average are devoted to science. We have, I am sure, none of us any desire to exclude, or discourage, literature. What we ask is that, say, six hours a week each should be devoted to mathematics, modern languages, and science, an arrangement which would still leave twenty hours for Latin and Greek. I admit the difficulties which schoolmasters have to contend with; nevertheless, when we consider what science has done and is doing for us, we cannot but consider that our present system of education is, in the words of the Duke of Devonshire's Commission, little less than a national misfortune.

In Agriculture the changes which have occurred in the period since 1831 have been immense. The last half century has witnessed the introduction of the modern system of subsoil drainage founded on the experiments of Smith of Deanston. The thrashing and drilling machines were the most advanced forms of machinery in use in 1831. Since then there have been introduced the steam-plough; the mowing-machine; the reaping-machine, which not only cuts the corn but binds it into sheaves; while the steam-engine thrashes out the grain and builds the ricks. Science has thus greatly reduced the actual cost of labour, and yet it has increased the wages of the labourer.

It was to the British Association, at Glasgow in 1841, that Baron Liebig first communicated his work "On the Application of Chemistry to Vegetable Physiology," while we have also from time to time received accounts of the persevering and important experiments which Mr. Lawes, with the assistance of Dr. Gilbert, has now carried on for more than forty years at Rothamsted, and which have given so great an impulse to agriculture by directing attention to the principles of cropping, and by leading to the more philosophical application of manures.

I feel that in quitting Section F so soon, I owe an apology to our fellow-workers in that branch of science, but I doubt not that my shortcomings will be more than made up for by the address of their excellent President, Mr. Grant-Duff, whose appointment to the governorship of Madras, while occasioning so sad a loss to his friends, will unquestionably prove a great advantage to India, and materially conduce to the progress of science in that country.

Moreover, several other subjects of much importance, which might have been referred to in connection with these latter Sections, I have already dealt with under their more purely scientific aspect.

Indeed, one very marked feature in modern discovery is the manner in which distinct branches of science have thrown, and are throwing, light on one another. Thus the study of geographical distribution of living beings, to the knowledge of which our late general secretary, Mr. Sclater, has so greatly contributed, has done much to illustrate ancient geography. The existence of high northern forms in the Pyrenees and Alps points to the existence of a period of cold when Arctic species occupied the whole of habitable Europe. Wallace's line—as it has been justly named after that distinguished naturalist—points to the very ancient separation between the Malayan and Australian regions; and the study of corals has thrown light upon the nature and significance of atolls and barrier-reefs.

In studying the antiquity of man, the archæologist has to invoke the aid of the chemist, the geologist, the physicist and the mathematician. The recent progress in astronomy is greatly due to physics and chemistry. In geology the composition of rocks is a question of chemistry; the determination of the boundaries of the different formations falls within the limits of geography; while palæontology is the biology of the past.

And now I must conclude. I fear I ought to apologise to you for keeping you so long, but still more strongly do I wish to express my regret that there are almost innumerable researches of great interest and importance which fall within the last fifty years (many even among those with which our Association has been connected) to which I have found it impossible to refer. Such for instance are, in biology alone, Owen's memorable report on the homologies of the vertebrate skeleton, Carpenter's laborious researches on the microscopic structure of shells, the reports on marine zoology by Allman, Forbes, Jeffreys, Spence Bate, Norman, and others; on Kent's Cavern by Pengelly; those by Duncan on corals; Woodward on crustaceæ; Carruthers, Williamson, and others on fossil botany, and many more. Indeed no one who has not had occasion to study the progress of science throughout its various departments can have any idea how enormous—how unprecedented—the advance has been.

Though it is difficult, indeed impossible, to measure exactly the extent of the influence exercised by this Association, no one can doubt that it has been very considerable. For my own part, I must acknowledge with gratitude how much the interest of my life has been enhanced by the stimulus of our meetings, by the lectures and memoirs to which I have had the advantage of listening, and above all, by the many friendships which I owe to this Association.

Summing up the principal results which have been attained in the last half-century we may mention (over and above the accumulation of facts) the theory of evolution, the antiquity of man, and the far greater antiquity of the world itself; the correlation of physical forces and the conservation of energy; spectrum analysis and its application to celestial physics; the higher algebra and the modern geometry; lastly, the innumerable applications of science to practical life—as, for instance, in photography, the locomotive engine, the electric telegraph, the spectroscope, and most recently the electric light and the telephone.

To science, again, we owe the idea of progress. The ancients, says Bagehot, "had no conception of progress; they did not so much as reject the idea; they did not even entertain it." It is not, I think, now going too far to say that the true test of the civilisation of a nation must be measured by its progress in science. It is often said, however, that great and unexpected as the recent discoveries have been, there are certain ultimate problems which must ever remain unsolved. For my part I would prefer to abstain from laying down any such limitations. When Park asked the Arabs what became of the sun at night, and whether the sun was always the same, or new each day, they replied that such a question was childish, and entirely beyond the reach of human investigation. I have already mentioned that, even as lately as 1842, so high an authority as Comte treated as obviously impossible and hopeless any attempt to determine the chemical composition of the heavenly bodies. Doubtless there are questions, the solution of which we do not as yet see our way even to attempt; nevertheless the experience of the past warns us not to limit the possibilities of the future.

But however this may be, though the progress made has been so rapid, and though no similar period in the world's history has been nearly so prolific of great results, yet, on the other hand, the prospects of the future were never more encouraging. We must not, indeed, shut our eyes to the possibility of failure; the temptation to military ambition; the tendency to over-interference by the State; the spirit of anarchy and socialism; these and other elements of danger may mar the fair prospects of the future. That they will succeed, however, in doing so, I cannot believe. I cannot but feel confident hope that fifty years hence, when perhaps the city of York may renew its hospitable invitation, my successor in this chair—more competent, I trust, than I have been to do justice to so grand a theme—will have to record a series of discoveries even more unexpected and more brilliant than those which I have, I fear so imperfectly, attempted to bring before you this evening. For one great lesson which science teaches is, how little we yet know, and how much we have still to learn.

SECTION B

CHEMICAL SCIENCE

OPENING ADDRESS BY PROF. A. W. WILLIAMSON, PH.D., LL.D., F.R.S., V.P.C.S., PRESIDENT OF THE SECTION

On the Growth of the Atomic Theory

IT has been thought desirable that on the occasion of this half-centenary celebration of the foundation of this great Association, some notice should be presented to the members of what has been doing in the respective branches of science during the period of our activity; and I have, accordingly, traced out for your consideration a very imperfect sketch of the theories which guided chemical inquiry at the beginning of that period, and of the leading changes which have been wrought in them by fifty years' work.

There is perhaps hardly any branch of science which during the last fifty years has made such great and steady progress as chemistry. Let any one compare recent dictionaries of the science (including the bulky supplements, which contain a record of the chief discoveries made while the body of the work was being compiled) with a treatise of chemistry fifty years old. Let him compare a published record of one year's progress of the science fifty years ago with one of modern date. Let him compare, as far as may be possible, the number of men who formerly devoted their whole time and energy to the advancement of chemistry, or who were engaged in industrial pursuits involving a knowledge of the science, with the corresponding number nowadays. Let him count up the services which chemistry had rendered to common life at the commencement of the epoch with those which it has now to show.

Everywhere he will see marvellous evidences of increasing growth. But if he be a reflecting man, he will not be satisfied with wondering at results: he will endeavour to trace them to

their causes, and to discover the guiding principles which have brought them about: he will try to derive, from a knowledge of those guiding principles, a perception of the means by which such progress can best be continued and extended—how it can be most effectively directed to the benefit of his fellow-men.

It is on this aspect of the question that I propose to address you to-day.

The process of scientific investigation includes a great variety of operations, which may be considered under three headings, mental, sensual, and physical. We think, we observe, and we work with our hands. In planning a new experiment we call to mind what is known of the phenomena in question, and form an opinion as to what is likely to happen under conditions somewhat different from those which existed in previous experiments. We regulate by careful observations the necessary manual operations, so as to obtain with accuracy the desired conditions for the new experiment, and we observe attentively the changes which take place in the course of that experiment. The result of such observations is sometimes in accordance with our anticipation, but very frequently at variance with it. If it accords with our anticipation, we put on record the extension which it has given to the application of the general theory on which that anticipation was founded. But if the result is not what we expected, we carefully and critically revise the reasoning which had led us to expect a particular result, and often repeat the same experiment with greater care, or some modification of it.

Materials for a new theory are gained when logically faultless reasoning, checked by accurate observations, have led to results which could not have been foreseen by the aid of any previous theory. When a theory has thus gained a footing in science, it serves as a guide in further work. It guides us in arranging known facts. It guides us to the discovery of new facts. Sometimes it does these things for a short time only, and is then superseded by some more general theory derived from a wider and more comprehensive view of the facts.

There is, perhaps, nowhere so severe and rigorous a test of the truth of an idea as that which is afforded by its use in any accurate department of experimental science; and it is worth while, on philosophical grounds, to consider briefly the conditions of growth of the chief chemical theories which have withstood this ordeal and proved themselves to be trustworthy guides in experimental science.

Now as far as I know them, the general theories which have played the chief part in the development of chemistry are mere condensed statements of fact.

Every thoughtful man of science has doubtless indulged in speculations to find the cause of facts which are as yet unexplained; has imagined some fundamental condition or property of matter which might cause it to produce effects such as are witnessed. It is to be hoped that the time may be far distant when men of science will confine their thoughts within the range of ideas which are proved to be true. But it is most important that they should not confuse such hypothetical speculations with theories which have received experimental verification, and that while employing any theory they should not lose sight of the limits within which it has been proved to be correct, beyond which it can only be used as an hypothesis.

The foundation of the science of chemistry was laid by the discovery of chemical elements; those distinct varieties of matter which we can neither produce nor destroy. Chemical science treats of those changes of property in matter which can be represented as due to changes of combination of elementary atoms. It knows nothing of the production or destruction of those elementary atoms. Speculations respecting their ultimate form or structure will have found a place in the science as soon as such speculations have helped to arrange the facts which are known, and to discover new chemical facts.

At the commencement of our epoch chemists had classified elements according to their electro-chemical properties. Chemical analysis had established the fact that a good many compounds could be represented as consisting of elementary atoms of two kinds combined in small number. Thus carbonic oxide and carbonic acid had been found to possess respectively a composition which could be represented (adopting our present atomic symbols) by the formulæ CO and CO_2, water by the formula H_2O, marsh gas CH_4, olefiant gas CH_2. The oxides and acids of nitrogen were represented by formulæ corresponding empirically to those which we now adopt. So also ammonia and hydric chloride had their present formulæ. Sulphurous and sulphuric acid had the respective formulæ SO_2 and SO_3. Phosphorus and phosphoric acid had the formulæ P_2O_3 and P_2O_5. Baryta and the oxides of iron had the formulæ BaO, FeO, Fe_2O_3.

Such primary compounds were classified upon the same principle which served for the classification of the elements themselves, into electro-positive or basylous and electro-negative or chlorous compounds, and the smallest quantity of each of them, which consistently with an atomic representation of the results of analysis, was deemed capable of existing, was called an atom of that compound.

Very simple compounds possessed of prominent characteristics and distinct reactions had first been isolated and identified. They were found to contain their constituent elements in proportions easily recognisable as multiples of atomic weights. But such simple compounds are rare exceptions among mineral and organic materials, and if the atomic theory could have gone no further than to guide us to an understanding of these few simple compounds, it must soon have given place to some more fundamental conception. It is moreover worthy of notice that in this its most elementary form the atomic theory was not the only conceivable interpretation of the proportions of combination between elements. Those proportions could be as consistently represented by fractions as by integral multiples. Thus, instead of representing carbonic acid as containing twice as much oxygen as is contained in carbonic oxide, we might have represented it as containing the same quantity of oxygen combined with half as much carbon, and using for the moment atomic symbols for a non-atomic theory, we might have written carbonic acid thus $\frac{C}{2}O$. Or we might represent them both by percentage numbers.

It was so simple and natural to adopt the atomic hypothesis, and to represent compounds as built up of atoms, that chemists seem to have paid little attention to any other mode of representing the proportions of combination. They assumed that the variable proportions of elements, which were observed in compounds, were due to the various numbers of elementary atoms respectively aggregated together in each compound. They perceived that the existence of elementary atoms involved the existence of compound atoms, or molecules, as we now call them, and accordingly they represented each known compound of two elements by a molecular formula as simple as possible, consistently with the view of its atomic constitution. Many of these molecules, such as those of the acids, were found to be capable of combining with others of the other class, forming salts, and those combinations were found to take place in proportions corresponding to the weights of the respective molecules, or to very simple multiples of those weights, and the secondary compounds or salts thus formed combined (if at all) in proportions corresponding to simple multiples of their molecular weights. The dualistic representation of the constitution of salts served to represent the results of their analysis consistently with the atomic theory, and a vast number of fundamental facts were collected and arranged by the aid of the dualistic theory of combination.

The actual numbers obtained by analysis of any particular compound exhibited sometimes a very near approximation to those required by an atomatic formula of its composition. Sometimes they differed considerably from those required by theory; but it was always found that the more pure in substance and the more accurate the analytical operations, the more nearly did the result agree with some atomic formula of the substance.

The compound atoms were units which had grown out of the atomic theory. Each of them was the smallest quantity of a compound, which (consistently with the results of analysis) could be represented as built dualistically of its constituent atoms.

Chemical combination was viewed as a process of juxtaposition, of simple or compound atoms, little account being taken of the disturbance of the previous arrangement of those compound atoms. It was when a constitution, similar to that attributed to salts, was imagined for other compounds not saline in their character, that the dualistic theory broke down. Thus chlorocarbonic acid was represented as a compound of carbonic acid with carbonic chloride, and was accordingly designated as carbonate of carbonic chloride, while the formula was made to contain the formulæ of those bodies. Chlorosulphuric acid and chlorochromic acid were in like manner represented as compounds of sulphuric and chromic acid respectively with imaginary hexachlorides.

Careful investigations of the reactions in which chlorocarbonic acid takes part showed, however, that in each of them it behaves

as a compound containing only two atoms of chlorine. It was found that the commonest and best-known carbonates and sulphates have a fundamentally similar constitution. Thus potassic carbonate may be represented as a compound in which the two atoms of chlorine in phosgene are replaced by two atoms of the radical O K; and oil of vitriol, as a compound of two atoms of hydroxyl with the same group, $S\,O_2$, which in chlorosulphuric acid is combined with two atoms of chlorine. Chlorochromic acid has not been examined to as great an extent as the above compounds, but all we know of it points clearly to its having molecular constitution similar to that of chlorosulphuric acid, viz. $Cl_2\,Cr\,O_2$, for not only do their vapour densities agree, but the chromates in their constitution and crystalline forms exhibit a clear analogy to the sulphates.

Moreover, the simpler molecular formulæ, which a fuller knowledge of their chemical behaviour suggested for these bodies, were found in all cases to agree with the volume belonging to the molecule of every pure substance known in the state of vapour.

A difficulty of another kind had been foreseen by the great founder of the dualistic system, and it was by the investigations in organic chemistry that it assumed serious proportions.

Carbon compounds were discovered possessing definite and specific properties, and presenting the characteristics of pure substances, but of which the results of analysis did not agree with any simple proportion between the numbers of their constituent atoms. Their empirical composition could not be decided by the aid of the so-called law of multiple proportions, for two or more atomic formulæ required percentages of the constituents differing so little from one another that analysis could not decide which was the true one.

In order to select the true molecular formulæ of such complex substances from among those which approached most nearly to the results of ultimate analysis, and to determine with certainty their empirical composition, it was necessary to find other methods for the determination of molecular weights. It was necessary to study the various properties of compounds of known composition, and of others which could be prepared in a state of purity; to determine the vapour densities and rates of diffusion of those which could be obtained in the gaseous state without decomposition; to determine boiling points and melting points; to examine crystalline forms of pure compounds and of mixtures; to determine solubilities and densities of solids and of liquids; but above all it was necessary to collect fuller and more accurate knowledge of the chemical changes which take place in the mutual reaction of molecules.

A vast amount of accurate and careful work of these kinds has been done, and has been subjected to rigid and often hostile scrutiny during the various steps of its progress. We now know that compound atoms, or molecules as we call them, which can be identified by their geometrical, mechanical, and other properties, are the same as the compound atoms indicated by the most comprehensive chemical evidences of composition and reactions. The molecular constitution of matter was predicted implicitly by the atomic theory of the constitution of the elements; and, wherever the physical properties of the molecules are such as afford any basis for the determination of their relative weights, such results agree with those derived from purely chemical considerations guided by the atomic theory.

Our knowledge of molecules is as yet in its infancy. Even among the commonest elements and compounds we know the molecular weights of very few, but what we do know of them proves that the idea of compound atoms invented by chemists to explain the elementary facts of chemical action is, as far as it goes, a true representation of what exists in nature.

Many of the molecules thus proved to exist were the same as those suggested under the dualistic system; but many were proved, by the more accurate and extensive knowledge of their reactions and properties, to have a different weight from that which had been at first attributed to them, yet always consistent with the fundamental requirements of the atomic theory. Thus H_2O, CO, CO_2, CH_4, SO_2, SO_3, CaO, FeO, Fe_2O_3, are the formulæ still used to denote the molecules of the respective compounds, though the last three ought probably to be represented by some multiple. On the other hand, the molecule of olefiant gas is now represented by the formula C_2H_4, instead of CH_2. The chloracetate is $C_2Cl_3HO_2$, instead of C_4Cl_6, C_2O_3, H_2O. The molecule of benzoil chloride is C_7H_5OCl, instead of one corresponding to $(C_7H_5)_2O_3$, $C_7H_5Cl_3$, and chlorosulphuric acid is Cl_2SO_2, instead of $2SO_3$, SCl_6.

In proportion as chemists came to know more of the constitution of molecules, and to study chemical reactions from the point of view of the changes which they bring about in the constitution of molecules, did the idea of substitution come to be more and more used in the place of that of mere additive combination. A vast number of processes of chemical combination, which had been considered as consisting of direct combination, were found to be processes of double decomposition.

One of the most important facts which was brought to light by the careful examination of the composition of salts and organic bodies, aided by the molecular method of representing their constitution, was that hydrogen is chemically one of the metals, and that the compounds formed by the combination of water with acids are analogous to other salts of those acids; while compounds of hydrogen with elements or radicals like chlorine are salts, analogous in their constitution to other chlorides, &c.

The molecular or unitary mode of viewing the constitution of each substance affords more *true* as well as more simple records of the facts observed in chemical reactions than could be obtained in the dualistic systems. A salt such as hydric sulphate used to be considered as containing sulphuric acid and water, and represented by a formula such as SO_3, H_2O, implying the presence in it of both the substances from which it was known to be formed.

When two elements combined, their product was considered and described as containing the elementary atoms which had served to form it, and it was consistent with this habit to represent a product which had been formed by the combination of two compound molecules as containing those molecules.

But the main business of chemical investigation is to observe accurately the changes of composition which take place in the reactions of known substances, with a view of discovering the atomic changes to which they are due.

The compound formed by the combination of sulphuric acid and water differs in many physical and chemical properties from both of those bodies. Its name and its atomic formula serve to denote the aggregate of properties which are known to belong to it, whereas the dualistic formula, SO_3, H_2O, served to recall the properties of the acid and base from which it was formed, rather than those of the compound itself.

Elementary chemical reactions which according to the binary mode of viewing compounds were supposed to consist of dualistic processes involving sometimes the assumption of forces (like predisposing affinity) of a purely metaphysical character, were now explained as consisting of atomic displacements, or interchanges of a kind well known to be of common occurrence. Thus the evolution of hydrogen by the action of zinc or aqueous hydric sulphate was supposed to be the result of a decomposition of water by the metal, such decomposition being induced by the presence of the acid (SO_3), which exerted a predisposing affinity for the zinc oxide. Our present explanation is a simple statement of the fact, that under the conditions described, zinc displaces hydrogen from its sulphate.

The recognition and study of the metallic functions of hydrogen enabled chemists to obtain far clearer and simpler views of the constitution of salts, and to observe the differences of property which are produced in them by the replacement of one element by another. It enabled us to see more and more clearly the characteristic functions of each element, by comparing the constitution and properties of salts containing it with those of the corresponding salts containing other elements.

Thus in the dualistic system we had for the three common phosphates, PO_4Na_3, PO_4Na_2H, PO_4NaH_2, molecular formulæ in which sodium was represented with twice as great an atomic weight as that which we attribute to it, and which in our atomic weights may be thus represented, viz. P_2O_5, $3Na_2O$; P_2O_5, $2Na$, O; P_2O_5, Na_2O. In like manner we had such a formula as P_2O_3, $2Na_2O$ (for the phosphite PO_3Na_2H), and for the hypophosphite PO_2NaH_2 we had a formula corresponding to P_2O, Na_2O.

Determinations of water of crystallisation and of chemically combined water proved that many of the compounds assumed on the dualistic system to exist are either not obtainable or have different properties and a different constitution from those which have been described. Thus we now know that the salts PO_4Na_2H, PO_4NaH_2, PO_3Na_2H, and PO_2NaH_2 cannot be deprived of the elements of water without undergoing a fundamental change of composition and of properties.

The atomic weights of the alkali metals and of silver were

found to be half of those of the dualistic system, and an atom of one of these metals, in common double decompositions between their salts and hydrogen-salts, changes place with one atom of hydrogen.

Many products of the combination of known molecules were found to be formed by processes of double decomposition, so that each molecule of such products is built up partly of atoms derived from one of the materials, partly of atoms from the other. Thus potassic hydrate is formed by the combination of a molecule of potash with one of water. Yet each molecule of the hydrate is built up of half a molecule of potash and half a molecule of water.

The study of organic compounds played an important part in the improvement of our processes of reasoning. Many of their molecules having a very complex structure were found to undergo in most of their reactions very simple changes, of the same kind as those which mineral compounds undergo. Most of the elements of each organic molecule remained combined together with functions analogous to those of hydrogen or chlorine.

The theory of radicals which had been suggested by the reactions of ammonia-salts and of cyanides was largely extended in organic chemistry.

Many families of organic compounds were discovered, in each of which the members are connected by close analogy of constitution and of properties. Each of these families forms what is called a homologous series, each term of the series being a compound of which the molecule contains one atom of carbon and two atoms of hydrogen more than the previous term.

Thus a series of compounds was proved to have reactions similar to those of common alcohol, and molecular weights ranging from 32 to 438. The lower terms of the series are distinguished from one another by differences of boiling points approximately proportional to the number of atoms of carbon and hydrogen by which they differ from one another; whilst the higher terms undergo decomposition at the higher temperatures required for their evaporation, and are distinguished from one another by differences of melting points, that of the alcohol $C_{30}H_{62}O$ being about 85° C. In their constitution these alcohols were found to be analogous to the alkaline hydrates.

In like manner various other series of alcohols were discovered corresponding respectively in their constitutions to other classes of metallic hydrates. Series were also found of which the members present analogies of reaction with monobasic, bibasic, tribasic hydrogen salts respectively.

These and many other such discoveries were made under the guidance of the atomic theory, developed to the point of systematically recognising and studying the mutual reaction of molecules.

One of the most remarkable and important extensions which our knowledge of molecules has undergone consisted in the discovery that various elements in what we are accustomed to consider the free state, really consist of molecules containing like atoms combined with one another.

Thus chemists adopt the formulæ O_2, H_2, Cl_2, P_4, J_2, As_4, to denote molecules of the respective elements, and we have for these molecular formulæ evidences of the same kinds as those which serve to establish the molecular formulæ ClH, H_2O, NH_3, &c. In all the best-known reactions in which chlorine or hydrogen are either taken up or evolved we find that those elements behave as chemical compounds of two like atoms; and, moreover, their molecules, as determined from a study of their reactions, have the same volume as that of every compound molecule proved to evaporate without decomposition.

With this knowledge of the molecular constitution of hydrogen and of chlorine gases, we come to regard the direct formation of hydric chloride as due to a process of double decomposition between two molecules, like the reaction of chlorine on an equal volume of marsh gas.

Many other reactions, such as the evolution of hydrogen by the action of zinc on a hydrogen salt, the liberation of chlorine and nitrogen on the explosive decomposition of their compound, the direct combination of oxygen and hydrogen, we may expect to be able to resolve into mere processes of double decomposition.

The earliest determinations of combining proportions were made with salts (hydrogen salts and others) which undergo double decomposition by mutual contact, and the term equivalent was subsequently introduced to indicate the proportional weights of analogous substances found to be of equal value in their chemical effects. Tables of equivalent weights of acids consisted of numbers standing to one another in the same proportions as the weights of the respective substances found to be of equal value in neutralising a fixed quantity of a particular base; and in like manner tables of the equivalent weights of bases recorded the proportions by weight in which certain bases might replace one another in the neutralisation of a particular quantity of a given acid. Similar determinations have been tabulated of the so-called equivalent weights of elements. Under the dualistic system chemists paid little attention to the essential difference between atomic weights and equivalent weights; and some were of opinion that the facts of chemistry might be represented as consistently from the point of view of equivalence as from that of atoms, and that the idea of atoms (which they considered to be hypothetical) might be dispensed with.

In the system of atomic weights employed under that system, two atoms of hydrogen were generally represented as reacting together, and the symbol of the double atom was marked thus, $\overline{\mathrm{H}}$. The alkali metals and silver were represented as having atomic weights twice as great as those which we now adopt, and equivalent to those of the magnesian metals and of oxygen. In a great number of the common reactions of these elements the atomic symbols were consistently used as equivalent symbols. But those who professed to dispense with the atomic theory used atomic symbols, even in cases where they did not represent equivalent weights. Thus nitrogen was always represented by its atomic symbol, and the composition and reactions of nitrogen compounds were always studied and represented in accordance with the atomic theory, using various multiple proportions of what they were still pleased to call equivalent weights, using molecular weights, and various other ideas which formed part of the atomic theory, and which had no known connection with the notion of fixed equivalence. If, however, it be true that all chemical compounds consist of elementary atoms, and that the explanation of chemical reactions consists in stating more and more precisely the changes of combination between the constituent atoms of the reacting molecules; equivalence could only be said to exist between a like number of atoms when they were known to have similar functions. It became necessary to study the relation of equivalence between elementary atoms, instead of studying them from the point of view of elements divisible in any proportion.

It is worth while noticing the general process by which this intellectual change was brought about; for there is a good deal yet to be done in the matter, and our future progress may be guided by experience gained in the past.

It was essentially one-sided. One consideration was brought into very prominent relief, and it threw a marvellous light on the matter. It gave us a clear view of the natural order among elements; but, like every other strong light, it fell on one side only.

The equality of vapour-volumes had been used with great advantage in conjunction with chemical reactions and other evidence as a characteristic of molecules, and the attention of chemists was greatly arrested by the consideration of four typical compounds, which upon the concurrent evidence of very extensive chemical examination and equality of vapour-volumes were known to have respectively a composition corresponding to the formulæ ClH, OH_2, NH_3, CH_4.

It was known that the atom of oxygen in water can be replaced by chlorine, but that two atoms of chlorine are needed for the purpose. The atom of nitrogen in ammonia requires three atoms of chlorine to replace it, whilst in marsh gas the atom of carbon is replaceable by four atoms of chlorine. Other elements were studied from the point of view of their respective resemblance to these, and arranged in classes, each of which consisted of atoms equivalent to one another. Thus chlorine, bromine, iodine, fluorine, hydrogen, potassium, sodium, lithium, silver, &c., constituted a class of atoms of equal value, and were called monads. Oxygen, sulphur, selenium, tellurium, calcium, strontium, barium, magnesium, zinc, cadmium, mercury, lead, copper, &c., were classed together as dyads, having equal value amongst themselves, but double the atomic value of the members of the first class. So nitrogen, phosphorus, arsenic, antimony, bismuth, with boron, and some other elements, were considered as forming a class of atoms each of which has three times the value of the monads. The class of tetrads contained carbon, silicon, tin, platinum, &c.

Many apparent exceptions to these atomic values were satisfactorily explained as due to the partial combination of like

atoms with one another. Thus in the vast majority of hydrocarbons, such as C_2H_6, C_2H_4, C_2H_2, &c., the atoms of carbon do not appear to be tetravalent, inasmuch as each of the molecules contains less than four atoms of hydrogen to every one atom of carbon. It was well known, however, that polyvalent atoms can combine partly with one element, partly with another, and also that like atoms can combine with one another. Why then should not two tetravalent atoms like carbon combine respectively with three atoms of a monad, and also combine with one another? The compound must be a single molecule with the properties known to belong to methyle C_2H_6. Again, if this molecule were deprived of two of its atoms of hydrogen, each of the atoms of carbon must combine further with the other atom of carbon forming H_2CCH_2; and a further step in this same direction would give us acetylene HCCH, in which each atom of carbon is combined with the other to the extent of three quarters of its value, and with one atom of hydrogen. An extension of this reasoning led to the discovery of long chains of atoms of carbon, each atom forming a link, and each of them (short of the ends) being combined with two other atoms of carbon, while its saturation is completed by hydrogen.

Similar partial combinations of like atoms with one another were recognised in many other classes of compounds, and there is strong reason to expect that the application of the principle will be far more widely extended in proportion as our knowledge of the silicates and other complex classes of compounds becomes somewhat definite.

This incorporation of the doctrine of equivalence into the atomic theory by the division of the elements into classes consisting respectively of equivalent atoms, was probably one of the most important general steps as yet made in the development of the atomic theory. It was seen to correspond in so clear and striking a manner with a vast number of well-known properties and reactions of compounds as to deserve and acquire the confident trust of chemists. But, as often happens in such cases, this confidence in the result carried many of them too far. It led them to assume that atomic values in all other chemical compounds must be always the same as in the compounds under consideration. They saw that they had got hold of the truth, and they thought it was the whole truth. For instance, one most distinguished chemist assumed that each elementary atom has only one value in its compounds; that the atom of nitrogen has always the value three, as in ammonia and its products of substitution, and that in sal ammoniac the atom of nitrogen is chemically combined only with three atoms of hydrogen, whilst the molecule of ammonia is in a state of molecular combination with hydric chloride. Another most distinguished chemist admitted that nitrogen and phosphorus have two atomic values, but not more than two. He held that the respective combining powers are always satisfied by the same number of atoms, no matter what the character of the uniting atoms may be.

With respect to these views it may be noticed that the assumption of combination between molecules as due to some other force than that which binds together the constituents of each molecule—in fact the assumption of molecular combination as an unknown something different from chemical combination, is open to even more grave objections than those which led us to abandon the dualistic system.

To represent a molecule of sal ammoniac as a compound containing two molecules, each one built up by the chemical combination of the constituent atoms, and the two united together by some other force called molecular, was hardly a step in advance of the view which represented it as containing two molecules united together by the same kind of force as that which holds together the atoms in each of the constituent molecules.

The other form of the theory of atomicity as an inherent property of each atom enabling it to combine with an equal number of other atoms, whatever the character of those other atoms may be, seems difficult to reconcile with such facts as the following:—An atom of nitrogen is not known to combine with more than three atoms of hydrogen alone, or of substances like hydrogen, but it forms stable compounds with five atoms (as in the ammonia salts), when four of them are basylous and one of them is chlorous. An atom of sulphur is not known to combine with more than two atoms of hydrogen alone, but it forms stable compounds with four atoms, if three of them are like hydrogen, while the fourth is chlorous. Instances like these are plentiful, and they lead us to look to the chemical characters of the atoms bound together in one molecule as a fundamental condition of the atomic value of the element which binds them together.

Theoretical limitations of natural forces are very difficult of proof, and it is well to be slow and cautious in adopting any such limitation.

A careful consideration of the facts of the case has led me not only to doubt the validity of the supposed limits of atomic value, but to doubt whether we have grounds for assigning any limits whatever to such values.

Atomic values appear to me to be in their very nature variable quantities, and I venture to think that chemistry will be greatly advanced by a full and careful study of the conditions of variation of atomic values.

Two conditions of change of atomic value are particularly worthy of notice:—

I. Temperature.

II. The chemical character of the uniting atoms.

Atomic values increase with fall of temperature, and diminish with rise of temperature. An atom which is combined with as many basylous monads as it can take up by themselves, will take up chlorous monads, or both chlorous and basylous, and reciprocally.

In illustration of the diminution of atomic values with rise of temperature, I may adduce the following well-known reactions: Sal ammoniac containing nitrogen combined with five monads breaks up at a high temperature into ammonia and hydric chloride; and in like manner other ammonia salts decompose by heat forming ammonia or an amide, with trivalent nitrogen. The highest chlorides of phosphorus and of antimony are decomposed by heat into free chlorine and the lower chloride. Potassic fluosilicate is decomposed by heat into silicic and potassic fluorides; and carbonic acid breaks up at high temperatures into a mixture of carbonic oxide and oxygen.

Amongst illustrations of the greater atomic values which elements assume by combining with both chlorous and basylous atoms than with atoms of the one kind only, we may take the following cases: platinum is a metal of which the atom has been supposed to be always tetravalent, because it has not been found capable of combining with more than four atoms of chlorine. The common solution formed by aqua regia contains the compound H_2PtCl_6, a perfectly definite and crystallisable hydrogen salt. Chemists are constantly making and using the potassium and ammonium salts, &c., corresponding to it, yet they conceal from themselves the fact that the atom of platinum is directly combined with eight monads by calling the compounds double salts. The atom of silicon in the silico-fluorides such as H_2SiF_6, or K_2SiF_6, is combined with twice as many monads as it can take up of one kind; so boron in the crystalline salt $NaBF_4$ has a higher atomic value than in its fluoride, owing to the presence of the atom of sodium.

In like manner the atom of gold in the well-known salt $NaAuCl_4$, has a higher value than it can assume with chlorine alone.

Sulphur, of which the atom does not combine with more than 2 atoms of hydrogen, forms with 3 atoms of methyle, or ethyle, and 1 atom of iodine, or chlorine, &c., the well-known compounds like $JSMe_3$; and iodine, which is considered a monad, forms the crystalline and stable periodate $OJ(OH)_5$ and the various metallic derivatives, such as

$$OJ\,\begin{matrix}ONa\\(OH)_4\end{matrix} \qquad OJ\,\begin{matrix}(ONa)_2\\(OH)_3\end{matrix} \qquad OJ\,(OAg)_5$$

The crystalline compound of the perchlorate with water ($HClO_4 2H_2O$) has probably a similar constitution. Chemical journals abound with descriptions of definite and well-characterised compounds, which have, like the above, been put aside by the atomicity theory, as mere molecular compounds. The following formulæ are taken almost at random, in illustration of the generality of atomic values far beyond those acknowledged by the theory of atomicity.

$JPCl_8$	K_2FeF_4	K_2SnCl_6
$KAsF_6$	$2(K_2FeCl_5)$	$KHgJ_3$
K_2AsF_7	$2(K_3FeF_6)$	$K_2HgJ_4 4H_2O$
$KSbF_6$	K_2CuCl_4	$KHgCl_3$
K_2SbCl_5	K_2CuF_4	K_2HgCl_4
K_2SbF_7	$KMgCl_3$	
K_3BiCl_6	K_2MgBr_4	
K_2AgJ_3		

I have for convenience written in the middle of each of these

formulæ the symbol of the atom which I assume to act as connecting element. If we consider the atomic values usually found in these elements, together with those represented by the above list, we see that their atomic values vary according to the numbers given in a line with them respectively in the following table. It has yet to be proved that the atom of platinum is tetravalent in any known compound, for there is no sufficient evidence to show that platinic chloride has a molecular weight corresponding to the formula $PtCl_4$, instead of one corresponding to Pt_2Cl_8, each atom of platinum being partly combined with the other, partly with chlorine.

Atomic Symbols.	Atomic Values.
C	2, 4
S	2, 4
Pt	4 (?), 8
Si	4, 8
Sn	4, 8
Cu	2, 6
Hg	2, 4, 6
Mg	2, 4, 6
Ag	1, 5
B	3, 5
I	1, 7
N	3, 5
P	3, 5, 7
As	3, 7, 9
Sb	3, 5, 7, 9
Bi	3, 7
Au	3 (?), 5

Not only are there elements of which an atom is found in combination with a greater number of basylous and chlorous monads together than of either kind alone, but there are also elements which are not known to form chemical compounds with hydrogen or potassium alone, and yet which combine with either of them when also combined with chlorine, fluorine, &c. This is illustrated by the following compounds, viz., $HAuCl_4$, H_2PtCl_6, $NaBF_4$, K_2SiF_6, K_2FeF_4, K_2CuCl_4. It is also well known that there are many cases of elements of which an atom cannot combine with as many monads of one kind as of another. For instance an atom of nitrogen or of antimony is only known to be trivalent in combination with hydrogen; but each of them occurs in the form of a pentavalent compound with chlorine. Antimony forms either no compound with five atoms of bromine, or a compound more unstable than the higher chloride.

Many more such instances might easily now be given, and a vast number will doubtless be found when the investigations of chemists are directed to the search for them. I have only given these few by way of illustration of the leading conditions of change of atomic values.

In the course of their investigations of the precise interchanges of atoms which take place between molecules, chemists were frequently led to observe evidences of the order in which the constituent elements are combined; and with the more wide and accurate knowledge of reactions which is now in their possession, they have been enabled to follow up so far the study of the respective state of combination of each atom in a molecule as to arrive at simple and consistent explanations of facts which had previously eluded the grasp of science.

Our knowledge of the order of combination of atoms in a molecule and of the differences between direct and indirect combinations of particular atoms may be said to have originated chiefly in the study of the compounds of nitrogen. Thus it was found that the hydrogen in ammonia differs in many of its chemical functions from hydrogen in hydrocarbons. A base (called methylia) was discovered having a molecular composition corresponding to the empirical formula (CNH_5), and this base was found to contain two atoms of hydrogen like those of ammonia, and three atoms like those in hydrocarbons. Its constitution was accordingly represented by a formula describing it as an ammonia, in which one atom of hydrogen is replaced by the monad methyle, or, to be more explicit, as containing two atoms of hydrogen directly combined with nitrogen, and three atoms of hydrogen indirectly combined with that same atom of nitrogen through the intervening atom of carbon. Writing in juxtaposition to one another the symbols of those atoms which are directly combined, we can express the facts by the following formula, viz. H_2NCH_3.

Those marvellous varieties of matter called isomeric compounds found their natural explanation in differences of the respective arrangements of like atoms. Thus two bases were discovered having the same empirical molecular formula C_2NH_7. One of them is made by different reactions from the other, and in its decompositions differs from the other. All these chemical differences between them are found to be due to the fact that one of them (called ethylia) contains two atoms of hydrogen directly combined with the nitrogen, and the monovalent hydrocarbon ethyle in place of the third atom of hydrogen; whilst the other (called dimethylia) contains only one atom of hydrogen combined directly with nitrogen, the carbon of the two atoms of methyle completing the saturation of the trivalent nitrogen, as expressed by the formula $HN(CH_3)_2$.

It was subsequently proved that an atom of oxygen may combine with two like or unlike monads, such monads being indirectly combined with one another through the intervening atom of oxygen. Thus five of the atoms of hydrogen in common alcohol were proved to be in direct combination with the carbon, whilst the other one is indirectly combined with it through the oxygen, as expressed by the formula $HO(C_2H_5)$.

Another compound (called methyl-oxide) was proved to have the same empirical composition, but very different properties and reactions, its constitution being explained by the formula H_3COCH_3.

Again, two compounds of distinct reactions and properties were found to have the same empirical molecular composition, C_2NH_3, and it was clearly proved that in one of them the two atoms of carbon are directly combined thus, $NCCH_3$, whilst in the other they are indirectly combined through the atom of nitrogen $CNCH_3$.

An immense amount of admirable work has been done of late years (especially in Germany) in working out the evidences of the atomic order of complex organic bodies, and in thereby obtaining a command of their reactions.

Evidences of the same kind have been obtained of the atomic arrangement of some few of the simplest inorganic bodies, and it is to be hoped that ere long chemists will recognise the importance of examining the constitution of salts with the aid of the principles established in organic chemistry.

The foundation is already laid by our knowledge of the constitution of such compounds as

$$HOH,\ HOK,\ HONH_4,\ HONO_2$$
$$HOPbOH,\ HOPbONO_2,\ \underset{O}{HOCOK},\ \underset{O_2}{HOSOH}$$

and there is a strong probability regarding the atomic constitution of many other water compounds, *e.g.*

$$SO_4H_2 + H_2O = OS(OH_2)_4, SO_2H_4 + H_2O = S(OH)_6.$$

Amongst the extensions of our means of examining the physical properties of matter, and thereby discovering new varieties of matter for chemical investigation, spectrum analysis has played an important part, and is no doubt destined to do far more. It has already led chemists to the discovery of several previously unknown elements, and has led to the detection of various known elements in distant masses of which we had previously no chemical knowledge.

Up to this point the growth of the atomic theory will be seen, from the general outline which I have endeavoured to trace, to have consisted mainly in the more and more full and exact identification of each elementary atom, and in the accumulation of more and more varied and accurate evidences of its functions in relation to other atoms. A step was made towards a knowledge of the general relations of atoms to one another by their preliminary classification according to their best-known values.

But a far greater step has been more recently made, one which is evidently destined to lead to most important results.

It was discovered that if we arrange the elements in the empirical order of their respective atomic weights, beginning with hydrogen and proceeding thence step by step to the heaviest atom, we have before us a natural series with periodically recurrent changes in the chemical and physical functions of its members.

Of course the series is imperfect, and exhibits gaps and irregularities; but what view of natural order was complete in its infancy?

Some of the gaps have already been filled up by the discovery of elements possessing the anticipated properties. The generalisation affords a brilliant addition to the previous corroborations of the reality of the units of matter which chemists have discovered.

Chemists have as yet taken but little account of atomic motion;

although the most perfect explanation of a chemical reaction consists of a statement of the atomic interchange which takes place between two molecules; or the change of mutual combination between the atoms in one molecule.

It has, however, been proved that the heat of combination affords a measure of its force; and we know that in giving off heat particles of matter undergo a diminution of velocity of motion. We see, accordingly, that substances capable of exerting great force by their combination are those which can undergo a great diminution of the velocity of their internal motions, and reciprocally.

The force of chemical combination is evidently a function of atomic motion.

It has been shown that the relative velocities of certain atomic interchanges afford a measure of the amount of chemical action between two substances; but a vast amount of work will doubtless be required to develop the atomic theory to the point of explaining the force of chemical action in precise terms of atomic motion.

The general terms of chemistry are mere symbols. Each of them serves to recall a group (usually a very large group) of facts established by observation. The explanation of each term is afforded by a careful study of the facts which it is used to denote; and, accordingly, a chain of evidence involving the use of chemical terms can be fully understood only by chemists accustomed to the consideration of such evidence. The general outline of it may perhaps be to some general thinkers of sufficient interest to attract them to further study of our science.

SECTION C

GEOLOGY

Opening Address by A. C. Ramsay, LL.D., F.R.S., &c., &c., Director-General of the Geological Survey, President of the Section

On the Origin and Progress of the Present State of British Geology, Especially since the first Meeting of the British Association at York in 1831

In the year 1788 Hutton published his first sketch of his "Theory of the Earth," afterwards extended and explained by Playfair in a manner more popular and perspicuous than is done in Hutton's own writings. In this grand work, Hutton clearly explains that the oldest known strata, like their successors, are derivative, and that as far as *observation* can discover, in all geological time, "we find no vestige of a beginning, and no sign of an end." The complement to this far-seeing observation was at length brought about by William Smith, in his original "Geological Map of the Strata of England and Wales" in 1815, followed, in 1816, by his "Strata Identified by Organised Fossils." This great discovery, for such it was, threw a new light on the history of the earth, proving what had before been unknown, that all the "Secondary" formations, at least from the Lias to the Chalk inclusive, contained each a set of distinctive fossils by which it could be recognised. A law was thus provided for the identification of formations which geographically are often widely separated from each other, not only in England in the case of minor outliers, but also easily applicable to great areas on the neighbouring continent of Europe.

In 1811 the first volume of the *Transactions* of the Geological Society was published, and in 1826 27 there appeared the first volume of the *Proceedings*, the object being to communicate to the Fellows as promptly as possible the *Proceedings* of the Society "during the intervals between the appearance of the several parts of the *Transactions*." The last volume of the *Transactions* contains memoirs read between the years 1845-1856, and only four volumes of the *Proceedings* appeared between the years 1826 and 1845 inclusive, after which the title of the annual volume was changed to that of the "Quarterly Journal of the Geological Society." The Geological Society, to which the science owes so much, was therefore in full action when the British Association was founded in 1831, and the memoirs read before the Society from 1831 to this date may be said to show generally the state of British geology during the last fifty years. To this must be added the powerful influence of the first (1830) and later editions of Lyell's "Principles of Geology," a work which helped to lay the foundations of those researches in Physical Geology which both in earlier and later years have attracted so much attention.

Fifty years ago, in this city, Viscount Milton was president of the first meeting of "The British Association for the Advancement of Science," which he explained had for its chief object "to give a stronger impulse and more systematic direction to scientific inquiry." In his address he pointed out the numbers of Philosophical Societies which had by degrees sprung up in all parts of the kingdom; and the practicability, through the means of the Association, "including all the scientific strength of Great Britain," "to point out the lines in which the direction of science should move."

In that year, 1831, Prof. Sedgwick was president of the Geological Society, and the Geological and Geographical Committee of the British Association recommended that geologists should examine the truth of that part of the theory of Elie de Beaumont, in its application to England, Scotland, and Ireland, which asserts that *the lines of disturbance of the strata assignable to the same age are parallel*; that Prof. Phillips be requested to draw up *a systematic catalogue of all the organised fossils of Great Britain and Ireland*; and that Mr. Robert Stephenson, civil engineer, be requested to prepare a report upon *the waste and extension of the land on the east coast of Britain, and the question of the permanence of the relative level of the sea and land.*

In 1881 it seems strange to us that, in 1831, with William Smith's map of "The Strata of England and Wales, with part Scotland," before them, it should have been considered necessary to institute an inquiry as to the truth of the general parallelism of disturbed strata, which, in a limited area like England, had suffered upheaval at different successive epochs; and we may fancy the internal smile with which Phillips, the nephew of Smith, regarded the needless proposal. The masterpiece of the old land-surveyor and civil engineer remains to this day the foundation of all subsequent geological maps of England and Wales; and *as an unaided effort of practical genius*—for such it was—it seems impossible that it should be surpassed, in spite of all the accuracy and detail which happily modern science has introduced into modern geological maps.

The first paper read at York, in the year 1831, was by Prof. Sedgwick, "On the General Structure of the Lake Mountains of the North of England." This was followed by "Supplementary Observations on the Structure of the Austrian and Bavarian Alps," by the Secretary of the Society, Mr. Murchison, a memoir at that time of the highest value, and still valuable both in a stratigraphical point of view and also for the light which it threw on the nature of the disturbances that originated the Alpine mountains, and their relations in point of date to the far more ancient mountains of Bohemia. In his elaborate address in the same year, on his retiring from the president's chair, he largely expatiates on the parallelism of many of the great lines of disturbance of what were then distinguished as the more ancient *schistose* and *greywacké* mountains, and quotes the authority of Elie de Beaumont for the statement, "that mountain chains elevated at the same period of time have a general parallelism in the bearing of their component strata." On a great scale this undoubtedly holds true, as, for example, in the case of the Scandinavian chain, and the more ancient Palæozoic rocks north of Scotland, Cumberland, and even of great part of Wales. The same holds good with regard to the parallelism of the much more recent mountain ranges of the Apennines, the Alps, the Caucasus, the Atlas, and the Himalayas, all of which strike more or less east and west, and are to a great extent of post-Eocene, and even partly of post-Miocene age. The same, however, is not precisely the case with the Apalachian chain and the Rocky Mountains of North America, the first of which trends N.N.W., and the latter N.N.E. The remarkable chain of the Ural Mountains trends nearly true north and south, and is parallel to no other chain that I know of, unless it be the Andes and the mountains of Japan. It is worthy of notice that the chain of the Ural is of pre-Permian age according to Murchison, while Darwin has shown that the chief upheaval of the Andes took place in post-Cretaceous times.

The Apalachian chain is chiefly of post-Carboniferous date, and the Rocky Mountains have been re-disturbed and re-elevated as late as post-Miocene times.

In the same address Prof. Sedgwick entered an eloquent protest against the broad uniformitarian views so powerfully advocated in the first edition of Lyell's "Principles of Geology" in 1830, in which, throwing aside all discussion concerning cosmogony, he took the world as he found it, and, agreeing with Hutton that geology is in no way concerned with, and not sufficiently

advanced to deal "with questions as to the origin of things," he saw that a great body of new data were required, such as engaged the attention of the Geological Society (founded in 1807), and which, along with other foreign societies and private work, has at length brought geological science to its present high position.

And what is that position? With great and consentient labour, many men, gifted with a knowledge of stratigraphical and palæontological geology, have, so to speak, more or less dissected all the regions of Europe and great part of North America, India, and of our colonies, and in vast areas, sometimes nearly adjoining, and sometimes far distant from each other, the various formations, by help of the fossils they contain, have been correlated in time, often in spite of great differences in their lithological characters. It is easy, for example, to correlate the various formations in countries so near as Great Britain and Ireland, or of the Secondary and Lower Tertiary formations of England and France; and what is more remarkable, it is easy to correlate the Palæozoic formations of Britain and the eastern half of the United States and Canada, even in many of the comparatively minute stratigraphical and lithological subdivisions of the Silurian, Devonian, and Carboniferous formations. The same may be said with regard to some of the Palæozoic formations of India, China, Africa, and Australia, and many of the Secondary and Tertiary deposits have in like manner been identified as having their equivalents in Europe. It is not to be inferred from these coincidences that such deposits were all formed *precisely* at the same time, but taken in connection with their palæontological contents, viewed in the light which Darwin has shown with regard to the life of the globe when considered in their relation to masses of stratified formations, no modern geologist who gives his mind to such subjects would be likely to state, for example, that in any part of the globe Silurian rocks may be equivalents in time to any of our Upper Palæozoic, Mesozoic, or Tertiary formations.

For all the latest details of *genera* and *species* found in the British Palæozoic rocks, from those of St. David's, so well worked out by Dr. Hicks, to the Carboniferous series inclusive, I must refer to the elaborate address of Mr. Etheridge, President of the Geological Society, which he delivered at the last anniversary meeting of that society. It is a work of enormous labour and skill, which could not have been produced by any one who had not a thorough personal knowledge of all the formations of Britain and of their fossil contents.[1]

In connection with such subjects I will not in any way deal with the tempting and important subject of cosmological geology, which in my opinion must go back to times far anterior to the date of the deposition, as common sediments, of the very oldest-known metamorphic strata. Cosmological speculations perhaps may be sound enough with regard to refrigeration, and the first consolidation of the crust of the earth, but all the known tangible rocky formations in the world have no immediate relation to them, and in my opinion the oldest Laurentian rocks were deposited long after the beginning and end of lost and unknown epochs, during which stratified rocks were formed by watery agents in the same way that the Laurentian rocks were deposited, and in which modern formations are being deposited now, and the gneissose structure of the most ancient formations was the result of an action which has at intervals characterised all geological time as late as the Eocene formations in the Alps and elsewhere.

The same kind of chronological reasoning is often applicable to igneous rocks. It was generally the custom, many years ago, to recognise two kinds of igneous rocks, viz., Volcanic and Plutonic, and this classification somewhat modified in details is still applicable, the Plutonic consisting chiefly of granitic rocks and their allies, and which though they have often altered and thrust veins into the adjoining strata, have never, as far as I know, overflowed in the manner of the lavas of modern and ancient volcanoes. Indeed, as far as I recollect, the first quoted examples of ancient volcanoes are those of Miocene age in the districts of Auvergne, the Velais, and the Eifel, and the fact that signs of ordinary volcanic phenomena are found in almost all the larger groups of strata was scarcely suspected. Now, however, we know them to be associated with strata of all or almost all geological ages, from Lower Silurian times down to the present day, if we take the whole world into account. Amongst them, those of Miocene date hold a very prominent place, greatly owing, doubtless, to the comparative perfection of their forms, as, for example, those of the South of France and of the Eifel. Their conical shapes, and numerous extinct craters, afford testimony so plain, that he who runs may read their history. The time when they became extinct would doubtless amaze us by its magnitude, if it could be stated in years, but yet it is comparatively so recent that not all the undying forces of atmospheric degradation have been able to obliterate their individual origin.

It is, however, generally very different with respect to volcanoes of Mesozoic age, for though Lyell stated with doubt, that volcanic products of Jurassic date are found in the Morea and in the Apennines; and Medlicott and Blanford consider that probably the igneous rocks of Rajmahal may be of that age, we must, perhaps, wait for further information before the question may be considered as finally settled. Of Jurassic age no actual craters remain. Darwin also has stated, on good grounds, that in the Andes a line of volcanic eruptions has been at work from before the deposition of the Cretaceo-oolitic formation down to the present day.

In the British Islands we have a remarkable series of true volcanic rocks, the chronology of which has been definitely determined. The oldest of these belongs to the Lower Silurian epoch, as shown, for example, on a large scale in Pembrokeshire, at Builth in Radnorshire, in the Longmynd country west of the Stiper stones in Shropshire, and on a far greater scale in North Wales and Cumbria. Of later date we find volcanic lavas and ashes in the Devonian rocks of Devon, and in the Old Red Sandstone of Scotland. The third series is plentiful among the Carboniferous rocks of Scotland, and in a smaller way interstratified with the Coal-measures of South Staffordshire, Warwickshire and the Clee Hills. The fourth series chronologically is associated with the Permian strata in Scotland, and the fifth and last consists of the Miocene basaltic rocks of the Inner Hebrides and the mainland of the West of Scotland.

In the British Islands the art of geological surveying has, I believe, been carried out in a more detailed manner than in any other country in Europe, a matter which has been rendered comparatively easy by the excellence of the Ordnance Survey maps both on the 1-inch and the 6-inch scales. When the whole country has been mapped geologically little will remain to be done in geological surveying, excepting corrections here and there, especially in the earliest published maps of the South-west of England. Palæontological detail may, however, be carried on to any extent, and much remains to be done in microscopic petrology which now deservedly occupies the attention of many skilled observers.

Time will not permit me to do more than advert to the excellent and well-known geological surveys now in action in India, Canada, the United States, Australia, New Zealand, and South Africa.

On the Continent of Europe there are National Geological Surveys of great and well-deserved repute conducted by men of the highest eminence in geological science, and it is to be hoped the day may come when a more detailed survey will follow the admirable map executed by Sir Roderick Murchison, De Verneuil, and Count Keyserling, and published in their joint work, "The Geology of Russia in Europe and the Ural Mountains."

It is difficult to deal with the Future of Geology. Probably in many of the European formations more may be done in tracing the details of subformations. The same may be said of much of North America, and for a long series of years a great deal must remain almost untouched in Asia, Africa, South America, and in the islands of the Pacific Ocean. If, in the far future, the day should come when such work shall be undertaken, the process of doing so must necessarily be slow, partly for want of proper maps, and possibly in some regions partly for the want of trained geologists. Palæontologists must always have ample work in the discovery and description of new fossils, marine, freshwater, and truly terrestrial; and besides common stratigraphical geology, geologists have still an ample field before them in working out many of those physical problems which form the true basis of Physical Geography in every region of the earth. Of the history of the earth there is a long past, the early chapters of which seem to be lost for ever, and we know little of the future except that it appears that "the stir of this dim spot which men call earth," as far as Geology is concerned, shows "no sign of an end."

[1] I must also, with much pleasure, advert to Prof. Prestwich's inaugural lecture when installed in the Chair of Geology at Oxford in 1875, the subject of which is "The Past and Future of Geology."

SECTION D

BIOLOGY

Opening Address by Richard Owen, C.B., F.R.S., President of the Section

The recent construction of the edifice of the British Museum (Natural History), Cromwell Road,[1] and the transference thereto of three of the Departments, the systematic arrangement of which in their respective galleries approaches closely to completion, have left me little leisure in the present year for other scientific work. The expression, moreover, in divers forms and degrees of the satisfaction and instruction such partial exhibition of the national treasures of natural history has afforded to all classes of visitors since the galleries were open to the public, in April last, encourages me to believe that a few words on this great additional instrument in advancing biological science may not be unacceptable to the Section of the British Association which I have now the honour to address.

It is true that when we last met at Swansea, my accomplished colleague, Dr. Albert Günther, F.R.S., selected a general description of the building as the subject of his address to Section D.

I was unwilling then, in consideration of the time of the Section already given to the matter, to respond to appeals of some of our fellow-members for information as to how, and through whom, the new Museum came to be, and to be where it is; but now, honoured by my present position, I venture to hope that a brief outline of its genetic history, which I have been preparing for publication in a fuller form, may be condoned.

In the actual phase of our Science, its cultivators, especially the younger generation, do not rest upon the determination and description, however minute and exhaustive, of the acquisitions so rapidly accumulating of objects or "new species"; but devote themselves also, and more especially, to the investigation of their developmental phenomena.

It has, therefore, seemed to me that it would not be inappropriate, as being germane to the present phase of research, to submit to the Section a few words on the genesis of this new national edifice, generously provided by the State for the promotion of Biology.

On the demise, in 1856, of Sir Henry Ellis, K.T., then Principal Librarian of the British Museum, the Government, made aware of the growth of the Departments of natural history, more especially of geology and palæontology, since the foundation of the Museum in 1753, when the collections of printed books and manuscripts predominated, determined that, together with a principal librarian, there should be associated a new official having special charge of the collections of natural history, but under similar subordinate relations to the Trustees. To this official was assigned the title of "Superintendent of the Departments of Natural History," and I had the honour to be selected for this office.[2]

Almost my first work was to ascertain the extent of my charges, and I confess that I was unprepared to find that the galleries assigned for the arrangement and public exhibition of the several natural history series in the British Museum were so inadequate to these ends as to necessitate the storage of many unexhibited, and in great proportion rare and valuable specimens. This condition affected principally the collection of fossil remains, but in not much less degree that of the recent natural history.

One of my colleagues, Mr. Charles König, then Keeper of the Department of Mineralogy, and most eminent in that science, applied the gallery assigned thereto principally to the rare and beautiful specimens of his favourite subject. When the newer science of palæontology entered upon its rapid growth, and, on the demise of Mr. König, led to the formation of a distinct Department of Geology, the proportion of the British Museum set apart for natural history could not afford for the exhibition of the fossils and rock specimens more or other space than might be gained from or intercalated among the mineral cabinets in one and the same gallery, viz. that which had been originally assigned to Mr. König.

The store-vaults in the basement of the Museum became accordingly invaded by the rapidly-accumulating unexhibited geological specimens, as those receptacles had been, and continued to be, needed for the storage of such specimens, and especially the osteological ones, of the Department of Zoology.

[1] The official designation assigned by the Trustees to the building and its contents.

[2] The date of my appointment is May 26, 1856.

In 1854 Dr. John Ed. Gray, Keeper of the Zoology, reported on the unfitness of the locality of his stored specimens, and prayed for additional accommodation for them.[1] But, on the report of the architect, to whom such appeal was referred, the Trustees "declined to adopt Dr. Gray's suggestion," and recommended "that steps should be taken to obviate the deterioration of the specimens complained of by Dr. Gray in consequence of the damp condition of the vaults in which they are contained."[2] To renewed appeals by the experienced Keeper, and agreeably with his ideas on the nature and extent of the required additional space for the zoology, the Trustees recommended:—"An additional gallery to the Eastern Zoological Gallery, and the substitution of skylights for the side windows," with a view to an additional gallery at an elevation above the floor of the one in use; they also resolved:—"That accommodation be provided for the officers of the Natural History Departments on the roof of the Print-room."[3]

But the inadequacy for exhibition purposes of additional space which might be gained by the new gallery, or by the accessory wall-gallery attainable by stairs in the one in use,[4] was so impressed on my convictions, that I determined, in 1857, to submit to the Trustees a statement embodying estimates of space required for exhibition of all and several the departments of natural history, with the grounds of such estimates, including considerations based upon the ratio of increase during the ten years preceding my appointment, and the conditions likely to affect the proportional number of future annual additions.

This purpose, which I deemed a duty, I endeavoured to effect in a "Report, with a Plan," submitted on February 10, 1859, which Report, being forwarded by the Trustees to the Treasury, and being deemed worthy of consideration by Parliament, was "Ordered by the House of Commons to be printed, 11th March, 1859," and can still be obtained at the Office of Parliamentary Papers or Blue Books.[5]

The Report included, as I have stated, estimates of space for the then acquired specimens of the several departments of natural history, together with space for the reception of the additional specimens which might accrue in the course of a generation, or thirty years. It further recommended that such museum-building, besides giving the requisite accommodation to the several classes of natural history objects, as they had been by authority exhibited and arranged for public instruction and gratification, should also include a hall, or exhibition-space for a distinct department, adapted to convey an elementary knowledge of the subjects of all the divisions of natural history to the large proportion of public visitors not specially conversant with any of those subjects.

I may crave permission to quote from that part of my Report which has received the sanction of the "Commission on the Advancement of Science" of 1874: "One of the most popular and instructive features in a public collection of natural history would be an apartment devoted to the specimens selected to show type-characters of the principal groups of organised and crystallised forms. This would constitute an epitome of natural history, and should convey to the eye in the easiest way an elementary knowledge of the sciences."[6]

An estimate of the space required for such apartment is given, and it has been obtained in the new Museum of Natural History.

I ventured also on another topic in connection with the more immediate object of my Report. Previous experience at the museum of the Royal College of Surgeons had impressed me with the influence on improved applications of collections and on the ratio of their growth, through Lectures expository of their nature. I felt confident that, with concurrence of authorities, both relations would be exemplified under the actual superintendence at the British Museum. Moreover, such museum of natural history has wider influences over possessors and collectors of rarities and of desiderated specimens than one of restricted kind, as in Lincoln's Inn Fields. I concluded my Report, therefore, by referring to the lecture theatre shown in

[1] See Parliamentary Paper, or Blue Book, folio 1858, entitled:—"Copies of all Communications made by the Officers and Architect of the British Museum to the Trustees, respecting the want of space for exhibiting the Collections in that Institution," p. 4. [2] *Ib.* p. 5. [3] *Ib.* p. 25 and p. 28.

[4] In his report of December 29, 1856, Dr. Gray states:—"Scarcely half of the zoological collections is exhibited to the public, and their due display would require more than twice the space devoted to them."—*Ib.* p. 21. To any removal of the natural history to another site Dr. Gray was strongly opposed.

[5] Parliamentary Papers, "Report with Plan," &c. (126, i.), fol. 1859.

[6] Report, *ut supra*, p. 22.

my plan, and expressed my belief that "Administrators will consider it due to the public that the gentlemen in charge of the several departments of the National Collection of Natural History should have assigned to them the duty of explaining the principles and economical relations of such departments, in elementary and free lectures, as, *e.g.* on Ethnology, Mammalogy, Ornithology, Herpetology and Ichthyology, Malacology and Conchology, Entomology, Zoophytology, Botany, Geology, Palæontology, Mineralogy."

After the lapse of twenty years I have lived to see the fulfilment of all the recommendations, save the final one, of my Report of 1859. The lecture-theatre was erased from my plan, and the elementary courses of lectures remain for future fulfilment.

Considering that, in the probable communication of this Report to Parliament, I was addressing the representatives of the greatest commercial and colonising nation in the globe, representatives of an empire exercising the widest range of navigation and supreme in naval power, such nation and empire might well be expected by the rest of the civilised world to offer to students and lovers of natural history the best and noblest museum of the illustrations of that great division of general science.

But for such a museum, a site or superficial space of not less than eight acres was asked for, the proportion of such space to be occupied by the proposed building being, at first, limited, and dependent upon its architectural arrangement in one, two, or more storeys. But the effect of restricting the site or available superficial space to that, *e.g.* on which the Museum at Bloomsbury now stands, was significantly demonstrative of difficulties to come, and concomitantly indicative of the administrative wisdom which would be manifested by securing, in a rapidly growing metropolis, adequate space for future additions to the building which might be in the first place erected thereupon.

Nevertheless one or two of my intimate and confidential friends dissuaded me from sending in a Report which might be construed or misinterpreted as exemplifying a character prone to inconsiderate and extravagant views, and such as might even lead to disagreeable personal consequences. Moreover the extent of space reported for seemed inevitably to involve change of locality. Two of my colleagues occupied the elegant and commodious residences attached to the British Museum; and it was possible that provision for such residences marked in the plan which accompanied my Report might not be adopted. Moreover no statement of grounds for adequate space requirements for the whole of the National Museum of Natural History had previously been submitted to authority. The legislative mind had not been prepared for calm and due consideration of the subject. Still I flattered myself that, by whomsoever the details and aims and grounds of my Report were known and comprehended, any strong opposition on the part of Parliament could hardly be expected. Nevertheless, an Irish Member seeing a way to a position in the House which is gained by the grant of a Committee of Inquiry, of which the Mover becomes Chairman, made my Report and Plan the ground of a motion to that effect, which was carried. The Select Committee, after taking the evidence published in the Blue Book (ordered to be printed August 10, 1860, quarto, pp. 238, with ten plans), reported against the removal of the Natural History Collections from the British Museum. As to the chief reasons alleged for such removal the Report states that with one "eminent exception the whole of the scientific naturalists examined before your Committee, including the Keepers of all the Departments of Natural History in the British Museum, are of opinion that an exhibition on so large a scale tends alike to the needless bewilderment and fatigue of the public, and the impediment of the studies of the scientific visitor . . . . Your Committee, therefore, recommend the adoption of the more limited kind of exhibition advocated by the other witnesses, in preference to the more extended method recommended by Prof. Owen."

Lest however the House might attach undue weight to the exceptional testimony, the chairman of the Committee deemed it his duty, in bringing up the Report, to warn the House of the character of such testimony, and his speech left, as I was told, a very unfavourable impression as regards myself. I was chiefly concerned to know what might be put upon record in "Hansard." In that valuable work hon. members revise their reported utterances before the sheets go to press. I was somewhat relieved to find Mr. Gregory regretting that "a man whose name stood so high should connect himself with so foolish, crazy, and extravagant a scheme, and should persevere in it after the folly had been pointed out by most unexceptionable witnesses. . . . "They had on one side, and standing alone, Prof. Owen and his ten-acre scheme, and on the other side all the other scientific gentlemen, who were perfectly unanimous in condemning the plan of Prof. Owen as being utterly useless and bewildering. . . . "Among these gentlemen were Prof. Huxley, Prof. Maskelyne, Mr. Waterhouse, Dr. Gray, Sir Roderick Murchison, Mr. Thomas Bell, P.G.S., Dr. Sclater, Sec.Z.S., Mr. Gould, and Sir Benjamin Brodie. To give the House some idea of that gigantic plan, he might mention that a part of it consisted of galleries 850 feet in length for the exhibition of whales. The scientific men examined on the subject, one and all, disapproved of that plan *in toto*; and they advocated what was technically called a 'typical mode of exhibition.'"[1]

In point of fact that Supplementary Exhibition Room which was planned and recommended for the purpose I have already cited, was urged by the instructor of Mr. Gregory as the sole reasonably required National Museum of Natural History, for which the nation ought to be called upon to provide space and funds, a conclusion subsequently adopted and unanimously recommended by the Royal Commission on Science.[2]

Although grief was natural and considerable at this result, not without mortification at the reception by Parliament of the "Report and Plan" submitted thereto, I now feel grateful that the sole responsibility of their author is attested in the pages of a Work[3] which will last as long as, and may possibly outlast, the great legislative organisation whose debates and determinations are therein authoritatively recorded.

I was not, however, cast down, nor did I lose either heart or hope; I was confident in the validity of the grounds of my appeal, and foresaw in the inevitable accumulations year by year, the evidence which would attest its soundness and make plain the emergency of the proposed remedy.

Moreover, there was one who, though not a naturalist, had devoted more time, pains, and thought to the subject than had been bestowed by any—whether naturalist or administrator—who testified adversely thereon. The Right Hon. William Ewart Gladstone, an elected Trustee of the British Museum, took nothing on trust; he explored with me in 1861 every vault and dark recess in the Museum which had been or could be allotted to the non-exhibited specimens of the natural history, those, viz. which it was my aim to utilise and bring to light. He gave the same attention to the series selected for exhibition in the public galleries, and appreciated the inadequacy of the arrangements to that end. He listened to my statements of facts, to the grounds of prevision of annual ratios of increase, to the reasons for providing space therefor, to my views of the aims of such exhibitions, and to the proposed extended applications and elucidations of the collections. Mr. Gladstone tested every averment, and elicited the grounds of every suggestion, with a tact and insight that contrasted strongly with the questionings in Mr. Gregory's committee-room, where too often vague interrogations met with answers to match.

Conformably with Mr. Gladstone's convictions, he as Chancellor of the Exchequer moved, May 12, 1862, for "Leave to bring in a Bill for removal of portions of the Trustees' Collections in the British Museum."

On May 19, when the Bill was to be read a second time, a new, unexpected, and formidable antagonist arose. Mr. Disraeli early got the attention of the House to a speech, warning hon. members of the "progressive increase of expenditure on civil estimates," and laying stress on the fact that the "estimates of the actual year showed no surplus."[4] The influence of this advocacy of economy is exemplified in the debate which ensued. For repetitions of the nature and terms of objections to the Report and plan, as already denounced by Mr. Gregory, Mr. Bernal Osborne, and others, reference may be made to the volume of "Hansard" cited below. An estimable hon. member, whose words had always and deservedly carried weight with the country party, lent his influence to the same result. Mr. Henley, representative of Oxfordshire, said:—"All the House knew was that a building was to be put up somewhere. He considered this a bad way of doing business, particularly at a time when nobody could be sanguine that the finances of the country were in a flourishing state. Let the stone once be set rolling, and then all gentlemen of science and taste would have a kick at it, and it would be knocked from one

[1] "Hansard." Debate of July 22, 1861, pp. 1861, 1918.
[2] Fourth Report, p. 4. [3] "Hansard," *ut supra*. [4] *Ib.* 1862, p. 1927.

to the other, and none of them probably would ever live to see an end of the expense."[1]

Permit me to give one more example of the baneful influence of the opening speech on our great instrument of scientific progress. Mr. Henry Seymour, Member for Poole, said:—"If a foreigner had been listening to the debates of that evening it must have struck him that it was, to say the least, a rather curious coincidence that a proposal to vote 600,000*l*. for a new collection of birds, beasts, and fishes at South Kensington should have been brought forward on the very evening when the Leader of the Opposition had made a speech denouncing that exorbitant expenditure—a speech, he might add, which was re-echoed by many Liberal members of the House."[2]

It was however not a "curious," but a "designed coincidence." Mr. Disraeli, knowing the temper of the House on the subject, and that the estimates for the required Museum of Natural History were to be submitted by Mr. Gladstone, chose the opportunity to initiate the business by an advocacy of economy which left its intended effect upon the House. In vain Lord Palmerston, in reply to the Irish denunciators, proposed as a compromise to "exclude whales altogether from disporting themselves in Kensington Gardens."[3] The Government was defeated by a majority of ninety-two, and the erection of a National or British Museum of Natural History was postponed, to all appearance indefinitely, and in reality for ten years.

Nevertheless, neither averments nor arguments in the House on May 19, 1862, nor testimonies in the hostile Committee of 1860, 1861, had shaken my faith in the grounds on which the "Report and Plan of 1859" had been based. The facts bearing thereupon, which it was my duty to submit in my "Annual Reports on the Natural History Departments of the British Museum," would, I still hoped, have some influence with hon. members of the legislature, to whom those Reports are transmitted.

The annual additions of specimens continued to increase in number and in value year by year. I embraced every opportunity to excite the interest of lovers of natural history travelling abroad and of intelligent settlers in our several colonies to this end, among the results of which I may cite the reception of the Aye-Aye, the Gorilla, the Dodo, the Notornis, the maximised and elephant-footed species of Dinornis, the representatives of the various orders and genera of extinct Reptilia from the Cape of Good Hope, and the equally rich and numerous evidences of the extinct Marsupialia from Australia, besides such smaller rarities as the animals of the Nautilus and Spirula.

Wherever room could be found in the exhibition galleries at Bloomsbury for these specimens, stuffed or as articulated skeletons, or as detached fossils, they were squeezed in, so to speak, to mutely manifest to all visitors, more especially administrative ones, the state of cram to which we were driven at Bloomsbury.

Another element of my Annual Reports was the deteriorating influence on valuable specimens of the storage vaults and the danger of such accumulations to the entire Museum and its priceless contents. And here perhaps you may deem some explanation needful of the grounds of the latter consideration addressed to economical granters of the national funds.

The number of specimens preserved in spirits of wine amounted to thousands; any accidental breakage, with conflagration, in the subterraneous localities contiguous with the heating-apparatus of the entire British Museum, would have been as destructive to the building as the gunpowder was meant to be when stored in the vaults beneath King James's Houses of Parliament.

At this crisis the "Leading Journal," after the stormy debate of May 19, 1862, made the following appeal to me:—"Let Mr. Owen describe exactly the kind of building that will answer his purpose, that will give space for his whales and light for his humming-birds and butterflies. The House of Commons will hardly, for very shame, give a well-digested scheme so rude a reception as it did on Monday night."[4]

My answer to this appeal was little more than some amplification, with additional examples, of the several topics embodied in the original Report. The pamphlet "On the Extent and Aims of a National Museum of Natural History," with reduced copies of the plans, went through two editions, and no doubt had the effect anticipated by the able Editor.

Another element of reviving hope was the acceptance by Mr. Gregory of the government of a tropical island.

The sagacious Prime Minister accurately gauged the modified feeling—the subsiding animosity—of Parliament on the subject, and submitted (June 15, 1863) a motion "for leave to purchase five acres for the required Natural History building." The choice of locality he left to honourable members. Lord Palmerston pointed out that the requisite extent of site could be obtained at Bloomsbury for 50,000*l*. per acre, and that it could be got at South Kensington for 10,000*l*. per acre; and his lordship distinctly stated that the space, in either locality, would be bought for the purpose of a Museum of Natural History. The purchase of the land at South Kensington was accordingly voted by 267 against 135, and thus the Government proposition was carried by a majority of 132. By this vote the decision of Mr. Gregory's Committee was virtually annulled.

In a conversation with which I was favoured by Lord Palmerston, I interposed a warning against restriction of space, and eventually eight acres of ground were obtained, including the site of the Exhibition Building of 1862, opposite Cromwell Gardens, and that extent of space is now secured for actual and prospective requirements of our National Museum of Natural History.

I am loth to trespass further on the time of the Section, but a few words may be expected from me of the leading steps to the acquisition of the present edifice, occupying a portion—about one-third—of that extent of ground.

Mr. Gladstone, adhering to the convictions which led him to submit his financial proposition of May, 1862, honoured me, at the close of that session of Parliament, with an invitation to Hawarden to discuss my plans for the Museum Building; and, after consideration of every detail, he requested that they might be left with him. He placed them, with my written expositions of details, in the hands of Sir Henry A. Hunt, C.B., responsible adviser on buildings, &c., at the Office of Works, with instructions that they should be put into working form, so as to support reliable estimates of cost. I was favoured with interviews with Sir Henry, resulting in the completion of such working plans of a museum, including a central hall, an architectural front of two storeys, and the series of single-storeyed galleries extending at right angles to the front, as shown in my original Plan. I was assured that such plan of building affording the space I had reported on, would be the basis to be submitted to the professional Architect whenever the time might arrive for Parliamentary sanction to the cost of such building.

Here I may remark that experiments which preceded the substitution, in 1835, of the actual Museum of the Hunterian Physiology at the Royal College of Surgeons, for the costly, cumbrous, and ill-lit building, with its three-domed skylights, which preceded it, had led to the conclusion that the light best fitted for a museum was that in which most would be reflected from the objects and least directly strike upon the eye; and this was found to be effected by admittance of the light at the angle between the wall and roof. But this plan of illumination is possible only in galleries of one storey, or the topmost in a many-storeyed edifice. Such system of illumination may be seen in every gallery of the museum described to you last year at Swansea, save those of the storeys of the main body below the sky-lit one which necessitate side windows.

I subjoin a copy of the letter from Sir Henry A. Hunt, conveying his conclusions respecting the plan of building discussed with him:—

"4, *Parliament Street, September* 25, 1862

"MY DEAR SIR,—I return you the drawings of the proposed Museum of Natural History at South Kensington. In May last I told Mr. Gladstone that the probable cost of covering five acres with suitable buildings would be about 500,000*l*., or 100,000*l*. per acre.

"The plan proposed by you will occupy about four acres, and will cost about 350,000*l*, or nearly 90,000*l*. per acre.

"Having prepared sketches showing the scheme suggested by you, I have been able to arrive more nearly at the probable cost than I had the means of doing in May last. But, after all, the difference is not great; although the present estimate is a more reliable one than the other. It is right, however, to state that the disposition of the building as proposed by you will give a greater amount of accommodation, and admit of a cheaper mode of construction, than I had calculated upon in May (relatively with the space intended to be covered), and therefore I think your plan far better adapted for the Museum than the plan I took the liberty to suggest to Mr. Gladstone.

"Believe me, &c,

"(Signed) HENRY A. HUNT"

[1] "Hansard," p. 1932. [2] *Ib*. 1862, p. 1918. [3] *Ib*. p. 1931.
[4] The *Times*, May 21, in a leader on the Museum Debate.

Sir H. A. Hunt had previously formed an estimate of cost for the Chancellor of the Exchequer on inspection of the Report and plan in the Parliamentary paper of March, 1859. The letter to which I refer I regard as an antidote to some previous quotations from adverse members of Parliament.

The working plans of Sir Henry A. Hunt were subsequently submitted for competition, and the designs of the accomplished and lamented Capt. Fowke, R.E., obtained the award in 1864. His untimely death arrested further progress or practical application of the prize designs.

In 1867 Lord Elcho pressed upon the House of Commons, through the Hungerford Bridge Committee, the Thames Embankment as a site for the New Museum of Natural History, but unsuccessfully. The debates thereon, nevertheless, caused some further delay.

In 1871 a vote of 40,000*l.* for beginning the Museum Buildings at South Kensington was carried without discussion. In 1872 a vote of 29,000*l.* for the same building was opposed by Lord Elcho, but was carried by a majority of 40 (85 against 45).

On the demise of Capt. Fowke Mr. Alfred Waterhouse was selected as architect. He accepted the general plans which had been sanctioned and approved by Sir H. A. Hunt and by Capt. Fowke, and I took the liberty to suggest, as I had previously done to Capt. Fowke, that many objects of natural history might afford subjects for architectural ornament; and at Mr. Waterhouse's request I transmitted numerous figures of such as seemed suitable for that purpose. I shall presently refer to the beautiful and appropriate style of architecture which Mr. Waterhouse selected for this building, but am tempted to premise a brief sketch of what I may call the "Genealogy of the British Museum," or what some of my fellow labourers, agreeably with the actual phase of our science, may prefer to call its "Phylogeny."

Sir Hans Sloane, M.D., after a lucrative practice of his profession in the then flourishing colony of Jamaica, finally settled at Chelsea, and there accumulated a notable museum of natural history, antiquities, medals, cameos, &c., besides a library of 50,000 volumes, including about 350 portfolios of drawings, 3500 manuscripts, and a multitude of prints. These specimens were specified in a MS. catalogue of thirty-eight volumes in folio, and eight volumes in quarto. Sir Hans valued this collection at the sum of 80,000*l.*; but at his death, in 1753, it was found that he had directed in his "Will" that the whole should be offered to Parliament for the use of the public on payment of a minor sum, in compensation to his heirs. This offer being submitted to the House of Commons, it was agreed to pay 20,000*l.* for the whole. At the same time the purchase of the Cottonian Library and of the Harleian MSS. was included in the Bill.[1]

The following are the terms of the enactment:—

Act 26, George II., Cap. 22 (1753).—Sections IX. and X.

"(IX.) And it be enacted by the authority aforesaid, that within the cities of London or Westminster or the suburbs thereof, one general repository shall be erected or provided in such convenient place and in such manner as the trustees hereby appointed, or the major part of them, at a general meeting assembled, shall direct for the reception not only of the said museum or collection of Sir Hans Sloane, but also of the Cottonian Library and of the additions which have been or shall be made thereunto by virtue of the last will and testament of the said Arthur Edwards, and likewise of the said Harleian collection of manuscripts and of such other additions to the Cottonian Library as, with the approbation of the trustees by this Act appointed, or the major part of them, at a general meeting assembled, shall be made thereunto in manner hereinafter mentioned, and of such other collections and libraries as, with the like approbation, shall be admitted into the said general repository, which several collections, additions, and library so received into the said general repository shall remain and be preserved therein for public use to all posterity.

"(X.) Provided always that the said museum or collection of Sir Hans Sloane, in all its branches, shall be kept and preserved together in the said general repository whole and entire, and with proper marks of distinction."

[1] In his letter of February 14, 1753, to his friend Mann, Horace Walpole, then Member for Lynn, writes:—"You will scarce guess how I employ my time, chiefly at present in the guardianship of embryos and cockle-shells. Sir Hans Sloane is dead, and has made me one of the trustees of his museum, which is to be offered for twenty-thousand pounds to the King and Parliament and (in default of acceptance) to the Royal Academies of Petersburg, Berlin, Paris, and Madrid. He valued it at four-score thousand, and so would any one who loves hippopotamuses, sharks with one ear, and spiders as big as geese. The king has excused himself, saying he did not think that there were twenty thousand pounds in the Treasury."—"Letter to Horace Mann," 8vo, vol. iv. p. 32.

The trustees appointed under the Act are of four classes: Royal, Official, Family, and Elected. The first class includes one trustee appointed by the Sovereign; the second class includes the Lord Archbishop of Canterbury, the Lord High Chancellor, the Speaker of the House of Commons, and twenty-two other high officials and presidents of societies. The three first in this class are designated "Principal Trustees," and in them is vested the patronage or appointment to every salaried office save one in the British Museum; the exception being the Principal Librarian, who is appointed by the Sovereign. Of the Family Trustees, the Sloane collections are now represented by the Earl of Derby and the Earl of Cadogan, the Cottonian Library by the Rev. Francis Annesley and the Rev. Francis Hanbury Annesley, the Harleian manuscripts by Lord Henry, C. G. Gordon-Lennox, M.P., and by the Right Hon. George A. F. Cavendish Bentinck, M.P. Among the Elected Trustees the honoured name of Walpole, associated with the origin of the British Museum, is continued by the Right Hon. Spencer Horatio Walpole, M.P., to whom the requisite Parliamentary business of the Museum is usually confided.

I may call attention to "the suburbs of London or Westminster" as one of the localities specified in the original Act of Parliament, and such situation was selected for the locality of the Library and the Museum. The Government issued lottery-tickets to the amount of 300,000*l.*, out of the profits of which the 20,000*l.* for the Sloanian Museum was paid and purchase made of a suitable building, with contiguous grounds for its reception and the lodgment of keepers.

To the north of the metropolis, about midway between the two cities of London and Westminster, there stood, in 1753, an ancient family mansion called Montague House. This is defined by Smollet in his "History of England" as "one of the most magnificent edifices in England."[1] Its style of architecture was that of the Tuilleries in Paris. From London it was shut off by a lofty brick wall, in the middle of which was a large ornamental gateway and lodge, through which, in my earlier years as a student of natural history, I have often passed to inspect, through the kindness of the then keepers of mineralogy and zoology, and make notes on, the Sloanian and subsequently-added rarities.

To the north of Montague House were the extensive gardens, beyond which stretched away a sylvan scene to the slopes of Highgate and Hampstead Hills.

The original location of the British Museum was more apart and remote from the actual metropolis and less easy of access than is the present Museum of Natural History at the West End.

The additions to the natural history series, which accrued from 1753 to 1833, together with the growth of other departments, necessitated provision of corresponding conservative and exhibition spaces. These were acquired by the erection, on the site of Montague House, of the present British Museum, the architect, Sir Sidney Smirke, adopting the Ionic Greek style.

The extent of space afforded by this edifice in comparison with that of its predecessor was such as to engender a conviction that it would suffice for all subsequent additions. The difficulty in our finite nature and limited capacity of looking forward is exemplified in such names as New College at Oxford, Newcastle, New Street, New Bridge, &c., as if nothing was ever to grow old; and the same restricted power of outlook affects our prevision of requirements of space for ever-growing collections.

The Printed Book Department, which took the lion's share of the then new British Museum, found itself compelled in the course of one generation to appropriate the quadrangle left by Smirke in order to admit light to the windows of the galleries, looking that way or inwards.

From analogy I foresee that some successor of mine may exemplify human short-sightedness in my limit of demand to eight acres for the growth of the present Museum.

However, these acres, after conflicts stretching over a score or more of years, have at last been acquired for due display and facilities of study of the subjects of Section D.

Amongst the works of architectural art which adorn the metropolis, Westminster Abbey and St. Paul's Cathedral stand supreme. Of later additions may with them be named the noble example of the Perpendicular Gothic selected by Barry for the Houses of Parliament, and, I may be permitted to add, the new Law Courts, which exemplify the more severe style of the Thirteenth-century Gothic.

[1] Ed. 1825, p. 332.

Mr. Alfred Waterhouse, R.A., for the realisation of the plans and requirements of our Museum of Natural History, has chosen an adaptation of the Round-arched Gothic, Romanesque, or Romaic of the twelfth century. No style could better lend itself to the introduction, for legitimate ornamentation, of the endless beautiful varieties of form and surface sculpture exemplified in the animal and vegetable kingdoms. But the skill in which these varieties have been selected and combined to produce unity of rich effects will ever proclaim Mr. Waterhouse's supreme mastery of his art.

I need only ask the visitor to pause at the grand entrance, before he passes into the impressive and rather gloomy vestibule which leads to the great hall, and prepares him for the flood of light displaying the richly-ornamented columns, arcades, and galleries of the Index Museum.

In the construction of a building for the reception and preservation of perishable objects, the material should be of a nature that will least lend itself to the absorption and retention of moisture. This material is that artificial stone called terra-cotta. The compactness of texture which fulfils the purpose in relation to dryness is also especially favourable for a public edifice in a metropolitan locality. The microscopic receptacles of soot-particles on the polished surface of the terra-cotta slabs are reduced to a minimum; the influence of every shower in displacing those particles is maximised. I am sanguine in the expectation that the test of exposure to the London atmosphere during a period equal to that which has elapsed since the completion of Barry's richly ornamented palace at Westminster, now so sadly blackened by soot, will speak loudly in favour of Mr. Waterhouse's adoption of the material for the construction of the National Museum of Natural History. A collateral advantage is the facility to which the moulded blocks of terra-cotta lend themselves to the kind of ornamentation to which I have already referred.

In concluding the above sketch of the development of our actual Museum of Natural History, I may finally refer, in the terms of our modern phylogenists, to the traceable evidences of "ancestral structures." In the architectural details of the new Natural History Museum you will find but one character of the primitive and now extinct museum retained, viz. the Central Hall. In Montague House there were no galleries, but side-lit saloons or rooms of varying dimensions and on different storeys.

In its successor, the Museum developed on its site at a later period, we find galleries added: that, for example, which was appropriated to the birds and shells being 300 feet in length. This architectural organisation still exists at Bloomsbury.

The Museum, which may be said to have budded off, has risen to a still higher grade of structure after settling down at South Kensington. In its anatomy we find, it is true, the central hall and long side-lit galleries; but in addition to these inherited structures we discern a series of one-storeyed galleries, manifesting a developmental advance in the better admission of light and a consequent adaptation of the walls as well as the floor to the needs of exhibition.[1]

Should the Section, as did the Académie des Sciences in relation to the passage cited, kindly condone such application to human contrivances of the current genealogical or phylogenetic language applied to vital structures, your President need hardly own his appreciation of the vast superiority of every step in advance which is manifested in existing as compared with extinct organisms. And thus, sensible as far as human faculty may comprehend them, that organic adaptations transcend the best of those conceived by the ingenuity of man to fulfil his special needs, he would ask whether analogy does not legitimately lead to the inference, for organic phenomena, of an Adapting Cause operating in a corresponding transcendent degree?

In conclusion, I am moved to remark that a Museum giving space and light for adequate display of the national treasures of Natural History may be expected to exert such influence on the progress of Biology as to condone, if not call for, a narrative of the circumstances attending its formation in the Records of the British Association for the Advancement of Science.

[1] In the notable reply (*Annales des Sciences Naturelles*, 1829) to an illustration of the unity of composition or of plan in Cephalopods and Vertebrates, by bending one of the latter so as to bring the pelvis in contact with the nape, advocated by Geoffroy St. Hilaire, Cuvier did not deem it too trivial to call in architecture to elucidate his objections. "La *composition d'une maison*, c'est le nombre d'appartemens ou de chambres qui s'y trouve; et son *plan*, c'est la disposition réciproque de ces appartemens et de ces chambres. Si deux maisons contenaient chacune un vestibule, une antichambre, une chambre à coucher, un salon, et une salle à manger, on dirait que leur *composition est la même*; et si cette chambre, ce salon, &c., étaient au même étage arrangés dans le même manière, on dirait aussi que leur *plan est le même*. Mais si leur ordre était différent, si de plain-pied dans une des maisons, ces pièces étaient placées dans l'autre aux étages successifs, on dirait qu'avec une composition semblable ces maisons sont construites sur des plans différens" (p. 245).

OUR ASTRONOMICAL COLUMN

ENCKE'S COMET.—We continue the ephemeris of this comet in the contracted form adopted in NATURE, vol. xxiv. p. 292, from the calculations of Dr. O. Backlund of Pulkowa:—

At Berlin midnight

	R.A. h. m. s.	Decl. ° ′	Log. distance from Sun.	Log. distance from Earth.
Sept. 2	4 23 4	+35 53·2	0·1659	0·0222
4	4 31 17	36 35·9		
6	4 40 7	37 19·2	0·1493	9·9885
8	4 49 38	38 3·0		
10	4 59 56	38 46·8	0·1317	9·9535
12	5 11 6	39 30·3		
14	5 23 16	40 12·8	0·1128	9·9173
16	5 36 32	40 53·3		
18	5 51 1	41 30·9	0·0926	9·8805
20	6 6 51	42 4·1		
22	6 24 7	42 30·9	0·0709	9·8439
24	6 42 54	42 49·3		
26	7 3 13	42 56·3	0·0474	9·8089
28	7 25 0	42 49·3		
30	7 48 5	42 24·9	0·0219	9·7776
Oct. 2	8 12 13	+41 40·4		

In 1848, when the perihelion passage occurred eleven days later than it will do in the present year, the comet was remarked to be "just visible" to the naked eye at Harvard Observatory, U.S., on the morning of October 9, when the theoretical intensity of light was 4·3, and it was "plainly visible" to the naked eye on the morning of November 4, with an intensity of 9·5. The latter is a greater value than will be attained at this appearance, the maximum being 7·5 on November 9. On October 10 the calculated brightness will be equal to that, when it was just visible without the telescope in 1848, but moonlight will interfere at the time. For about four weeks after September 10 the comet will not set in London. As we have already stated it will be nearest to the earth on October 11, and in perihelion on November 15.

[Since the above was in type we learn from Mr. A. A. Common that he detected Encke's comet with his three-feet reflector at Ealing, shortly before midnight on Saturday last. On the following night, when it was better seen, its diameter was about 2′, and there was a central condensation of light.]

SCHAEBERLE'S COMET.—This comet will soon be well observable in the other hemisphere. The following track depends upon elements which Dr. v. Hepperger has calculated from observations to August 11:—

At Berlin Midnight.

	R.A. h. m.	Decl. ° ′	Log. distance from Earth.	Intensity of light.
Sept. 1	13 30·0	+11 27	9·8329	12·5
5	13 52·7	+ 1 41	9·8965	8·5
9	14 7·7	− 5 32	9·9606	5·7
13	14 18·1	10 52	0·0198	3·8
17	14 25·7	14 56	0·0725	2·6
21	14 31·4	18 8	0·1191	1·9
25	14 36·1	20 42	0·1601	1·4
29	14 40·0	22 51	0·1962	1·0
Oct. 3	14 43·5	24 41	0·2284	0·8
7	14 46·6	−26 16	0·2569	0·6

The intensity of light on July 18, the date of the first European observation, is taken as unity.

NOTES

THE Royal Gardens, Kew, have just received, through the kind exertions on their behalf of Sir Ferdinand von Mueller, K.C.M.G., F.R.S., Government Botanist, Melbourne, perhaps the most remarkable Australian Cycadaceous stem which has ever been imported into this country. It is about four feet high, five and a half feet in circumference, and weighs about six hundredweight. It is the type of a new species described by von

Mueller as *Macrozamia Moorei*, in honour of Mr. Charles Moore, F.L.S., the Director of the Botanic Garden, Sydney. The exhibition of two stems (of which that secured for and sent to Kew is one) in the Queensland Court at the Melbourne Exhibition, seems to have drawn attention to the species. The plants appear to have been obtained from the mountainous district near Springsure in Queensland, where specimens have been seen twenty feet in height, with a girth of six feet four inches, cones measuring two to three feet in length, and leaves seven feet long. The stem at Kew has been placed in the Palm House, where it can scarcely fail to be an object of interest. It is in excellent condition, and there is every reason to hope that it will in time push a new crown of leaves. But even if it does not it will at any rate form, as Sir Ferdinand von Mueller has suggested, a unique museum specimen.

THE *Gazette* contains the official notice of the appointment of a Royal Commission, consisting of Mr. Bernhard Samuelson, M.P., F.R.S., Prof. H. E. Roscoe, D.C.L., F.R.S., Mr. Philip Magnus, Mr. John Slagg, M.P., Mr. Swire Smith, and Mr. William Woodall, M.P., "to inquire into the instruction of the industrial classes of certain foreign countries in technical and other subjects, for the purpose of comparison with that of the corresponding classes in this country; and into the influence of such instruction on manufacturing and other industries at home and abroad."

THE Queen has signified her pleasure to confer upon Mr. MacCormac, of St. Thomas's Hospital, Honorary Secretary-General of the late International Medical Congress, the honour of knighthood.

THE Meteorological Station to be erected at Pavia will be under the direction of Prof. Cantoni, who will establish a station for terrestrial physics, for the investigation of the influence of heat, light, electricity, &c., on vegetation in general, and some cultivations in particular, and also for the observation of the diurnal and annual variations of terrestrial magnetism.

THE scientific activity of Paris is at present almost exclusively concentrated on electricity, and the Paris Electrical Exhibition will have a scientific significance which is quite unusual. The initiative has been taken by the German Government, which has sent several professors to deliver lectures on the objects exhibited by that nation. Dr. Christian, of the Physiological Museum of Berlin University, gives explanations every day at two o'clock of the galvanometers on the Siemens system constructed by him. On Monday M. du Moncel, Member of the Institute, delivered a lecture on Telegraphy at ten o'clock in the morning, and conducted his audience through the galleries to visit the instruments described by him. Other lectures have been advertised for the different days of the week from August 29 to September 3. The Exhibition was opened to the Press last Friday and to the public last Saturday, at night from eight to eleven.

THE electric tramway in Paris has at length begun to work, and has several times gone backwards and forwards. A single overhead tube was tried at first to convey the current, but it was found impossible to insulate the rail by which it returned. Two overhead copper tubes are now used, along each of which at the bottom runs a longitudinal slit. A wire passing through the slit is attached to the tramcar beneath, and above to a small wheel which runs freely in the copper tube. As the car advances it draws along the little wheels through each tube, and thus maintains the connection.

A NUMBER of natives of Tierra del Fuégo are at present at the Jardin d'Acclimatation in Paris.

THE French Government have resolved to grant a subvention for erecting a statue in Franche Comté to Claude de Jouffroy as the inventor of steam vessels. The French Académie des Sciences at its last sitting adopted a report of M. de Lesseps in favour of Jouffroy's claim to that distinction and to public gratitude.

THE new botanical lecture theatre of the University of Edinburgh was used by Prof. Dickson for the first time during the past summer session. It is built from the plans of Mr. Robertson, of H.M. Board of Works, and is a large octagonal building lighted from the roof and by windows on six sides. It is seated for 600, and had this year to contain 450 students. The acoustic arrangements are perfect. The old lecture-room has been converted into a general laboratory, while the former laboratory becomes a private room. The practical teaching has been conducted as formerly by Mr. Geddes, lecturer on Zoology in the School of Medicine, assisted by Mr. J. M. Macfarlane, B.Sc. Besides the usual elementary class, a class for advanced workers has also been started, a considerable number of investigations have been prosecuted, and instruction in drawing has been provided. The latter arrangement has been peculiarly successful.

THE twelfth meeting of German Anthropologists was opened at Ratisbon on August 8 by the president, Prof. Fraas. Some 250 members were present. The secretary, Prof. Ranke, read the report on the widely-extended activity of the Society. Prof. Ohlenschläger (Munich) spoke on the Roman epoch in Bavaria and the excavations in the Roman burial-ground near Ratisbon. Other addresses were delivered by Professors von Virchow, Tischler, Undset, Groos, Mehlis, Klopfleich, Schaaffhausen, Vater and Török. The next meeting-place will be Frankfort.

THE second meeting of Austrian Anthropologists took place at Salzburg on August 12-16. Some 270 gentlemen were present, principally Germans, Norwegians, and Russians. Of eminent scientific men we may mention Prof. von Virchow, the travellers Dr. Holub and Dr. Nachtigal, Prof. Steub (Munich), and Prof. Johannes Ranke. Count Wurmbrand was elected president, and Baron von der Sacken vice-president. Addresses were delivered by Herren Prinzinger, Much, and Zillner on the ancient inhabitants of Noricum, which the former two said were Germans: Herr Zillner however believed them to have been Kelts. On the second day Crown-Prince Rudolf took part in the meeting. Count Wurmbrand spoke on the development of the forms of bronzes and clay vessels, Herr Wolderich on prehistoric dogs, Holub on the South African negro tribes. Herr Maska reported on the discoveries near Schamberg, and Professors von Virchow and Schaaffhausen had an animated debate on the jaw of Neutitschein. Other addresses were delivered by Professors Tischler, Luschan, von der Sacken, Müllner, and Schaaffhausen. The usual excursions terminated the meeting.

THE death is announced of Prof. L. Spangenberg, director of the technical Versuchsanstalt and Professor of the Engineering Sciences at the Technical High School of Berlin. He died on August 6 last.

THE celebrated Egyptologist, Brugsch Pacha, has changed his residence from Cairo to Berlin, where he will lecture at the University.

THE European Vice-Consul at Tchesme telegraphs to Constantinople on August 27 that Tchesme and Chio were, on the night of the 26th, visited by an earthquake still more terrible than that of the 3rd of April. The destruction of property, he says, is considerable, and the inhabitants are in despair. Contemporaneously with the shocks of earthquake felt at Chio and Tchesme, the earth at Zante is reported to have suddenly given

out intense heat, accompanied by a strong breeze from the east, causing much alarm. These phenomena, however, subsided immediately. On the 24th inst. the entire island was enveloped in smoke, clouds from the west-south-west obscuring the sea from noon until dusk. Masses of calcined leaves also fell throughout the island.

A SEVERE shock of earthquake is reported to have been experienced in the mining district of Teversal, in Nottinghamshire, about noon on Friday last. In one of the pits belonging to the Stanton Ironworks Company the miners were so alarmed at the shock and the accompanying noise, that, thinking an explosion had occurred, they rushed to the mouth of the pit. In the Pear Tree Inn, Faekley, bricks were removed from the chimney, and the same thing was noticed in a house at Teversal. The station-master at the latter village, while sitting in his house, was thrown from his seat by the shock, and a quantity of plaster was detached from the ceiling. There was no explosion in the mines or other circumstance to account for the phenomena, and an upheaval of the floor of one of the pits indicated that the cause of disturbance was below the workings. One of the pits is 430 yards deep. The shock travelled in a north-west direction.

THE programme of the Congress of German Antiquarians, which will meet at Frankfort on September 11-15, has now been published. On the 11th the twenty-five years jubilee of the Frankfort Antiquarian Society will be celebrated.

THE professorship of Natural History and Geology at the Royal Agricultural College, Cirencester, vacant by the resignation of Prof. M. G. Stuart, has been filled by the appointment of Mr. Allen Harker, late of the Zoological Station, Naples.

ONE of the exhibits at the International Medical and Sanitary Exhibition was a "Compact School Collection for Use in Teaching the Chemistry of Foods," suggested by W. Stephen Mitchell, M.A. This form of case has been arranged for the purpose of affording, at a low cost, help to teachers in giving demonstrations on the chemistry of foods. The leading idea is that the teachers will be able to have on the walls of their schoolrooms the actual objects they are talking about, and the children will be familiarised by having always before them, not simply words or diagrams, but samples of the things themselves. Mr. Mitchell believes, as he stated in a paper read before the Society of Arts and the Domestic Economy Congress held at Birmingham, that with such diagrams as he showed, having lines of different lengths to represent quantities, greater accuracy of the knowledge of quantities can be conveyed than by showing the measured quantities in heaps as is the plan adopted at Bethnal Green. This does not get over the difficulty of showing the gases. Teachers must learn how to prepare these to show to their classes. The apparatus and materials for doing this are not costly, and are described in the "Shilling Chemistry Primer" published by Macmillan and Co. The cases are arranged with a sliding panel in front, so that the bottles can be taken out.

AT Castrop (Westphalia) a meteor was observed in the north-east sky on July 30 at 8.15 p.m. It moved in the direction from north-west to south-south-east. A meteoric stone weighing about 5 lbs. fell in the immediate vicinity of a field labourer, and penetrated into the ground to the depth of 1 metre. It was intensely hot, and was afterwards forwarded to Herr Oberbergrath Runge at Dortmund, in whose possession it now is.

ON August 15 the well-known Professor of Physics, Dr. Wilhelm Weber of Göttingen University, celebrated the day when, fifty years ago, he was called to that University from Halle. He is now seventy-seven years of age, and lectured until a few years ago.

A TERRIBLE catastrophe happened on August 16 at Remscheid (Rhenish Prussia). Suddenly the so-called Brennende Berg opened to the extent of some sixty to one hundred square metres and threw up gigantic flames. A house standing near sank into the burning gulf, and its inmates unfortunately perished. It is believed that the disaster was caused by the ignition of petroleum gas rising from a petroleum vein in the depth of the mountain.

Apropos of the forthcoming erection of a monument to Sauvage at Boulogne-sur-Mer, it may be mentioned that a rival claim to the invention of the screw propeller has been set up on behalf of a person named Dallery, also a Frenchman, whose granddaughter, it is alleged, has submitted certain evidence to the Académie des Sciences, showing that her grandfather, who died in 1835, took out a patent as long ago as 1813 for certain contrivances, including a screw propeller and a tubular boiler. M. de Lesseps is of opinion that although Dallery, like Pauchon, had long ago conceived the idea of the screw, yet it is to Sauvage that the credit is due of having been the first to apply it to practical purposes.

THE following candidates have been successful in obtaining Royal Exhibitions of 50*l*. per annum each for three years, and free admission to the course of instruction at the following institutions:—1. The Normal School of Science and Royal School of Mines, South Kensington and Jermyn Street, London—Thomas Mather, aged twenty-four, pattern maker, Manchester; Alfred Sutton, twenty-one, engine-fitter, Brighton; William H. Littleton, seventeen, student, Bristol. 2. The Royal College of Science, Dublin—Arthur Whitwell, nineteen, ex-pupil teacher, Nottingham; Frederick J. Willis, eighteen, student Bristol; Christopher J. Whittaker, twenty-one, pattern maker, Accrington.

MR. E. B. TYLOR requests us to mention that the portrait of Andaman Islanders in his "Anthropology," p. 88, which was reproduced in Mr. Wallace's review in NATURE, vol. xxiv. p. 242, is from one of the admirable series of photographs taken in 1872 by Dr. G. E. Dobson, now of the Army Medical School, Netley Hospital. By inadvertence, the cut in question was printed in the "Anthropology" without reference to Dr. Dobson. His paper "On the Andamans and Andamanese" in vol. iv. of the *Journal* of the Anthropological Institute, which gives an account of his visit to the natives in their forest-home, is illustrated with a set of three portrait-groups, which show perfectly their peculiar and homogeneous race-type.

MESSRS. CASSELL AND CO. have issued the first part of "an entirely new and revised edition" of Dr. Robert Brown's "Races of Mankind," under the title of "The Peoples of the World."

PHYLLOXERA has made its appearance in Hungarian vineyards. Its occurrence in the district of Szölös Urdo (Torda Comitat) has been officially stated. Also in the Swiss canton of Neufchâtel it is spreading to an alarming extent. The vineyards of Grand-Saconnex, Colombier, and La Coudre are fast succumbing to the plague.

M. SYNYROS, an Athens merchant, has recently given 100,000 francs for building a museum at Olympia.

THE construction of another great Alpine tunnel which should bring Paris and the North of France into more direct communication with Italy than is afforded by the existing tunnel through Mont Cenis, is under consideration with the French Government, the projects including not only one through Mont Blanc, but also through the Simplon or the Great St. Bernard. It is not likely, however, that the latter will meet with much encouragement. The tunnel under the Simplon would be 60,719 feet long, while that under Mont Blanc is only 44,292

feet. As connected with other Alpine tunnels, Mont Cenis is 40,093 feet, and St. Gothard, 48,952 feet. The Simplon would therefore be longest of all; but, on the other hand, it would be on a lower level than the others, the entrance at Brieg being only 2333 feet, and that at Iselle 2253 feet above the sea level. The entrances to the Mont Blanc tunnel would be 3345 feet at Mont Quart, and 4215 feet at Entrèves above the sea level. The Bardonneche entrance to Mont Cenis is 3970 feet, and that at Modane 3799 feet, while in the case of the St. Gothard tunnel the northern entrance at Göschenen is 3638 feet, and the southern, at Airolo, 3756 feet above the sea. Thus the Mont Cenis tunnel is shorter, but 330 feet higher than the Mont Blanc, while the Simplon would be about half as long again, but about 1000 feet lower. Supposing that the operations would be conducted at the same rate as they have been at St. Gothard, the boring will take 4218 days, or, working at both ends, 2109—nearly six years.

THE additions to the Zoological Society's Gardens during the past week include two Guinea Baboons (*Cynocephalus sphinx*), a Grivet Monkey (*Cercopithecus griseo-viridis*, var.) from West Africa, presented by Mr. Lionel Hart; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by the Rev. George Cuffe; two Arabian Gazelles (*Gazella arabica*), three Domestic Pigeons (*Columba œnas*) from Arabia, presented by Mr. Reginald Zohrab; two Common Squirrels (*Sciurus vulgaris*), British, presented by Lieut.-Col. F. D. Waters, 82nd Regiment; a Collared Peccary (*Dicotyles tajaçu*) from Guiana, presented by Capt. W. F. Wardroper; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. L. H. Haworth; a Cinereous Sea Eagle (*Haliaëtus albicilla*) from Norway, presented by Mr. James Ashbury; a Red and Blue Macaw (*Ara macao*) from South America, presented by Mrs. Supple; two Common Barn Owls (*Strix flammea*), British, presented by Mr. C. T. Foster; an Upland Goose (*Bernicla magellanica*) from South America, presented by Mr. A. Nesbitt; two Common Kestrels (*Tinnunculus alaudarius*), British, presented by Mr. J. Edwards: a Bonnet Monkey (*Macacus radiatus*) from India, a Common Marmoset (*Hapale jacchus*), from South-East Brazil, deposited; two European Scops Owls (*Scops giu*), European, purchased. Amongst the additions to the Insectarium during the same period are larvæ of the Common Butterfly (*Vanessa C. album*), Lobster Moth (*Stauropus fagi*), Pale Tussock Moth (*Orgyia pudibunda*)—the so-called Hop-Dog—*Dipthera orion*, *Halias prasinana*, and *Deilephila euphorbiæ* and *galii*; also a perfect insect of *Cholus forbesi*, being the third known example of this species, originally described from specimens captured in an orchid-house at Highgate. The present specimen was found, under similar conditions, by Dr. Wallace of Colchester.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 22.—M. Jamin in the chair.—The following papers were read:—Meridian observations of small planets and of Comet *b* 1881, at Paris Observatory, during the second quarter of 1881, by M. Mouchez.—Remarks on M. Jamin's note on comets, by M. Faye.—On spectrum analysis applied to comets, by the same.—On the nature of the repulsive force exercised by the sun, by the same. He associated it long ago with the state of incandescence of the sun; and, in an experiment, rare gaseous matter made luminous by means of an induction-spark was repelled by an incandescent plate at a sensible distance. Some thought this not decisive, however; for the gaseous matter might become more conductive through heating, so that the effect observed might be a sort of obscure discharge. M. Faye invites physicists to take up the matter afresh.—On the interior state of the terrestrial globe, by M. Roche. Supposing the globe formed of a nucleus or solid block nearly homogeneous, covered with a lighter layer, of density geologically shown to be about 3 in relation to water; he finds it possible to harmonise the general values of precession and flattening, if it be considered that the interior nucleus has solidified and taken its definitive form under influence of a rotation less rapid than that now animating the earth. The central block is probably like meteoric iron in specific gravity, while the enveloping layer is comparable to aëroliths of stony nature, with little iron.—On the irreducible co-variants of the binary quartic of the eighth order, by Prof. Sylvester.—On a new species of Cissus (*Cissus Rocheana*, Planch), from the interior of Sierra Leone, capable of bearing the winter of Marseilles, by M. Planchon. Its endurance is a matter of temperament, and a proof of the extent of the scale of resistance to cold and heat which some plants possess, and which often upsets all prevision. The American *Vitis riparia* lives sixty miles north of Quebec, and is also found in the sub-tropical Southern States.—On the laws of formation of cometary tails, by M. Schwedoff. Starting with the existence of an infinite number of ponderable particles in celestial space, he shows that those with parabolic orbits have most chance of collision and consequent heating and dispersion. The sudden vaporisation of solids, due to passage among them of a cometary nucleus, generates the cometary nebulosity. The velocity of propagation of visible waves accompanying the nucleus is equal to the velocity of the nucleus itself at the moment of departure of these waves. The maximum of intensity of a cosmic wave is found in the tangent to the orbit of the nucleus at the point of departure of the wave. With these two laws he seeks to explain the phenomena observed.—On a particular case of the theory of motion of an invariable solid in a resistant medium, by M. Willotte.—M. Trève communicated the results of some experiments as to the effects produced by shunts in telephonic circuits.—Solar observations at the Royal Observatory of the Roman College during the first quarter of 1881, by P. Tacchini. After the secondary minimum in the end of last year, the solar activity resumed its course towards the maximum. The distribution of protuberances, &c., was the same as in the last quarter of 1880.—Observations of solar spots and faculæ in April to July, 1881, by P. Tacchini. A minimum of spots occurred in May, and an exceptional maximum in July; now, the activity is anew at a minimum. During this year several periods of abundant frequency have recurred.—Spectroscopic studies on comets *b* and *c* 1881, by M. Thollon. Comet *c* seems to be almost wholly gaseous. The brightness of the head and tail of the comets seems to vary rapidly and uniformly with distance from the sun; arguing that their white light is almost wholly reflected sunlight. The slowness of variation of the band spectrum is against the view that the cometary elements are rendered incandescent by calorific action of the sun. The comets have probably a light and heat of their own.—Researches on the telluric lines of the solar spectrum, by M. Egoroff. Sending a strong electric beam through 18m. of aqueous vapour, and increasing the tension to 6 atm., the spectrum was notably changed in aspect. The group *a* in the extreme red he thinks fundamental for aqueous vapour, and he is going to examine it in detail.—On the existence of a new metallic element, actinium, in the zinc of commerce, by Mr. Phipson.—Note relative to a new series of phosphates and arseniates, by MM. Filhol and Senderens.—Fixation of hypochlorous acids on propargylic compounds, by M. Henry.—On the abnormal presence of uric acid in the salivary, gastric, nasal, pharyngeal, sudoral, and uterine secretions, and in menstrual blood; diagnostic and therapeutic indications, by M. Boucheron.—Observations during a thunderstorm on June 25, 1881, by M. Larroque.

CONTENTS

THURSDAY, SEPTEMBER 8, 1881

THE STUDENT'S DARWIN

The Student's Darwin. By Edward B. Aveling, D.Sc., Fellow of University College, London. International Library of Science and Freethought. Vol. II. (London: Annie Besant and Charles Bradlaugh, 1881.)

SEVERAL months ago we reviewed the first volume of this series, and now in reviewing the second we are still of opinion that the promoters of the series are mistaken, so far as they may have the interests of science at heart, in associating their endeavours to render science popular with their systematic onslaught against theistic belief. In itself science has no necessary relation to any such belief; it is neither theistic nor atheistic; it is simply extra-theistic. It is but an extension of common experience, and as such has to deal only with the facts of ordinary knowledge without at any point being able to escape from the sphere of the phenomenal; in so far as any inferences are extended from this domain they are not scientific but metaphysical. Therefore, although it may be of use in the interests of "Freethought" to represent science as not merely neutral but negative in its bearings upon religion, the attempt to do so is detrimental to the interests of science; so far as it may be successful it can only tend to increase the suspicious dislike of scientific knowledge which large masses of the general public are already too apt to harbour. Still, as the leading object of the "International Library" is no doubt that of advancing anti-theistic dogma, its promoters are probably careless whether in so doing they are either loyal or just to the cause of science, under whose banner and in whose name they profess to march.

But beyond recording our dissent from the unreasonable and, from our point of view, pernicious association of "Science" with "Freethought" which is being carried through the "International Library," we have nothing further to do with this matter; in these columns we have only to deal with the science, and so shall not again refer to the freethought, although it may be noted as a curious illustration of the contrast between "the solid ground of nature" and the quicksands of speculative thinking, that one of our most recent reviews was that of a book by Dr. Lauder Brunton, who is certainly no less an authority in science than Dr. Aveling, and whose whole object was seen to be the exact reverse of that which appears in "The Student's Darwin,"—viz., to show that Darwinism is *not* opposed to theism. For ourselves, it is needless to add, we hold that the theory of evolution resembles all other scientific theories in having no point of legitimate contact with any ulterior question of metaphysics, further than that of removing from metaphysics certain erroneous arguments previously based upon imperfect knowledge.

Dr. Aveling has been a diligent student of Mr. Darwin's books, and on reading his epitome of them, even in the most cursory way, one is more than ever amazed at the enormous fertility of Mr. Darwin's work. At every page one feels how meagre the epitome is—often little better than an index—and yet for more than 300 pages the index runs on showing as in a sketch what the mind of one man has accomplished, till the reader who is able to remember how many and minute are the details which the index embraces is glad to agree with an introductory remark of the writer, "It is well that all of us should know at least the outline of the work that has been done by this man. For as the name of Chaucer marks the fourteenth, and the name of Shakespeare the sixteenth century, so probably will the name of Charles Darwin mark the nineteenth century in the years to come."

The object of the "Student's Darwin" is thus, as its author says, to furnish a brief summary of the main results of Mr. Darwin's labours, and as the abstract has on the whole been well made, it ought to be found useful for any one who has not time to read for himself the originals. It would have been desirable to have gone less into mere description of species, and more fully into the theory of their origin; for no one who is likely to read the book will profit by the former, while the chief object of the "Student's Darwin" ought to be that of rendering a careful and complete abstract of Darwinism. Yet this is far from being the case in the book before us When, for instance, we have the arguments from Classification, Morphology, Development, and Rudimentary Organs all compressed into less than two pages, it is evident that the analysis is becoming much too scanty; and in fact no one depending for his information upon this analysis alone could form any just idea of the mass of evidence in favour of evolution and natural selection which Mr. Darwin has collected. This fault is the less pardonable, because it cannot be pleaded in excuse for it that the author is pressed for space, seeing that throughout the book he every here and there devotes a paragraph or two to bad attempts at "fine writing," which, besides being blemishes from a literary point of view, absorb a number of pages which might have been profitably devoted to a further exposition of what he properly terms "the *magnum opus.*"

Dr. Aveling, however, everywhere exhibits a just estimate of Mr. Darwin's powers, as a few quotations may suffice to show. "From these pages" (*i.e.* those of the Monograph of the Cirripedia) "the student will turn with renewed reverence for the great generaliser, who is so patient and so completely master of detail." "Preconceived notions are not for him. He states the arguments for the conclusions that would strengthen the position of the great theory of evolution only less clearly than he states those that tell against that theory. No man was ever more of judge than he; no man was ever less of advocate. . . . The obligations of Charles Darwin to other workers in the same field as himself are always paid with a cordiality and courtesy that must be as gratifying to them as they are natural to him." "Only thirty-four years and the man who has produced the new thoughts is still among us! To-day they form part of the accepted creed of scientific thinkers. . . . To those who remember how few of the great have beheld with their own patient eyes their own greatness in some faint degree recognised during their own lives, their own thoughts accepted as true guides by the thoughtful, assuredly there is cause for comfort here." "Looking back over them again" (*i.e.* the whole series of works), "we cannot fail to be impressed with those two large attributes of genius that are especially his—unrivalled powers of observation, unrivalled powers of generalisation. And the homage that we pay him

to-day is, I am assured, but the feeblest of utterances as compared with the heartfelt gratitude and wondering praise that will be the reward of this great thinker in those future times when the very lowliest in the land shall have full grasp of the meaning of his teaching," &c.

On the whole, the "Student's Darwin" deserves to be successful in its object of popularising Mr. Darwin's work. The great bar to its usefulness will be its needlessly aggressive tone towards religion, which is sure greatly to lessen a circulation which it might otherwise have had. GEORGE J. ROMANES

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Tebbutt's Comet—Origination of its Proper Light

WHILE there seems now no doubt that the honour of being the discoverer of the great comet of 1881 belongs without question to that life-long and most persevering observer, as well as successful computer, of comets, in Australia, Mr. John Tebbutt, three communications which chance to have arrived here this morning from different countries contain most diverse ideas of the nature of that portion of the comet's light which universal spectroscopic observation proves is inherent to the comet itself, indicating the existence there of carburetted gas of some kind, and is quite distinct from the concomitant weak reflection of solar light.

1. In Abbé and Chanoine Moigno's *Les Mondes* for August 25, that excellent physicist in Paris, M. Jamin, is represented as stating that the comet's carburetted gas could be rendered "properly" luminous only in two modes, viz., either by combustion or electric discharges. "If by combustion," says he, "how did it first take fire? what keeps up the fire perpetually? and how are the materials of the comet kept, in such a fire, from becoming red-hot, and then giving out quite a different spectrum to any that has yet been observed?" Wherefore he concludes that the cause of the "proper" light of the comet is the illumination of its constituent molecules by electric discharge, as in the gas-vacuum tubes of our laboratories.

2. But next comes a pamphlet from that accomplished spectroscopist and astronomer, Prof. C. A. Young of Princeton, New Jersey, U.S., setting forth that the bands of carburetted gas seen in the comet's spectrum do most admirably and exactly agree with the combustion-bands of coal-gas and air, as seen in a Bunsen-burner or a blowpipe flame, or in the blue base of all carbo hydrogen flames known; while they do, on the contrary, most eminently, markedly, and distinctly disagree from the bands of the spectra of the same gases as seen in gas vacuum-tubes when illumined by electric discharge. And this conclusion of the eminent American physicist is confirmed by a pamphlet just received from M. Fiéver, the spectroscopic observer of the re-organised Royal Observatory of Brussels; as was also announced at the very time of the comet's appearance by the present most acutely observing Astronomer-Royal at Greenwich.

3. What then! Is M. Jamin's theory of the comet's proper light being entirely due to electrical illumination utterly overthrown, and the celestial phenomenon given over to a process of combustion, the mere mention of the necessary details of which suffices to show it ridiculous and impossible?

4. Not yet, I venture to think. We ought to discriminate in such a case most carefully between electricities of different intensities and different temperatures. Something too of that kind, and even much to the purpose of this cometary case, I had the honour of setting forth to the Royal Society, Edinburgh, last year, in a paper which is now being printed for their *Transactions*. For it was shown therein that, when using an induction-coil capable of giving sparks of such intensity as to be five inches long in the open air, a gas vacuum-tube of olefiant gas showed only the carburetted bands which Prof. Young alludes to as being absolutely *not* the bands which the spectrum of the comet exhibited. But when a smaller coil was employed, and more particularly when its outer helix of long thin wire was replaced by another of short thick wire (specially prepared for the experiment), and the sparks thereby lowered in intensity to such a degree as from 1·3 of an inch, to be capable of only passing through 0·2 inch of air, then, when employed to illuminate the same olefiant gas vacuum-tube, besides the bands seen before (but now more faintly), another set of bands appeared, which were exactly those of the combustion of coal-gas and air, of Bunsen burners, blowpipe flames, blue base of all carbo-hydrogen flames, and finally—*teste* Prof. C. A. Young, M. Fiévez, the Astronomer-Royal, W. H. M. Christie, and others—of Tebbutt's great comet of 1881.

5. From this condensation of testimonies I presume that no other conclusion is to be drawn than that the electrical discharges permeating the whole length of a comet's tail must be something exceedingly weak in intensity;—and the gentlemen who employ electrically lit-up gas vacuum-tubes in their laboratories must do their spiriting with them in future much more gently, if they would really arrive at what goes on in cometary existences. The following exception, too, duly mentioned by Prof. Young, to his general rule, seems to tend in the same direction. For he states "that while the evidence as to the identity of the flame and comet spectra is almost overwhelming, the peculiar ill-defined appearance of the cometary bands at the time of the comet's greatest brightness is, however, something which he has not yet succeeded in imitating with the flame spectrum."

6. "Certainly not," we may add to this most honest confession; for as the comet's greater brightness near its perihelion passage could hardly be due to anything else than a temporary increase in the intensity of its illuminating electric currents, that would tend to bring out the *tube-set* of carburetted bands to interfere with, and spoil the neatness and sharpness of, the so-called *flame*-bands, and would certainly imply a quality or temperature which does not exist in any known simple flame, but is found in the spark of even the smallest induction coil, unless some special means are taken to damp down its intensity.

I have long wished at this Observatory to try a whole course of electric illuminations, as of the old friction machine, Holtz's machine, modern dynamo-machine, coils in variety, and whatever is capable of giving out electricity in any visible luminous shape; but the state of miserable starvation in which this Royal Observatory, Edinburgh, is kept throughout all its branches by Government, and their continued neglect of the applications of their own "Board of Visitors" to "endeavour to obtain justice to this Observatory"—the very words of the last public remit from the Board-meeting, of which the venerable Duncan McLaren, then M.P. for Edinburgh, was chairman—prevent any important apparatus being purchased, or even obtained on loan, to prosecute the inquiries which the science of the times demands. PIAZZI SMYTH,
Astronomer-Royal for Scotland

Royal Observatory, Edinburgh, August 29

Schaeberle's Comet

SINCE my last remarks I have had an opportunity to examine this fine object with the 6" Cooke equatorial. On Wednesday evening, the 24th, simultaneously with the Great Bear stars, it was easily seen as soon as twilight set in, near the horizon and considerably more to the west than on the 21st. With a comet eyepiece it presented, in spite of its low altitude, a sharp and well-defined figure. The nucleus was stellar-like, with, I thought, a still brighter minute central point. No jets of light proceeded directly from it, but it appeared surrounded by a circular nebulosity of greater extent than the base of the tail, and giving the headed form to the comet frequently seen in old drawings of these objects. The tail was straight, long, and luminous, with a central ray of condensed light which gave it a cylindrical look. When first examined three small stars were involved in the tail without any apparent diminishing of their brightness; while two others below served to define the limit of the tail's visibility in the comet eyepiece. This measured two degrees only, but both it and the nucleus were of a peculiarly fine pale blue tint. I send a drawing of the telescopic appearance of the comet at 8h. 40m. On the nights of the 27th and 28th the comet was again examined at about 8h. 30m. Under a lower power Kellner the appearance was that of a round comet with a central

nucleus and circular coma. The tail was to be seen, but was quite faint, and as before was less at the base than the width of

Sketch of Schaeberle's Comet, August 24, 8h. 40m. in 6" Cooke equatorial.

the coma. Dr. de Konkoly I see has examined the spectrum of this comet, and found it a faint continuous one, with three tolerably bright lines, at following positions:—

	Estimated brightness.
I. 5601 ± 2·5	0·4
II. 5161 ± 0·9	1·0
III. 4753 ± 0·6	0·8

Schaeberle's Comet, August 28, 8h. 32m.

The appearance of this comet throughout has been peculiarly distinguished from that class in which jets of light streaming from the nucleus in front fall back to form the tail or a bright margin to it. These, as far as I have seen, have been absent.

Guildown, August 31 J. RAND CAPRON

Comet *b* 1881

M. CH. FIEVEZ, the Astronomer adjoint at the Royal Observatory, Brussels, has been good enough to send me a copy of his note on the analysis of the light of this comet, made with the 15-inch Merz-Cooke equatorial, provisionally installed at the Avenue Cortenberg. The polariscopic observations demonstrate that the polarisation of the nucleus was strong (très nette et bien accentuée), while that of the tail was very weak. These observations were made at several days interval, from 11h. till midnight. Sky polarisation was scarcely sensible. The spectroscopic observations proved the spectrum of the comet to consist of four bands of intensity in the following order: green, blue, violet, and yellow, with wave-lengths 5160, 4780, 4200 (about), and 5620. The original appearance of these bands was modified as the comet receded from the sun, their edges towards the red then becoming more and more defined. The nucleus presented a brilliant continuous spectrum, in which however the Fraunhofer lines were not recognised. The conclusions arrived at by M. Fievez were as follows:—That a great part of the light of the comet was inherent to it, while the other part was reflected solar light. That the strong polarisation of the nucleus indicated a marked state of condensation of the matter composing it. That the spectrum differed little from that of other comets. Lastly, that the marked modifications in the brilliancy of the continuous spectrum, and in the appearance of the spectrum bands indicated a progressive diminution in the comet's temperature. The chief interest in the above observations attaches to the feeble polarisation detected in the tail as compared with that found by Prof. A. W. Wright and Mr. Cowper Ranyard, and in the absence of the Fraunhofer lines, which were measured by Dr. N. de Konkoly, and also photographed by Dr. Huggins. Whence, we may ask, arises the divergence of conclusions arrived at by M. Fievez and Prof. Wright respectively, the one considering that the principal part of the comet's light is from itself, the other that it is reflected sunlight, and why were the Fraunhofer lines seen in the one case, and not in the other? The answer lies, I think, not with the instruments employed, but rather in the interesting probability of change in the comet's structure or condition during the time of its examination. A comparison of the many observations recorded during its stay with us may possibly lead to important discoveries in this direction. I am much interested to see that Prof. C. A. Young informs us that the green band was seen by observers at Princeton split up into fine sharp lines coinciding with those seen in the flame spectrum, a result to be expected, but hitherto not attained. J. RAND CAPRON

Guildown, September 3

THE comet at present visible was examined by me with the spectroscope on the 8¼-inch refractor on Saturday evening, August 27. The three principal hydrocarbon bands were plainly visible, the central one being the brightest, and on comparing them with the spectrum of a spirit-lamp flame the coincidence of the least refrangible sides of the bands in the two spectra was sensibly complete. The nucleus gave a narrow continuous spectrum, and I could see no trace of such a spectrum except from that point. I could see no other band in the spectrum except the three above mentioned, but the proximity of the comet to the horizon may have something to do with this.

GEORGE M. SEABROKE

Temple Observatory, Rugby, August 29

A Pink Rainbow

I SPENT Sunday, August 21, at Mr. Tennyson's house, Aldworth, near Haslemere. The house stands on an elevated ledge of the Blackdown range, looking over the Weald towards the Brighton Downs, between east and south-east. About sunset the deep red of the south-eastern sky attracted our attention, and while we were looking at it we saw stretching across it a well-marked rainbow, but of a uniform red or pink colour, which Mrs. Tennyson compares, in a note I have just had from her, to a "pink postage-stamp"—not the one now in use, but the last discarded one. This was seen distinctly by Mrs. Tennyson, Mr. Hallam Tennyson, and myself for, I think, more than a minute. Mr. Hallam went to call his father, who was in another room,

but before he came, "the bow," to quote Mr. Tennyson's words, "had assumed its usual colours, which were, however, very faint." Mrs. Tennyson says the pink colour "was visible for a very little time just at sunset, and then I saw a dull olive green at the lower edge." After that, as Mr. Tennyson says, we all saw the vanishing ghost, as it were, of an ordinary rainbow. The actual uniform redness came just at sunset, as marked in the almanac we consulted—ten minutes past seven. A. M.

August 26

The Glacial Period

PLEASE correct an error in the notice of my paper on the Glacial Period (NATURE, vol. xxiv. p. 364). It is on the western slope of New Zealand that the glaciers reach to the highest mean annual temperature (10° C., or 50° F.) as well as to the lowest level. *Apropos* of my studies on this subject, I should be very glad to meet some of the British glacialists at Venice, at the third International Geographical Congress, and discuss some points of interest with them. As there is, a few days later, an International Geological Congress at Bologna, it will be the easier for geologists to make a short stay at Venice before. The Geographical Congress begins on September 15.

St. Petersburg, August 13-25 A. WOEIKOF

THE BRITISH ASSOCIATION

THE Jubilee Meeting of the British Association has come to a close, and whether we take the test of work done, or of the numbers present as members or associates, it must be admitted that it has been a great success. While in 1879, in the densely populated town of Sheffield, the total was 1404, and at Swansea last year 915, the number has risen this year to 2533, which includes 22 foreign members, 510 ladies, and 1173 associates. Of course York does not supply the whole of the latter: many come from Leeds, Sheffield, and Scarborough, and the surrounding towns. Seven times previously has the number been greater; the maximum (3335) having been attained at Newcastle-on-Tyne in 1863. As regards work done, it may be mentioned that on Friday nearly a hundred papers were announced for reading in the various sections. One of the laws of the Jewish jubilee festival was that the land should remain untilled for a year; but we have reversed this, and only cultivated our scientific soil the more. Sir David Brewster, in the original letter which laid the foundations of the society, suggested York as the most central city of the three kingdoms, but he first inquired "if York will furnish the accommodation necessary for so large a meeting, which might perhaps consist of 100 individuals." Apparently therefore he did not contemplate the admission of associates, or the use of the Association as a means of scattering broadcast the results of the scientific year, but rather regarded it as a means whereby the cultivators of science might become better acquainted with each other at a time when communication with London was far more difficult, and intercourse through scientific publications far more restricted than now. But the first meeting numbered 350 members, and included some of the most representative men of science of the day. On this occasion the presidential address lasted five minutes.

The proceedings commenced on Wednesday, August 31, by the reading of the Report of the Council, in which it was announced that Mr. P. L. Sclater had resigned the office of general secretary, and that he would be succeeded by Mr. F. M. Balfour of Cambridge. Mr. G. E. Gordon has also retired from the assistant secretaryship, and is to be succeeded by Prof. Bonney, with the title of secretary and a salary of 300*l.* per annum, with 25*l.* for travelling expenses. Mr. Spottiswoode succeeds Sir Philip de Malpas Grey Egerton as trustee.

The new members of council are Messrs. Warren De La Rue, A. Vernon Harcourt, G. W. Hastings, J. C. Hawkshaw, and G. Prestwich.

Sir John Lubbock's address was listened to by a very crowded audience. The Exhibition Hall is a fine building, and was prettily decorated, but its acoustic properties are somewhat deficient, and the unsteady electric light was painful to the eyes. The address occupies fifty octavo pages, of which nearly twenty were omitted during delivery. On the subject of education the President expressed himself strongly; he asked that more time should be given to French, German, science, and mathematics. "What we ask is that, say, six hours a week each should be devoted to mathematics, modern languages, and science, an arrangement which would still leave twenty hours for Latin and Greek"; and he added, "we cannot but consider that our present system of education is, in the words of the Duke of Devonshire's Commission, little less than a national misfortune."

Sir John Lubbock adopted a judicious mean between the address devoted entirely to one subject on the one hand, and giving a general *résumé* of the progress of all the sciences on the other; for while he spoke in detail and authoritatively concerning the biological sciences, he also furnished accounts of the progress of the physical sciences, prepared by men well competent to discuss them.

The Section work began in earnest on Thursday morning. Some idea of the number of representative men who were present at the meeting may be gathered from the fact that in Section A there are ten vice-presidents and fifty-seven members of committee, and these numbers are exceeded in some of the sections; so that there are more than fifty vice-presidents of sections, and more than three hundred members of sectional committees. The sections were housed in capacious and very suitable rooms, and the attendance was very good.

The loan collection of scientific apparatus, although it contained some very interesting examples, was by no means a collection which represents the experimental progress of the last fifty years, and the appeal for historical apparatus has scarcely been responded to. The exhibition was shown at the Thursday *soirée*, and remained open till the end of the week of meeting. A good catalogue of thirty-two pages was prepared. We may particularly notice some beautifully-finished telescopes and transit instruments, and an electric chronograph exhibited by Messrs. T. Cooke and Sons; a model of the Vienna 27-inch refractor and its dome by Mr. Howard Grubb; and a very old telescope constructed by Abraham Sharp. The Manchester Literary and Philosophical Society exhibited some of the apparatus used by John Dalton in his researches; and the Science and Art Department sent astrolabes and sun-dials of the sixteenth, seventeenth, and eighteenth centuries. A few instruments were sent by foreign makers. Dr. Stone exhibited a large syren fitted with a key-board and worked at an uniform rate by clockwork. A quantity of physiological apparatus was exhibited by Dr. Burdon-Sanderson and Mr. G. B. Thistleton. Mr. Francis Galton exhibited and explained his composite photographic process, "a method of superposing the images of separate portraits and thence creating a face, the sum of all the components employed; it has a curious air of individuality about it, but is a perfectly ideal face, like all, but exactly resembling none." Dr. Tempest Anderson, one of the local secretaries, exhibited some ophthalmic appliances. The North-Eastern Railway Company exhibited an interesting meteorite which fell on March 14 last between the Middlesbrough and Ormesby stations of the Guisborough line. It is of the stony tufaceous type, and weighs three and a half pounds.

On Friday afternoon several manufactories were visited, also the gas-works and water-works. Messrs. Cooke's works were of especial interest, particularly the processes connected with the grinding of lenses and the graduation of circles by means of a large dividing-engine, the great circle of which is marked with divisions, each of which is

equal to five minutes of arc. Saturday afternoon was as usual devoted to excursions, but the steady downpour of rain did much to mar the enjoyment. Several people in the vicinity of York have entertained the members very hospitably, and have thrown open their houses. On Monday the usual meteorological breakfast took place; forty persons were present, and meteorology was the chief order of the day in Section A. In the evening Mr. Spottiswoode gave a discourse on "The Electric Discharge." The Red Lion Club met on Tuesday before the *soirée*.

Southampton has been chosen as the place of meeting in 1882, and Dr. C. W. Siemens has been elected president. A vigorous contest for the honour of receiving the Association took place yesterday between six towns:—Leicester, Nottingham, Southport, Oxford, Birmingham, and Aberdeen. The claims of each town were stated by delegates, and afterwards votes were taken by a show of hands. Birmingham withdrew. The President of the Royal Society, Sir Joseph Hooker, and Professors Acland, H. J. S. Smith, and Prestwich, strongly advocated the claims of Oxford, and the show of hands was declared to be in its favour. Southport was second on the list. Worcester has lodged a claim for 1884.

Altogether more than three hundred papers or reports have been read.

Eighteen papers were put on the list of Section A for Tuesday; twenty-eight in the Geological Section, thirteen in that of Anthropology, and fifteen in Mechanical Science. Thus the work has never flagged at all.

At the Committee Meeting on Wednesday Capt. Bedford Pim gave notice of motion that the meeting be held in Canada in 1885.

The following grants have been made:—

	£
The Council—Exploration of Mountain District of Eastern Equatorial Africa	100
A—Mathematics and Physics	
Mr. G. H. Darwin—Lunar Disturbance of Gravity	15
Dr. A. Schuster—Meteoric Dust	20
Prof. Sylvester—Fundamental Invariants (partly renewed)	80
Mr. R. H. Scott—Synoptic Charts of the Indian Ocean	50
Prof. G. C. Foster—Standards for Use in Electrical Measurements (partly renewed)	100
B—Chemistry	
Prof. Dewar—Present State of Knowledge of Spectrum Analysis	5
Prof. Balfour Stewart—Calibration of Mercurial Thermometers	20
Prof. Roscoe—Wave-lengths Tables of Spectra of Elements	50
Dr. Hugo Müller—Chemical Nomenclature	10
Prof. Odling—Photographing the Ultra-Violet Spark Spectra	25
C—Geology	
Dr. J. Evans—Record of the Progress of Geology	100
Prof. Ramsay—Earthquake Phenomena of Japan	25
Dr. H. C. Sorby—Conditions of Conversion of Sedimentary Materials into Metamorphic Rocks	10
Prof. W. C. Williamson—Fossil Plants of Halifax	15
Dr. Sorby—Conversion of Sediments into Metamorphic Rocks	10
Prof. A. C. Ramsay—Geological Map of Europe	25
Prof. E. Hull—Circulation of Underground Waters	15
Prof. W. C. Williamson—Tertiary Flora associated with the Basalts of the North of Ireland	20
Dr. Sorby—British Fossil Polyzoa	10
Prof. Leith-Adams—Carboniferous Limestone Caves in South Ireland	10
Prof. Green—Exploration of Raygill Fissure	20
D—Biology	
Mr. F. M. Balfour—Table at the Zoological Stations at Naples	80
Dr. Burdon-Sanderson—Albuminoid Substances of Serum	10
Dr. Pye Smith—Influence of Bodily Exercise on the Elimination of Nitrogen	50
Dr. M. Foster—Zoological Station in Scotland	£40
Mr. J. Cordeaux—Migration of Birds	15
Lieut.-Col. Godwin-Austen—Natural History of Socotra	100
Mr. Staniton—Record of Zoological Literature	100
Mr. Sclater—Natural History of Timorlaut	100
Prof. Flower—Photographs of Typical Races	10
Statistics	
Mr. F. Galton—Anthropometrics	50

SECTION A

MATHEMATICAL AND PHYSICAL

OPENING ADDRESS BY SIR WILLIAM THOMSON, F.R.S., PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY OF GLASGOW, PRESIDENT OF THE SECTION

On the Sources of Energy in Nature Available to Man for the Production of Mechanical Effect

DURING the fifty years' life of the British Association, the Advancement of Science for which it has lived and worked so well has not been more marked in any department than in one which belongs very decidedly to the Mathematical and Physical Section—the science of Energy. The very name energy, though first used in its present sense by Dr. Thomas Young about the beginning of this century, has only come into use practically after the doctrine which defines it had, during the first half of the British Association's life, been raised from a mere formula of mathematical dynamics to the position it now holds of a principle pervading all nature and guiding the investigator in every field of science.

A little article communicated to the Royal Society of Edinburgh a short time before the commencement of the epoch of energy under the title "On the Sources Available to Man for the Production of Mechanical Effect"[1] contained the following:—

"Men can obtain mechanical effect for their own purposes by working mechanically themselves, and directing other animals to work for them, or by using natural heat, the gravitation of descending solid masses, the natural motions of water and air, and the heat, or galvanic currents, or other mechanical effects produced by chemical combination, but in no other way at present known. Hence the stores from which mechanical effect may be drawn by man belong to one or other of the following classes:—

"I. The food of animals.

"II. Natural heat.

"III. Solid matter found in elevated positions.

"IV. The natural motions of water and air.

"V. Natural combustibles (as wood, coal, coal-gas, oils, marsh-gas, diamond, native sulphur, native metals, meteoric iron).

"VI. Artificial combustibles (as smelted or electrically-deposited metals, hydrogen, phosphorus).

"In the present communication, known facts in natural history and physical science, with reference to the sources from which these stores have derived their mechanical energies, are adduced to establish the following general conclusions:—

"1. *Heat radiated from the sun* (sunlight being included in this term) *is the principal source of mechanical effect available to man*.[2] From it is derived the whole mechanical effect obtained by means of animals working, water-wheels worked by rivers, steam-engines, galvanic engines, windmills, and the sails of ships.

"2. The motions of the earth, moon, and sun, and their mutual attractions, constitute an important source of available mechanical effect. From them all, but chiefly no doubt from the earth's motion of rotation, is derived the mechanical effect of water-wheels driven by the tides.

"3. The other known sources of mechanical effect available to man are either terrestrial—that is, belonging to the earth, and available without the influence of any external body—or meteoric—that is, belonging to bodies deposited on the earth from external space. Terrestrial sources, including mountain quarries and mines, the heat of hot springs, and the combustion of native sulphur, perhaps also the combustion of inorganic native combustibles, are actually used, but the mechanical effect

[1] Read at the Royal Society of Edinburgh on February 2, 1852 (*Proceedings* of that date).

[2] A general conclusion equivalent to this was published by Sir John Herschel in 1833. See his "Astronomy," edit. 1849, § (399).

obtained from them is very inconsiderable, compared with that which is obtained from sources belonging to the two classes mentioned above. Meteoric sources, including only the heat of newly-fallen meteoric bodies, and the combustion of meteoric iron, need not be reckoned among those available to man for practical purposes."

Thus we may summarise the natural sources of energy as Tides, Food, Fuel, Wind, and Rain.

Among the practical sources of energy thus exhaustively enumerated, there is only one not derived from sun-heat—that is the tides. Consider it first. I have called it *practical*, because tide-mills exist. But the places where they can work usefully are very rare, and the whole amount of work actually done by them is a drop to the ocean of work done by other motors. A tide of two metres' rise and fall, if we imagine it utilised to the utmost by means of ideal water-wheels doing with perfect economy the whole work of filling and emptying a dock-basin in infinitely short times at the moments of high and low water, would give just one metre-ton per square metre of area. This work done four times in the twenty-four hours amounts to 1-1620th of the work of a horse power. Parenthetically, in explanation, I may say that the French metrical equivalent (to which in all scientific and practical measurements we are irresistibly drawn, notwithstanding a dense barrier of insular prejudice most detrimental to the islanders),—the French metrical equivalent of James Watt's "horse-power" of 550 foot-pounds per second, or 33,000 foot-pounds per minute, or nearly two million foot-pounds per hour, is 75 metre-kilogrammes per second, or 4½ metre-tons per minute, or 270 metre-tons per hour. The French ton of 1000 kilogrammes used in this reckoning is 0·984 of the British ton.

Returning to the question of utilising tidal energy, we find a dock area of 162,000 square metres (which is a little more than 400 metres square) required for 100 horse-power. This, considering the vast costliness of dock construction, is obviously prohibitory of every scheme for economising tidal energy by means of artificial dock-basins, however near to the ideal perfection might be the realised tide-mill, and however convenient and non-wasteful the accumulator—whether Faure's electric accumulator, or other accumulators of energy hitherto invented or to be invented—which might be used to store up the energy yielded by the tide-mill during its short harvests about the times of high and low water, and to give it out when wanted at other times of the six hours. There may however be a dozen places possible in the world where it could be advantageous to build a sea wall across the mouth of a natural basin or estuary, and to utilise the tidal energy of filling it and emptying it by means of sluices and water-wheels. But if *so* much could be done, it would in many cases take only a little more to keep the water out altogether, and make fertile land of the whole basin. Thus we are led up to the interesting economical question, whether is forty acres (the British *agricultural* measure for the area of 162,000 square metres) or 100 horse-power more valuable. The annual cost of 100 horse-power night and day, for 365 days of the year, obtained through steam from coals, may be about ten times the rental of forty acres at 2*l.* or 3*l.* per acre. But the value of land is essentially much more than its rental, and the rental of land is apt to be much more than 2*l.* or 3*l.* per acre in places where 100 horse-power could be taken with advantage from coal through steam. Thus the question remains unsolved, with the possibility that in one place the answer may be *one hundred horse-power*, and in another *forty acres*. But, indeed, the question is hardly worth answering, considering the rarity of the cases, if they exist at all, where embankments for the utilisation of tidal energy are practicable.

Turning now to sources of energy derived from sun-heat, let us take the wind first. When we look at the register of British shipping and see 40,000 vessels, of which about 10,000 are steamers and 30,000 sailing ships, and when we think how vast an absolute amount of horse-power is developed by the engines of those steamers, and how considerable a proportion it forms of the whole horse-power taken from coal annually in the whole world at the present time, and when we consider the sailing ships of other nations, which must be reckoned in the account, and throw in the little item of windmills, we find that, even in the present days of steam ascendency, old-fashioned Wind still supplies a large part of all the energy used by man. But however much we may regret the time when Hood's young lady, visiting the fens of Lincolnshire at Christmas, and writing to her dearest friend in London (both sixty years old now if they are alive), describes the delight of sitting in a bower and looking over the wintry plain, not desolate, because "windmills lend revolving animation to the scene," we cannot shut our eyes to the fact of a lamentable decadence of wind-power. Is this decadence permanent, or may we hope that it is only temporary? The subterranean coal-stores of the world are becoming exhausted surely, and not slowly, and the price of coal is upward bound—upward bound on the whole, though no doubt it will have its ups and downs in the future as it has had in the past, and as must be the case in respect to every marketable commodity. When the coal is all burned; or, long before it is all burned, when there is so little of it left and the coal-mines from which that little is to be excavated are so distant and deep and hot that its price to the consumer is greatly higher than at present, it is most probable that windmills or wind-motors in some form will again be in the ascendant, and that wind will do man's mechanical work on land at least in proportion comparable to its present doing of work at sea.

Even now it is not utterly chimerical to think of wind superseding coal in some places for a very important part of its present duty—that of giving light. Indeed, now that we have dynamos and Faure's accumulator, the little want to let the thing be done is cheap windmills. A Faure cell containing 20 kilogrammes of lead and minium charged and employed to excite incandescent vacuum-lamps has a light-giving capacity of 60-candle hours (I have found considerably more in experiments made by myself; but I take 60 as a safe estimate). The charging may be done uninjuriously, and with good dynamical economy in any time from six hours to twelve or more. The drawing-off of the charge for use may be done safely, but somewhat wastefully, in two hours, and very economically in any time of from five hours to a week or more. Calms do not last often longer than three or four days at a time. Suppose then that a five days storage-capacity suffices (there may be a little steam-engine ready to be set to work at any time after a four-days' calm, or the user of the light may have a few candles or oil-lamps in reserve, and be satisfied with them when the wind fails for more than five days). One of the twenty kilogramme cells charged when the windmill works for five or six hours at any time, and left with its 60 candle-hours' capacity to be used six hours a day for five days, gives a 2-candle light. Thus thirty-two such accumulator cells so used would give as much light as four burners of London 16-candle gas. The probable cost of dynamo and accumulator does not seem fatal to the plan, if the windmill could be had for something comparable with the prime cost of a steam-engine capable of working at the same horse-power as the windmill when in good action. But windmills as hitherto made are very costly machines, and it does not seem probable that, without inventions not yet made, wind can be economically used to give light in any considerable class of cases, or to put energy into store for work of other kinds.

Consider, lastly, rain-power. When it is to be had in places where power is wanted for mills or factories of any kind, water-power is thoroughly appreciated. From time immemorial, water-motors have been made in large variety for utilising rain-power in the various conditions in which it is presented, whether in rapidly-flowing rivers, in natural waterfalls, or stored at heights in natural lakes or artificial reservoirs. Improvements and fresh inventions of machines of this class still go on, and some of the finest principles of mathematical hydrodynamics have, in the lifetime of the British Association, and, to a considerable degree, with its assistance, been put in requisition for perfecting the theory of hydraulic mechanism and extending its practical applications.

A first question occurs: Are we necessarily limited to such natural sources of water-power as are supplied by rain falling on hill-country, or may we look to the collection of rain-water in tanks placed artificially at sufficient heights over flat country to supply motive power economically by driving water-wheels? To answer it: Suppose a height of 100 metres, which is very large for any practicable building, or for columns erected to support tanks; and suppose the annual rainfall to be three-quarters of a metre (30 inches). The annual yield of energy would be 75 metre-tons per square metre of the tank. Now one horse-power for 365 times 24 hours is 236,500 foot-tons; and therefore (dividing this by 75) we find 3153 square metres as the area of our supposed tank required for a continuous supply of one horse-power. The prime cost of any such structure, not to speak of the value of the land which it would cover, is utterly prohibitory of any such plan for utilising the motive power of

rain. We may or may not look forward hopefully to the time when windmills will again "lend revolving animation" to a dull flat country; but we certainly need not be afraid that the scene will be marred by forests of iron columns taking the place of natural trees, and gigantic tanks overshadowing the fields and blackening the horizon.

To use rain-power economically on any considerable scale we must look to the natural drainage of hill country and take the water where we find it either actually falling or stored up and ready to fall when a short artificial channel or pipe can be provided for it at moderate cost. The expense of aqueducts, or of underground water-pipes, to carry water to any great distance—any distance of more than a few miles or a few hundred yards—is much too great for economy when the yield to be provided for is *power;* and such works can only be undertaken when the *water itself* is what is wanted. Incidentally, in connection with the water supply of towns, some part of the energy due to the head at which it is supplied may be used for power. There are however but few cases (I know of none except Greenock) in which the energy to spare over and above that devoted to bringing the water to where it is wanted, and causing it to flow fast enough for convenience at every opened tap in every house or factory, is enough to make it worth while to make arrangements for letting the water-power be used without wasting the water-substance. The cases in which water-power is taken from a town supply are generally very small, such as working the bellows of an organ, or "hair-brushing by machinery," and involve simply throwing away the used water. The cost of energy thus obtained must be something enormous in proportion to the actual quantity of the energy, and it is only the smallness of the quantity that allows the convenience of having it when wanted at any moment, to be so dearly bought.

For anything of great work by rain-power, the water-wheels must be in the very place where the water supply with natural fall is found. Such places are generally far from great towns, and the time is not yet come when great towns grow by natural selection beside waterfalls, for power; as they grow beside navigable rivers, for shipping. Thus hitherto the use of water-power has been confined chiefly to isolated factories which can be conveniently placed and economically worked in the neighbourhood of natural waterfalls. But the splendid suggestion made about three years ago by Mr. Siemens in his presidential address to the Institution of Mechanical Engineers, that the power of Niagara might be utilised, by transmitting it electrically to great distances, has given quite a fresh departure for design in respect to economy of rain-power. From the time of Joule's experimental electromagnetic engines developing 90 per cent. of the energy of a Voltaic battery in the form of weights raised, and the theory of the electromagnetic transmission of energy completed thirty years ago on the foundation afforded by the train of experimental and theoretical investigations by which he established his dynamical equivalent of heat in mechanical, electric, electro chemical, chemical, electro-magnetic, and thermo-elastic phenomena, it had been known that potential energy from any available source can be transmitted electromagnetically by means of an electric current through a wire, and directed to raise weights at a distance, with unlimitedly perfect economy. The first large-scale practical application of electro-magnetic machines was proposed by Holmes in 1854, to produce the electric light for lighthouses, and persevered in by him till he proved the availability of his machine to the satisfaction of the Trinity House and the delight of Faraday in trials at Blackwall in April, 1857, and it was applied to light the South Foreland lighthouse on December 8, 1858. This gave the impulse to invention; by which the electro-magnetic machine has been brought from the physical laboratory into the province of engineering, and has sent back to the realm of pure science a beautiful discovery, that of the fundamental principle of the dynamo, made triply and independently, and as nearly as may be simultaneously in 1867 by Dr. Werner Siemens, Mr. S. A. Varley, and Sir Charles Wheatstone; a discovery which constitutes an electro-magnetic analogue to the fundamental electrostatic principle of Nicholson's revolving doubler, resuscitated by Mr. C. F. Varley in his instrument "for generating electricity" patented in 1860; and by Holtz in his celebrated electric machine; and by myself in my "replenisher" for multiplying and maintaining charges in Leyden jars for heterostatic electrometers, and in the electrifier for the siphon of my recorder for submarine cables.

The dynamos of Gramme and Siemens, invented and made in the course of these fourteen years since the discovery of the fundamental principle, give now a ready means of realising economically on a large scale for many important practical applications, the old thermo-dynamics of Joule in electro-magnetism; and, what particularly concerns us now in connection with my present subject, they make it possible to transmit electro-magnetically the work of waterfalls through long insulated conducting wires, and use it at distances of fifties or hundreds of miles from the source, with excellent economy—better economy, indeed, in respect to proportion of energy used to energy dissipated than almost anything known in ordinary mechanics and hydraulics for distances of hundreds of yards instead of hundreds of miles.

In answer to questions put to me in May, 1879,[1] by the Parliamentary Committee on Electric Lighting, I gave a formula for calculating the amount of energy transmitted, and the amount dissipated by being converted into heat on the way, through an insulated copper conductor of any length, with any given electromotive force applied to produce the current. Taking Niagara as example, and with the idea of bringing its energy usefully to Montreal, Boston, New York, and Philadelphia, I calculated the formula for a distance of 300 British statute miles (which is greater than the distance of any of those four cities from Niagara, and is the radius of a circle covering a large and very important part of the United States and British North America), I found almost to my surprise that even with so great a distance to be provided for, the conditions are thoroughly practicable with good economy, all aspects of the case carefully considered. The formula itself will be the subject of a technical communication to Section A in the course of the Meeting on which we are now entering. I therefore at present restrict myself to a slight statement of results.

1. Apply dynamos driven by Niagara to produce a difference of potential of 80,000 volts between a good earth connection and the near end of a solid copper wire of half an inch (1·27 centimetres) diameter, and 300 statute miles (483 kilometres) length.

2. Let resistance by driven dynamos doing work, or by electric lights, or, as I can now say, by a Faure battery taking in a charge, be applied to keep the remote end at a potential differing by 64,000 volts from a good earth-plate there.

3. The result will be a current of 240 webers through the wire taking energy from the Niagara end at the rate of 26,250 horse-power, losing 5250 (or 20 per cent.) of this by the generation and dissipation of heat through the conductor and 21,000 horse-power (or 80 per cent. of the whole) on the recipients at the far end.

4. The elevation of temperature above the surrounding atmosphere, to allow the heat generated in it to escape by radiation and be carried away by convection is only about 20° Centigrade; the wire being hung freely exposed to air like an ordinary telegraph wire supported on posts.

5. The striking distance between flat metallic surfaces with difference of potentials of 80,000 volts (or 5,000 Daniell's) is (Thomson's "Electrostatics and Magnetism," § 340) only 18 millimetres, and therefore there is no difficulty about the insulation.

6. The cost of the copper wire, reckoned at 8*d.* per lb., is 37,000*l.*; the interest on which at 5 per cent. is 1900*l.* a year. If 5250 horse-power at the Niagara end costs more than 1900*l.* a year, it would be better economy to put more copper into the conductor; if less, less. I say no more on this point at present, as the economy of copper for electric conduction will be the subject of a special communication to the Section.

I shall only say in conclusion that one great difficulty in the way of economising the electrical transmitting power to great distances (or even to moderate distances of a few kilometres) is now overcome by Faure's splendid invention. High potential, as Siemens, I believe, first pointed out, is the essential for good dynamical economy in the electric transmission of power. But what are we to do with 80,000 volts when we have them at the civilised end of the wire? Imagine a domestic servant going to dust an electric lamp with 80,000 volts on one of its metals! Nothing above 200 volts ought on any account ever to be admitted into a house or ship or other place where safeguards against accident cannot be made absolutely and for ever trustworthy against all possibility of accident. In an electric workshop 80,000 volts is no more dangerous than a circular saw.

[1] Printed in the Parliamentary Blue Book Report of the Committee on Electric Lighting, 1879.

Till I learned Faure's invention I could but think of step-down dynamos, at a main receiving-station, to take energy direct from the electric main with its 80,000 volts, and supply it by secondary 200 volt dynamos or 100 volt dynamos, through proper distributing wires, to the houses and factories and shops where it is to be used for electric lighting, and sewing-machines, and lathes, and lifts, or whatever other mechanism wants driving power. Now the thing is to be done much more economically, I hope, and certainly with much greater simplicity and regularity, by keeping a Faure battery of 40,000 cells always being charged direct from the electric main, and applying a methodical system of removing sets of 50, and placing them on the town-supply circuits, while other sets of 50 are being regularly introduced into the great battery that is being charged, so as to keep its number always within 50 of the proper number, which would be about 40,000 if the potential at the emitting end of the main is 80,000 volts.

SECTION D

BIOLOGY

Department of Anthropology

OPENING ADDRESS BY PROF. W. H. FLOWER, LL.D., F.R.S., PRES. Z.S., V.P. ANTHROP. INST., &C., CHAIRMAN OF THE DEPARTMENT

IT is impossible for us to commence the work of this section of the Association without having vividly brought to our minds the loss which has befallen us since our last meeting—the loss of one who was our most characteristic representative of the complex science of anthropology—one who had for many years conducted with extraordinary energy, amidst multifarious other avocations, a series of researches into the history, customs, and physical characters of the early inhabitants of our island, for which he was so especially fitted by his archæological, historical, and literary, as well as his anatomical knowledge, and who was also the most popular and brilliant expositor, to assemblies such as meet together on these occasions, of the results of those researches. I need scarcely say that I refer to Prof. Rolleston.

Within the last few months the study of our subject in this country has received an impulse from the publication of a book—small in size, it is true, but full of materials for thought and instruction—the "Anthropology" of Mr. E. B. Tylor, the first work published in English with that title, and one very different in its scope and method from the old ethnological treatises.

The immense array of facts brought together in a small compass, the terseness and elegance of the style, the good taste and feeling with which difficult and often burning questions are treated, should give this book a wide circulation among all classes, and thoroughly familiarise both the word and the subject to English readers.

The origin and early history of man's civilisation, his language, his arts of life, his religion, science, and social customs in the primitive conditions of society, are subjects in which, in consequence of their direct continuity with the arts and sciences, religious, political, and social customs among which we all live, by which we are all influenced, and about which we all have opinions, every person of ordinary education can and should take an interest. In fact, really to understand all these problems in the complex condition in which they are presented to us now, we ought to study them in their more simple forms, and trace them as far as may be to their origins.

But, as the author remarks, this book is only an introduction to anthropology, rather than a summary of all that it teaches; and some, even those that many consider the most important, branches of the subject are but lightly touched upon, or wholly passed over.

In one of the estimates of the character and opinions of the very remarkable man and eminent statesman, whose death the country was mourning last spring, it was stated: "Lord Beaconsfield had a deep-rooted conviction of the vast importance of race, as determining the relative dominance both of societies and of individuals" (*Spectator*, April 23, 1881); and with regard to the question of what he meant by "race," we have a key in the last published work of the same acute observer of mankind: "Language and religion do not make a race—there is only one thing which makes a race, and that is blood" ("Endymion," vol. ii. p. 205). Now "blood" used in this sense is defined as "kindred; relation by natural descent from a common ancestor; consanguinity" (Webster's "Dictionary"). The study of the true relationship of the different races of men is then not only interesting from a scientific point of view, but of great importance to statesmanship in such a country as this, embracing subjects representing almost every known modification of the human species, whose varied and often conflicting interests have to be regulated and provided for. It is to want of appreciation of its importance that many of the inconsistencies and shortcomings of the government of our dependencies and colonies are due, especially the great inconsistency between a favourite English theory and a too common English practice—the former being that all men are morally and intellectually alike, the latter being that all are equally inferior to himself in all respects: both propositions egregiously fallacious. The study of race is at a low ebb indeed when we hear the same contemptuous epithet of "nigger" applied indiscriminately by the Englishman abroad to the blacks of the West Coast of Africa, the Kaffirs of Natal, the Lascars of Bombay, the Hindoos of Calcutta, the aborigines of Australia, and even the Maoris of New Zealand!

But how is he to know better? Where in this country is any instruction to be had? Where are the books to which he may turn for trustworthy information? The subject, as I have said, is but slightly touched upon in the last published treatise on anthropology in our language. The great work of Pritchard, a compendium of all that was known at the time it was written, is now almost entirely out of date. In not a single university or public institution throughout the three kingdoms is there any kind of systematic teaching, either of physical or of any other branch of anthropology, except so far as comparative philology may be considered as bearing upon the subject. The one society of which it is the special business to promote the study of these questions, the Anthropological Institute of Great Britain and Ireland, is, I regret to say, far from flourishing. An anthropological museum, in the proper sense of the word, either public or private, does not exist in this country.

What a contrast is this to what we see in almost every other nation in Europe! At Paris there is, first, the Muséum d'Histoire Naturelle, where man, as a zoological subject—almost entirely neglected in our British Museum—has a magnificent gallery allotted to him, abounding not only in illustrations of osteology, but also in models, casts, drawings, and anatomical preparations showing various points in his physical or natural history, which is expounded to the public in the free lectures of the venerable Prof. Quatrefages and his able coadjutor, Dr. Hamy; there is also the vigorous Society of Anthropology, which is stated in the last annual report to number 720 members, showing an increase of forty-four during the year 1880, and which is forming a museum on a most extensive scale; and, finally, the School of Anthropology, founded by the illustrious Broca, whose untimely death last year, instead of paralysing, seems to have stimulated, the energies of colleagues and pupils into increased activity. In this school, supported partly by private subscriptions, partly by the public liberality of the Municipality of Paris, and of the Department of the Seine, are laboratories in which all the processes of anthropological manipulation are practised by students and taught to travellers. Here all the bodies of persons of outlandish nationalities dying in any of the hospitals of Paris are dissected by competent and zealous observers, who carefully record every peculiarity of structure discovered, and are thus laying the foundation for an exhaustive and trustworthy collection of materials for the comparative anatomy of the races of man. Here, furthermore, are lectureships on all the different branches. Biological and anatomical anthropology, ethnology, prehistoric, linguistic, social, and medical anthropology are all treated of separately by eminent professors who have made these departments their special study. The influence of so much activity is spreading beyond the capital. The foundation of an anthropological society at Lyons has been announced within the present year.

In Germany, although there is not at present any institution organised like the school at Paris, the flourishing state of the Berlin Ethnological Society, which also reports a large increase in the number of its members, the various other societies and journals, and the important contributions which are continually being made from the numerous intellectual centres of that land of learning, all attest the interest which the study of man excites there. In Italy, in the Scandinavian kingdoms, in Russia, and even in Spain, there are signs of similar activity. A glance at the recent periodical literature of America, especially the publications of the Smithsonian Institution, will show how strongly

the scientific work of that country is setting in the same direction.

It is true that a very great proportion of the energies of the societies, institutions, and individuals who cultivate this vast subject are, in all these lands, as it is indeed to so great an extent in our own, devoted to that branch which borders upon the old and favourite studies of archæology and geology. The fascinating power of the pursuit of the earliest traces of man's existence upon the earth, with the possibilities of obtaining some glimpses of his mode of origin, is attested in the devotion seen everywhere in museums, in separate publications, and in journals, to pre-historic anthropology.

But, though the study of man's origin and earliest appearances upon the earth, and that of the structural modifications to which in course of time he has arrived, or the study of races, are intimately related, and will ultimately throw light upon one another, I venture to think that the latter is the more pressing of the two, as it is certainly the more practically important; and hence the necessity for greater attention to physical anthropology. In seeking for a criterion upon which to base our study of races, in looking for essential proofs of consanguinity of descent from common ancestors in different groups of men, I have no hesitation in saying that we must first look to their physical or anatomical characters, next to their moral and intellectual characters—for our purpose more difficult of apprehension and comparison—and, lastly, as affording hints, often valuable in aid of our researches, but rarely to be depended upon, unless corroborated from other sources, to language, religion, and social customs.

The study of the physical or anatomical character of the races of man is unfortunately a subject beset with innumerable difficulties. It can only be approached with full advantage by one already acquainted with the ordinary facts of human anatomy, and with a certain amount of zoological training. The methods used by the zoologist in discriminating species and varieties of animals, and the practice acquired in detecting minute resemblances and differences that an ordinary observer might overlook, are just what are required in the physical anthropologist.

As the great problem which is at the root of all zoology is to discover a natural classification of animals, so the aim of zoological anthropology is to discover a natural classification of man. A natural classification is an expression of our knowledge of real relationship, of consanguinity—of "blood," as the author of "Endymion" expresses it. When we can satisfactorily prove that any two of the known groups of mankind are descended from the same common stock, a point is gained. The more such points we have acquired, the more nearly shall we be able to picture to ourselves, not only the present, but the past distribution of the races of man upon the earth, and the mode and order in which they have been derived from one another.

The difficulties in the way of applying zoological principles to the classification of man are vastly greater than in the case of most animals; the problem being, as we shall see, one of much greater complexity. When groups of animals become so far differentiated from each other as to represent separate species, they remain isolated; they may break up into further subdivisions—in fact, it is only by further subdivision that new species can be formed; but it is of the very essence of species, as now universally understood by naturalists, that they cannot recombine, and so give rise to new forms. With the varieties of man it is otherwise. They have never so far separated as to answer to the physiological definition of species. All races are fertile one with another, though perhaps in different degrees. Hence new varieties have constantly been formed, not only by the segmentation, as it were, of a portion of one of the old stocks, but also by various combinations of those already established.

Neither of the old conceptions of the history of man, which pervaded the thought, and form the foundation of the works of all ethnological writers up to the last few years, rest on any solid basis, nor account for the phenomena of the present condition and distribution of the species.

The one view—that of the monogenist—was that all races, as we see them now, are the descendants of a single pair, who, in a comparatively short period of time, spread over the world from one common centre of origin, and became modified by degrees in consequence of changes of climate and other external conditions. The other—that of the polygenist—is that a certain number of varieties or species (no agreement has been arrived at as to the number, which is estimated by different authorities at from three to twenty or more) have been independently created in different parts of the world, and have perpetuated the distinctive characters as well as the geographical position with which they were originally endowed.

The view which appears best to accord with what is now known of the characters and distribution of the races of man, and with the general phenomena of nature, may be described as a modification of the former of these hypotheses.

Without entering into the difficult question of the method of man's first appearance upon the world, we must assume for it a vast antiquity—at all events as measured by any historical standard. Of this there is now ample proof. During the long time he existed in the savage state—a time compared to which the dawn of our historical period was as yesterday—he was influenced by the operation of those natural laws which have produced the variations seen in other regions of organic nature. The first men may very probably have been all alike; but, when spread over the face of the earth, and become subject to all kinds of diverse external conditions—climate, food, competition with members of his own species or with wild animals—racial differences began slowly to be developed through the potency of various kinds of selection acting upon the slight variations which appeared in individuals in obedience to the tendency implanted in all living things.

Geographical position must have been one of the main elements in determining the formation and the permanence of races. Groups of men isolated from their fellows for long periods, such as those living on small islands, to which their ancestors may have been accidentally drifted, would naturally, in course of time, develop a new type of features, of skull, of complexion or hair. A slight set in one direction, in any of these characters, would constantly tend to intensify itself, and so new races would be formed. In the same way different intellectual or moral qualities would be gradually developed and transmitted in different groups of men. The longer a race thus formed remained isolated, the more strongly impressed and the more permanent would its characteristics become, and less liable to be changed or lost, when the surrounding circumstances were altered, or under a moderate amount of intermixture from other races—the more "true," in fact, would it be. On the other hand, on large continental tracts, where no "mountains interposed make enemies of nations," or other natural barriers form obstacles to free intercourse between tribe and tribe, there would always be a tendency towards uniformity, from the amalgamation of races brought into close relation by war or by commerce. Smaller or feebler races have been destroyed or absorbed by others impelled by superabundant population or other causes to spread beyond their original limits; or sometimes the conquering race has itself disappeared by absorption into the conquered.

Thus, for untold ages, the history of man has presented a shifting kaleidoscopic scene; new races gradually becoming differentiated out of the old elements, and, after dwelling a while upon the earth, either becoming suddenly annihilated or gradually merged into new combinations; a constant destruction and reconstruction; a constant tendency to separation and differentiation, and a tendency to combine again into a common uniformity—the two tendencies acting against and modifying each other. The history of these processes in former times, except in so far as they may be inferred from the present state of things, is a difficult study, owing to the scarcity of evidence. If we had any approach to a complete palæontological record, the history of man could be reconstructed: but nothing of the kind is forthcoming. Evidences of the anatomical characters of man as he lived on the earth during the time when the great racial characteristics were being developed, during the long antehistoric period in which the negro, the Mongolian, and the Caucasian were being gradually fashioned into their respective types, is entirely wanting, or, if any exists, it is at present safely buried in the earth, perhaps to be revealed at some unexpected time and in some unforeseen manner.

It will be observed, and perhaps observed with perplexity by some, that no definition has as yet been given of the oft-recurring word "race." The sketch just drawn of the past history of man must be sufficient to show that any theory implying that the different individuals composing the human species can be parcelled out into certain definite groups, each with its well-marked and permanent limits separating it from all others, has no scientific foundation; but that in reality these individuals are aggregated into a number of groups of very different value in a zoological sense, with characters more or less strongly marked

and permanent, and often passing insensibly into one another. The great groups are split up into minor subdivisions, and filling up the gaps between them all are intermediate or intercalary forms, derived either from the survival of individuals retaining the generalised or ancestral characters of a race from which two branches have separated and taken opposite lines of modification, or from the reunion of members of such branches in recent times. If we could follow those authors who can classify mankind into such divisions as trunks, branches, races, and sub-races, each having its definite and equivalent meaning, our work would appear to be greatly simplified, although perhaps we should not be so near the truth we are seeking. But being not yet in a position to define what amount of modification is necessary to constitute distinction of race, I am compelled to use the word vaguely for any considerable group of men who resemble each other in certain common characters transmitted from generation to generation.

In approaching the question of the classification of the races of man from a physical point of view, we must bestow great care upon the characters upon which we rely in distinguishing one group from another. It is well known in zoology that the modifications of a single organ or system may be of great value, or may be quite useless according as such modifications are correlated with others in different organs or systems, or are mere isolated examples of variation in the economy of the animal without structural changes elsewhere. The older ornithologists associated in one order all the birds with webbed feet, and the order thus constituted, *Natatores* or *Palmipedes*, which received the great sanction of Cuvier, still stands in many zoological compilations. Recent investigations into the anatomy of birds have shown that the species thus associated together show no other sign of natural affinity, and no evidence of being derived from the same stock. In fact, there is tolerably good proof that the webbing of the feet is a merely adaptive character, developed or lost, present or absent, irrespective of other structural modifications. In the same way, when anthropology was less advanced than it is now, it was thought that the distinction between long and short-headed, dolichocephalic and brachycephalic, people, pointed out by Retzius, indicated a primary division of the human species; but it was afterwards discovered that, although the character was useful otherwise, it was one of only secondary importance, as the long-headed as well as the short-headed group both included races otherwise of the strongest dissimilarity.

In all classifications the point to be first ascertained is the fundamental plan of construction; but in cases where the fundamental plan has undergone but little modification, we are obliged to make use of what appear trivial characters, and compensate for their triviality by their number. The more numerous the combinations of specialised characters, by which any species or race differs from its congeners, the more confidence we have in their importance. The separation of what is essential from what is incidental or merely superficial in such characters lies at the root of all the problems of this nature that zoologists are called upon to solve; and in proportion as the difficulties involved in this delicate and often perplexing discrimination are successfully met and overcome will the value of the conclusions be increased. These difficulties, so familiar in zoology, are still greater in the case of anthropology. The differences we have to deal with are often very slight; their significance is at present very little understood. We go on expending time and trouble in heaping up elaborate tables of measurements, and minutely recording every point that is capable of description, with little regard to any conclusions that may be drawn from them. It is certainly time now to endeavour, if possible, to discriminate characters which indicate deep-lying affinity from those that are more transient, variable, or adaptive, and to adjust, as far as may be, the proper importance to be attached to each.

It is, however, quite to be expected that, in the infancy of all sciences, a vast amount of labour must be expended in learning the methods of investigation. In none has this been more conspicuous than in the subject under consideration. Many have come to despair, for instance, of any good commensurate with the time it occupies, coming of the minute and laborious work involved in craniometry. This is because nearly all our present methods are tentative. We have not yet learnt, or are only beginning to learn, what lines of investigation are profitable and what are barren. The results, even as far as we have gone, are, however, quite sufficient, in my opinion, to justify perseverance. I am, however, not so sure whether it be yet time to answer the demand, so eager and so natural, which is being made in many quarters, for the formulation of a definite plan of examination, measurement, and description to which all future investigation should rigidly adhere. All steps to promote agreement upon fundamental points are to be cordially welcomed, and meetings or congresses convened for such a purpose will be of use by giving opportunities for the impartial discussion of the relative value of different methods; but the agreement will finally be brought about by the general adoption of those measurements and methods which experience proves to be the most useful, while others will gradually fall into disuse by a kind of process of natural selection.

The changes and improvements which are being made yearly, almost monthly, in instruments and in methods, show what we should lose if we were to stop at any given period, and decree in solemn conclave that this shall be our final system, this instrument and this method shall be the only one used throughout the world, that no one shall depart from it. We scarcely need to ask how long such an agreement would be binding. The subject is not sufficiently advanced to be reduced to a state of stagnation such as this would bring it to.

To take an example from what is perhaps the most important of the anatomical characters by which man is distinguished from the lower animals, the superior from the inferior races of man; the smaller or greater projection forwards of the lower part of the face in relation to the skull proper, or that which contains the brain. From the time when Camper drew his facial angle, to the present day, the readiest and truest method of estimating this projection has occupied the attention of anatomists and anthropologists, and we are still far from any general agreement. Every country, every school, has its own system, so different that comparison with one another is well nigh impossible. This is undoubtedly an evil; but the question is whether we should all agree to adopt one of the confessedly defective systems now in vogue, or whether we should not rather continue to hope for, and endeavour to find, one which may not be subject to the well-known objections urged against all.

We want, especially in this country, more workers, trained and experienced men who will take up the subject seriously, and devote themselves to it continuously. Of such we may say, without offence to those few who have done occasional excellent work in physical anthropology, but whose chief scientific activity lies in other fields, we have not one. In the last number of the French *Revue d'Anthropologie*, a reference caught my eye to a craniometrical method in use by the "English school" of anthropologists. It was a reference only to a method which I had ventured to suggest, but which, as far as I know, has not been adopted by any one else. A school is just what we have not, and what we want—a body of men not only willing to learn, but able to discuss, to criticise, to give their approval to, or reduce to its proper level, the results put forth by our few original investigators and writers. The rapidity with which any one of the most slender pretensions who ventures into the field (I speak from painful experience) is raised to be an oracle among his fellows is one of the most alarming proofs of the present barrenness of the land.

Another most urgent need is the collection and preservation of the evidences of the physical structure of the various modifications of man upon the earth. Especially urgent is this now, as we live in an age in which, in a far greater degree than any previous one, the destruction of races, both by annihilation and absorption, is going on. The world has never witnessed such changes in its ethnology as those now taking place, owing to the rapid extension of maritime discovery and maritime commerce, which is especially affecting the island population among which, more than elsewhere, the solution of the most important anthropological problems may be looked for. If we have at present neither the knowledge nor the leisure to examine and describe, we can at least preserve from destruction the materials for our successors to work upon. Photographs, models, anatomical specimens, skeletons or parts of skeletons, with their histories carefully registered, of any of the so-called aboriginal races, now rapidly undergoing extermination or degeneration, will be hereafter of inestimable value. Drawings, descriptions, and measurements are also useful, though in a far less degree, as allowance must always be made for imperfections in the methods as well as the capacity of the artist or observer. Such collections must be made upon a far larger scale than has hitherto been attempted, as, owing to the difficulties already pointed out in the classification of man, it is only by large numbers that the errors arising

from individual peculiarities or accidental admixture can be obviated, and the prevailing characteristics of a race or group truly ascertained. It is only in an institution commanding the resources of the nation that such a collection can be formed, and it may therefore be confidently hoped that the trustees of the British Museum will appropriate some portion of the magnificent new building, which has been provided for the accommodation of their natural history collections, to this hitherto neglected branch of the subject.

I have mentioned two of the needs of anthropology in this country—more workers and better collections: there is still a third—that of a society or institution in which anthropologists can meet and discuss their respective views, with a journal in which the results of their investigations can be laid before the public, and a library in which they can find the books and periodicals necessary for their study. All this ought to be provided by the Anthropological Institute of Great Britain and Ireland, which originated in the amalgamation of the old Ethnological and Anthropological Societies. But, as I intimated some time ago, the Institute does not at the present time flourish as it should; its meetings are not so well attended as they might be; the journal is restricted in its powers of illustration and printing by want of funds; the library is quite insufficient for the needs of the student.

This certainly does not arise from any want of good management in the Society itself. Its afairs have been presided over and administered by some of the most eminent and able men the country has produced. Huxley, Lubbock, Busk, Evans, Tylor, and Pitt-Rivers have in succession given their energies to its service, and yet the number of its members is falling away, its usefulness is crippled, and its very existence seems precarious. Some decline to join the Institute, others leave it upon the plea that, being unable from distance or other causes to attend the meetings, they cannot obtain the full return for their subscriptions; others on the ground that the journal does not contain the exact information which they require.

There surely is to be found a sufficient number of persons who are influenced by different considerations, who feel that anthropological science is worth cultivating, and that those who are laboriously and patiently tracing out the complex problems of man's diversity and man's early history are doing a good work, and ought to be encouraged by having the means afforded them of carrying on their investigations and of placing the results of their researches before the world—who feel, moreover, that there ought to be some central body, representing the subject, which may, on occasion, influence opinion or speak authoritatively on matters often of great practical importance to the nation.

There must be many in this great and wealthy country who feel that they are helping a good cause in joining such a society, even if they are not individually receiving what they consider a full equivalent for their small subscription—many who feel satisfaction in helping the cause of knowledge, in helping to remove the opprobrium that the British Anthropological Society alone of those of the world is lacking in vitality, and in helping to prevent this country from falling behind all the nations in the cultivation of a science in which, for the strongest reasons, it might be expected to hold the foremost place. It is a far more grateful task to maintain, extend, and if need be improve, an existing organisation, than to construct a new one. I feel, therefore, no hesitation in urging upon all who take interest in the promotion of the study of anthropology to rally round the Institute, and to support the endeavours of the present excellent president to increase its usefulness.

Department of Anatomy and Physiology

OPENING ADDRESS BY J. BURDON-SANDERSON, M.D., LLD., F.R.S., PROFESSOR OF PHYSIOLOGY IN UNIVERSITY COLLEGE, LONDON, VICE-PRESIDENT OF THE SECTION

On the Discoveries of the Past Half-Century relating to Animal Motion

THE two great branches of Biology with which we concern ourselves in this section, Animal Morphology and Physiology, are most intimately related to each other. This arises from their having one subject of study—the living animal organism. The difference between them lies in this, that whereas the studies of the anatomist lead him to fix his attention on the organism itself, to us physiologists it, and the organs of which it is made up, serve only as *vestigia*, by means of which we investigate the vital processes of which they are alike the causes and consequences.

To illustrate this I will first ask you to imagine for a moment that you have before you one of those melancholy remainders of what was once an animal—to wit, a rabbit—which one sees exposed in the shops of poulterers. We have no hesitation in recognising that remainder as being in a certain sense a rabbit; but it is a very miserable vestige of what was a few days ago enjoying life in some wood or warren, or more likely on the sand-hills near Ostend. We may call it a rabbit if we like, but it is only a remainder—not the thing itself.

The anatomical preparation which I have in imagination placed before you, although it has lost its inside and its outside, its integument and its viscera, still retains the parts for which the rest existed. The final cause of an animal, whether human or other, is muscular action, because it is by means of its muscles that it maintains its external relations. It is by our muscles exclusively that we act on each other. The articulate sounds by which I am addressing you are but the results of complicated combinations of muscular contractions—and so are the scarcely appreciable changes in your countenances by which I am able to judge how much, or how little, what I am saying interests you.

Consequently the main problems of physiology relate to muscular action, or, as I have called it, animal motion. They may be divided into two—namely (1) in what does muscular action consist—that is, what is the process of which it is the effect or outcome? and (2) how are the motions of our bodies co-ordinated or regulated? It is unnecessary to occupy time in showing that, excluding those higher intellectual processes which, as they leave no traceable marks behind them, are beyond the reach of our methods of investigation, these two questions comprise all others concerning animal motion. I will therefore proceed at once to the first of them—that of the process of muscular contraction.

The years which immediately followed the origin of the British Association exceeded any earlier period of equal length in the number and importance of the new facts in morphology and in physiology which were brought to light; for it was during that period that Johannes Müller, Schwann, Henle, and, in this country, Sharpey, Bowman, and Marshall Hall, accomplished their productive labours. But it was introductory to a much greater epoch. It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologist—*e.g.* J. Müller—recognised that the knowledge they possessed both of vital and physical phenomena was insufficient to refer both to a common measure. The method, therefore, was to study the processes of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all, unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. Let us inquire for a moment what causes have conduced to the change.

The most efficient cause was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the Conservation of Energy. In the application of this great principle to physiology, the men to whom we are indebted are, first and foremost, J. R. Mayer, of whom I shall say more immediately; and secondly, to the great physiologists still living and working among us, who were the pupils of J. Müller—viz. Helmholtz, Ludwig, Du Bois-Reymond, and Brücke.

As regards the subject which is first to occupy our attention, that of the *process* of muscular contraction, J. R. Mayer occupies so leading a position that a large proportion of the researches which have been done since the new era, which he had so important a share in establishing, may be rightly considered as the working out of principles enunciated in his treatise[1] on the relation between organic motion and exchange of material. The most important of these were, as expressed in his own words: (1) "That the chemical force contained in the ingested food and in the inhaled oxygen is the source of the motion and heat which are the two products of animal life; and (2) that these products vary in amount with the chemical process which produces them." Whatever may be the claims of Mayer to be regarded as a great discover in physics, there can be no doubt

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: ein Beitrag zur Naturkunde," Heilbronn, 1845.

that as a physiologist he deserves the highest place that we can give him, for at a time when the notion of the correlation of different modes of motion was as yet very unfamiliar to the physicist, he boldly applied it to the phenomena of animal life, and thus re-united physiology with natural philosophy, from which it had been rightly, because unavoidably, severed by the vitalists of an earlier period.

Let me first endeavour shortly to explain how Mayer himself applied the principle just enunciated, and then how it has been developed experimentally since his time.

The fundamental notion is this: the animal body resembles, as regards the work it does and the heat it produces, a steam-engine, in which fuel is continually being used on the one hand, and work is being done and heat produced on the other. The using of fuel is the chemical process, which in the animal body, as in the steam-engine, is a process of oxidation. Heat and work are the useful products, for as, in the higher animals, the body can only work at a constant temperature of about 100° F., heat may be so regarded.

Having previously determined the heat and work severally producible by the combustion of a given weight of carbon, from his own experiments and from those of earlier physicists, Mayer calculated that if the oxidation of carbon is assumed to represent approximately the oxidation process of the body, the quantity of carbon actually burnt in a day is far more than sufficient to account for the day's work, and that of the material expended in the body not more than one-fifth was used in the doing of work, the remaining four-fifths being partly used, partly wasted in heat production.

Having thus shown that the principles of the correlation of process and product hold good, so far as its truth could then be tested, as regards the whole organism, Mayer proceeded to inquire into its applicability to the particular organ whose function it is "to transform chemical difference into mechanical effect"—namely, muscle. Although, he said, a muscle acts under the direction of the will, it does not derive its power of acting from the will any more than a steamboat derives its power of motion from the helmsman. Again (and this was of more importance, as being more directly opposed to the prevalent vitalism), a muscle, like the steamboats use in the doing of work, not the material of its own structure, or mechanism, but the fuel—*i.e.* the nutriment—which it derives directly from the blood which flows through its capillaries. "The muscle is the instrument by which the transformation of force is accomplished, not the material which is itself transformed." This principle he exemplified in several ways, showing that if the muscle of our bodies worked, as was formerly supposed, at the expense of their own substance, their whole material would be used up in a few weeks, and that in the case of the heart, a muscle which works at a much greater rate than any other, it would be expended in as many days—a result which necessarily involved the absurd hypothesis that the muscular fibres of our hearts are so frequently disintegrated and reintegrated that we get new hearts once a week.

On such considerations Mayer founded the prevision, that, as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the limits of the chemical process either in the whole animal body or in a single muscle during a given period, and to measure the production of heat and the work done during the same period, the result would show a quantitative correlation between them.

If the time at our disposal permitted, I should like to give a short account of the succession of laborious investigations by which these previsions have been verified. Begun by Bidder and Schmidt in 1851,[1] continued by Pettenkofer and Voit,[2] and by the agricultural physiologists[3] with reference to herbivora, they are not yet by any means completed. I must content myself with saying that by these experiments the first and second parts of this great subject—namely, the limits of the chemical process of animal life and its relation to animal motion under different conditions—have been satisfactorily worked out, but that the quantitative relations of heat production are as yet only insufficiently determined.

Let me sum up in as few words as possible how far what we have now learnt by experiment justifies Mayer's anticipations, and how it falls short of or exceeds them. First of all, we are as certain as of any physical fact that the animal body in doing work does not use its own material—that, as Mayer says, the oil to his lamp of life is food; but in addition to this we know what he was unaware of, that what is used is not only not the living protoplasm itself, but is a kind of material which widely differs from it in chemical properties. In what may be called commercial physiology—*i.e.*, in the literature of trade puffs—one still meets with the assumption that the material basis of muscular motion is nitrogenous; but by many methods of proof it has been shown that the true "Oel in der Flamme des Lebens" is not proteid substance, but sugar, or sugar-producing material. The discovery of this fundamental truth we owe first to Bernard (1850-56), who brought to light the fact that such material plays an important part in the nutrition of every living tissue; secondly, to Voit (1866), who in elaborate experiments on carnivorous animals, during periods of rest and exertion, showed that, in comparing those conditions, no relation whatever shows itself between the quantity of proteid material (flesh) consumed, and the amount of work done; and finally to Frankland, Fick, and his associate Wislicenus, as to the work-yielding value of different constituents of food, and as to the actual expenditure of material in man during severe exertion. The subjects of experiment used by the two last-mentioned physiologists were themselves; the work done was the mountain ascent from Interlaken to the summit of the Faulhorn; the result was to prove that the quantity of material used was proportional to the work done, and that that material was such as to yield water and carbonic acid exclusively.

The investigators to whom I have just referred aimed at proving the correlation of process and product for the whole animal organism. The other mode of inquiry proposed by Mayer, the verification of his principle in respect of the work-doing mechanism—that is to say, in respect of muscle taken separately—has been pursued with equal perseverance during the last twenty years, and with greater success; for in experimenting on a separate organ, which has no other functions excepting those which are in question, it is possible to eliminate uncertainties which are unavoidable when the conditions of the problem are more complicated. Before I attempt to sketch the results of these experiments, I must ask your attention for a moment to the discoveries made since Mayer's epoch, concerning a closely related subject, that of the Process of Respiration.

I wish that I had time to go back to the great discovery of Priestley (1776), that the essential facts in the process of respiration are the giving off of fixed air, as he called it, and the taking in of dephlogisticated air, and to relate to you the beautiful experiments by which he proved it; and then to pass on to Lavoisier (1777), who, on the other side of the Channel, made independently what was substantially the same discovery a little after Priestley, and added others of even greater moment. According to Lavoisier, the chemical process of respiration is a slow combustion which has its seat in the lungs. At the time that Mayer wrote, this doctrine still maintained its ascendency, although the investigations of Magnus (1838) had already proved its fallacy. Mayer himself knew that the blood possessed the property of conveying oxygen from the lungs to the capillaries, and of conveying carbonic acid gas from the capillaries to the lungs, which was sufficient to exclude the doctrine of Lavoisier. Our present knowledge of the subject was attained by two methods—viz., first, the investigation of the properties of the colouring matter of the blood, since called "hæmoglobin," the initial step in which was made by Prof. Stokes in 1862; and secondly, the application of the mercurial air-pump as a means of determining the relations of oxygen and carbonic acid gas to the living blood and tissues. The last is a matter of such importance in relation to our subject that I shall ask your special attention to it. Suppose that I have a barometer of which the tube, instead of being of the ordinary form, is expanded at the top into a large bulb of one or two litres capacity, and that, by means of some suitable contrivance, I am able to introduce, in such a way as to lose no time and to preclude the possibility of contact with air, a fluid ounce of blood from the artery of a living animal into the vacuous space—what would happen? Instantly the quantity of blood would be converted into froth, which would occupy the whole of the large bulb. The colour of the froth would at first be scarlet, but would speedily change to crimson. It would soon subside, and we should then have the cavity which was before vacuous occupied by the blood and its gases—namely, the oxygen, carbonic acid gas, and nitrogen previously contained in it. And if we had the means (which

[1] Bidder and Schmidt, "Die Verdauungssäfte und der Stoffwechsel," Leipzig. 1852.

[2] Pettenkofer and Voit, *Zeitschr. f. Biologie*, passim, 1866-80.

[3] Henneberg and Stohmann, "Beiträge zur Begründung einer rationellen Futterung der Wiederkäuer," Brunswick and Göttingen, 1860-70.

actually exist in the gas-pump) of separating the gaseous mixture from the liquid, and of renewing the vacuum, we should be able to determine (1) the total quantity of gases which the blood yields, and (2), by analysis, the proportion of each gas.

Now, with reference to the blood, by the application of the "blood-pump," as it is called, we have learnt a great many facts relating to the nature of respiration, particularly that the difference of venous from arterial blood depends not on the presence of "effete matter," as used to be thought, but on the less amount of oxygen held by its colouring matter, and that the blood which flows back to the heart from different organs, and at different times, differs in the amount of oxygen and of carbonic acid gas it yields, according to the activity of the chemical processes which have their seat in the living tissues from which it flows.[1] But this is not all that the blood-pump has done for us. By applying it not merely to the blood, but to the tissues, we have learnt that the doctrine of Lavoisier was wrong, not merely as regards the place, but as regards the nature of the essential process in respiration. The fundamental fact which is thus brought to light is this, that although living tissues are constantly and freely supplied with oxygen, and are in fact constantly tearing it from the hæmoglobin which holds it, yet they themselves yield no oxygen to the vacuum. In other words, the oxygen which living protoplasm seizes upon with such energy that the blood which flows by it is compelled to yield it up, becomes so entirely part of the living material itself that it cannot be separated even by the vacuum. It is in this way only that we can understand the seeming paradox that the oxygen, which is conveyed in abundance to every recess of our bodies by the blood-stream, is nowhere to be found. Notwithstanding that no oxidation-product is formed, it becomes latent in every bit of living protoplasm; stored up in quantity proportional to its potential activity—*i.e.* to the work, internal or external, it has to do.

Thus you see that the process of tissue respiration—in other words, the relation of living protoplasm to oxygen—is very different from what Mayer, who localised oxidation in the capillaries, believed it to be. And this difference has a good deal to do with the relation of Process to Product in muscle. Let us now revert to the experiments on this subject which we are to take as exemplification of the truth of Mayer's forecasts.

The living muscle of a frog is placed in a closed chamber, which is vacuous—*i.e.* contains only aqueous vapour. The chamber is so arranged that the muscle can be made to contract as often as necessary. At the end of a certain period it is found that the chamber now contains carbonic acid gas in quantity corresponding to the number of contractions the muscle has performed. The water which it has also given off cannot of course be estimated. Where do these two products come from? The answer is plain. The muscle has been living all the time, for it has been doing work, and (as we shall see immediately) producing heat. What has it been living on? Evidently on stored material. If so, of what nature? If we look for the answer to the muscle, we shall find that it contains both proteid and sugar-producing material, but which is expended in contraction we are not informed. There is, however, a way out of the difficulty. We have seen that the only chemical products which are given off during contraction are carbonic acid gas and water. It is clear, therefore, that the material on which it feeds must be something which yields, when oxidised, these products, and these only. The materials which are stored in muscle are oxygen and sugar, or something resembling it in chemical composition.

And now we come to the last point I have to bring before you in connection with this part of my subject. I have assumed up to this moment that heat is always produced when a muscle does work. Most people will be ready to admit as evidence of this, the familiar fact that we warm ourselves by exertion. This is in reality no proof at all.

The proof is obtained when, a muscle being set to contract, it is observed that at each contraction it becomes warmer. In such an experiment, if the heat capacity of muscle is known, the weight of the particular muscle, and the increase of temperature, we have the quantity of heat produced.

If you determine these data in respect of a series of contractions, arranging the experiments so that the work done in each contraction is measured, and immediately thereupon reconverted into heat, the result gives you the total product of the oxidation process in heat.

[1] Ludwig's first important research on this subject was published in 1862.

If you repeat the same experiment in such a way that the work done in each contraction is not so reconverted, the result is *less* by the quantity of heat corresponding to the work done. The results of these two experiments have been found by Prof. Fick to cover each other very exactly. I have stated them in a table[1] in which we have the realisation as regards a single muscle of the following forecast of Mayer's as regards the whole animal organism. "Convert into heat," he said, "by friction or otherwise, the mechanical product yielded by an animal in a given time, add thereto the heat produced in the body directly during the same period, and you will have the total quantity of heat which corresponds to the chemical processes." We have seen that this is realisable as regards muscle, but it is not even yet within reach of experimental verification as regards the whole animal.

I now proceed abruptly (for the time at our disposal does not admit of our spending it on transitions) to the consideration of the other great question concerning vital motion, namely the question how the actions of the muscles of an animal are so regulated and co ordinated as to determine the combined movements, whether rhythmical or voluntary, of the whole body.

As every one knows who has read the "Lay Sermons," the nature and meaning of these often unintentional but always adapted motions, which constitute so large a part of our bodily activity, was understood by Descartes early in the seventeenth century. Without saying anything as to his direct influence on his contemporaries and successors, there can be no doubt that the appearance of Descartes was coincident with a great epoch—an epoch of great men and great achievements in the acquirement of man's intellectual mastery over nature. When he interpreted the unconscious closing of the eyelids on the approach of external objects, the acts of coughing, sneezing, and the like as mechanical and reflected processes, he neither knew in what part of the nervous system the mechanisms concerned were situated, nor how they acted.[2] It was not until a hundred years after that Whytt and Hales made the fundamental experiments on beheaded frogs, by which they showed that the involuntary motions which such preparations execute cease when the whole of the spinal cord is destroyed—that if the back part of the cord is destroyed, the motions of the hind limbs, if the fore part, those of the fore limbs cease. It was in 1751 that Dr. Whytt published in Edinburgh his work on the involuntary motions of animals. After this the next great step was made within the recollection of living physiologists: a period to which, as it coincided with the event which we are now commemorating—the origin of the British Association—I will now ask your special attention.

Exactly forty-nine years ago Dr. Marshall Hall communicated to the Zoological Society of London the first account of his experiments on the reflex function of the spinal cord. The facts which he had observed, and the conclusions he drew from them, were entirely new to him, and entirely new to the physiologists to whom his communication was addressed. Nor can there be any reason why the anticipation of his fundamental discovery by Dr. Whytt should be held to diminish his merit as an original

[1] RELATION OF PRODUCT AND PROCESS IN MUSCLE
(Result of one of Fick's experiments)

Mechanical product	6670	grammemillimeters.
Its heat-value	15·6	milligrammeunits.
Heat produced	39·0	"
Total product reckoned as heat ...	54·6	"

[2] Descartes' scheme of the central nervous mechanism comprised all the parts which we now regard as essential to "reflex-action." Sensory nerves were represented by threads (filets) which connected all parts of the body to the brain ("Œuvres," par V. Cousin, vol. iv. p. 359): motor nerves by tubes which extended from the brain to the muscles; "motor centres" by "pores" which were arranged on the internal surface of the ventricular cavity of the brain and guarded the entrances to the motor tubes. This cavity was supposed to be kept constantly charged with "animal spirits" furnished to it from the heart by arteries specially destined for the purpose. Any "incitation" of the surface of the body by an external object which affects the organs of sense does so, according to Descartes, by producing a *motion* at the incited part. This is communicated to the pore by the thread and causes it to open, the consequence of which is that the "animal spirit" contained in the ventricular cavity enters the tube and is conveyed by it to the various muscles with which it is connected, so as to produce the appropriate motions. The whole system, although it was placed under the supervision of the "*âme raisonable*" which had its office in the pineal gland, was capable of working independently. As instances of this mechanism Descartes gives the withdrawal of the foot on the approach of hot objects, the actions of swallowing, yawning, coughing, &c. As it is necessary that, in the performance of these complicated motions, the muscles concerned should contract in succession, provision is made for this in the construction of the systems of tubes which represent the motor nerves. The weakness of the scheme lies in the absence of fact basis. Neither threads nor pores nor tubes have any existence.

investigator. In the face of historical fact it is impossible to regard him as the discoverer of the "reflex function of the spinal cord," but we do not the less owe him gratitude for the application he made of the knowledge he had gained by experiments on animals to the study of disease. For no one who is acquainted with the development of the branch of practical medicine which relates to the diseases of the central nervous system will hesitate in attributing the rapid progress which has been made in the diagnosis and treatment of these diseases, to the impulse given by Dr. Marshall Hall to the study of nervous pathology.

In the mind of Dr. Marshall Hall the word reflex had a very restricted meaning. The term "excito-motary function," which he also used, stood in his mind for a group of phenomena of which it was the sole characteristic that a sensory impression produced a motor response. During the thirty years which have elapsed since his death, the development of meaning of the word reflex has been comparable to that of a plant from a seed. The original conception of reflex action has undergone, not only expansion, but also modification, so that in its wider sense it may be regarded as the empirical development of the philosophical views of the animal mechanism promulgated by Descartes. Not that the work of the past thirty years by which the physiology of the nervous system has been constituted can be attributed for a moment to the direct influence of Descartes. The real epoch-maker here was Johannes Muller. There can be no doubt that Descartes' physiological speculations were well known to him, and that his large acquaintance with the thought and work of his predecessors conduced, with his own powers of observation, to make him the great man that he was; but to imagine that his ideas of the mechanism of the nervous system were inspired, or the investigations by which, contemporaneously with Dr. Marshall Hall, he demonstrated the fundamental facts of reflex action, were suggested by the animal automatism of Descartes, seems to me wholly improbable.

I propose, by way of conclusion, to attempt to illustrate the nature of reflex action in the larger sense, or, as I should prefer to call it, the Automatic Action of Centres, by a single example —that of the nervous mechanism by which the circulation is regulated.

The same year that J. R. Mayer published his memorable essay, it was discovered by E. H. Weber that, in the vagus nerve, which springs from the medulla oblongata and proceeds therefrom to the heart, there exist channels of influence by which the medulla acts on that wonderful muscular mechanism. Almost at the same time with this, a series of discoveries [1] were made relating to the circulation, which, taken together, must be regarded as of equal importance with the original discovery of Harvey. First, it was found by Henle that the arterial blood-vessels by which blood is distributed to brain, nerve, muscle, gland, and other organs, are provided with muscular walls like those of the heart itself, by the contraction or dilatation of which the supply is increased or diminished according to the requirements of the particular organ. Secondly, it was discovered simultaneously, but independently, by Brown-Séquard and Augustus Waller, that these arteries are connected by nervous channels of influence with the brain and spinal cord, just as the heart is. Thirdly, it was demonstrated by Bernard that what may be called the heart-managing channels spring from a small spot of grey substance in the medulla oblongata, which we now call the "heart-centre"; and a little later by Schiff, that the artery-regulating channels spring from a similar head-central office, also situated in the medulla oblongata, but higher up, and from subordinate centres in the spinal cord.

If I had the whole day at my disposal and your patience were inexhaustible, I might attempt to give an outline of the issues to which these five discoveries have led. As it is, I must limit myself to a brief discussion of their relations to each other, in order that we may learn something from them as to the nature of automatic action.

Sir Isaac Newton, who, although he knew nothing about the structure of nerves, made some shrewd forecasts about their action, attributed to those which are connected with muscles an alternative function. He thought that by means of motor nerves the brain could determine either relaxation or contraction of muscles. Now as regards ordinary muscles, we know that this is not the case. We can will only the shortening of a muscle, not its lengthening. When Brown-Séquard discovered the function of the motor nerves of the blood-vessels, he assumed that the same limitation was applicable to it as to that of muscular nerves in general. It was soon found, however, that this assumption was not true in all cases—that there were certain instances in which, when the vascular nerves were interfered with, dilatation of the blood-vessels, consequent on relaxation of their muscles, took place; and that, in fact, the nervous mechanism by which the circulation is regulated is a highly-complicated one, of which the best that we can say is that it is perfectly adapted to its purpose. For while every organ is supplied with muscular arteries, and every artery with vascular nerves, the influence which these transmit is here relaxing, there constricting, according (1) to the function which the organ is called upon to discharge; and (2) the degree of its activity at the time. At the same time the whole mechanism is controlled by one and the same central office, the locality of which we can determine with exactitude by experiment on the living animal, notwithstanding that its structure affords no indication whatever of its fitness for the function it is destined to fulfil. To judge of the complicated nature of this function we need only consider that in no single organ of the body is the supply of blood required always the same. The brain is during one hour hard at work, during the next hour asleep; the muscles are at one moment in severe exercise, the next in complete repose; the liver, which before a meal is inactive, during the process of digestion is turgid with blood, and busily engaged in the chemical work which belongs to it. For all these vicissitudes the tract of grey substance which we call the *vascular centre* has to provide. Like a skilful steward of the animal household, it has, so to speak, to exercise perfect and unfailing foresight, in order that the nutritive material which serves as the oil of life for the maintenance of each vital process, may not be wanting. The fact that this wonderful function is localised in a particular bit of grey substance is what is meant by the expression "automatic action of a centre."

But up to this point we have looked at the subject from one side only.

No state ever existed of which the administration was exclusively executive—no government which was, if I may be excused the expression, absolutely absolute. If in the animal organism we impose on a centre the responsibility of governing a particular mechanism or process, independently of direction from above, we must give that centre the means of being itself influenced by what is going on in all parts of its area of government. In other words, it is as essential that there should be channels of information passing inwards, as that there should be channels of influence passing outwards. Now what is the nature of these channels of information? Experiment has taught us not merely with reference to the regulation of the circulation, but with reference to all other automatic mechanisms, that they are as various in their adaptation as the outgoing channels of influence. Thus the vascular centre in the medulla oblongata is so cognisant of the chemical condition of the blood which flows through it, that if too much carbonic acid gas is contained in it, the centre acts on information of the fact, so as to increase the velocity of the blood-stream, and so promote the arterialisation of the blood. Still more strikingly is this adaptation seen in the arrangement by which the balance of pressure and resistance in the blood-vessels is regulated. The heart, that wonderful muscular machine by which the circulation is maintained, is connected with the centre, as if by two telegraph wires—one of which is a channel of influence, the other of information. By the latter the engineer who has charge of that machine sends information to headquarters whenever the strain on his machine is excessive, the certain response to which is relaxation of the arteries and diminution of pressure. By the former he is enabled to adapt its rate of working to the work it has to do.

If Dr. Whytt, instead of cutting off the head of his frog, had removed only its brain—*i.e.*, the organ of thought and consciousness—he would have been more astonished than he actually was at the result; for a frog so conditioned exhibits, as regards its bodily movements, as perfect adaptiveness as a normal frog. But very little careful observation is sufficient to show the difference. Being incapable of the simplest mental acts, this true animal automaton has no notion of requiring food or of seeking it, has no motive for moving from the place it happens to

[1] The dates of the discoveries relating to this subject here referred to are as follows:—Muscular Structure of Arteries, Henle, 1841; Function of Cardiac Vagus, E. H. Weber, 1845; Constricting Nerves of Arteries, B. Séquard, 1852, Aug. Waller, 1853; Cardiac Centre, Bernard, 1858; Vascular Centre, Schiff, 1858; Dilating Nerves, Schiff, 1854; Eckhard, 1864; Lovén, 1866. Of the more recent researches by which the further elucidation of the mechanism by which the distribution of blood is adapted to the requirements of each organ, the most important are those of Ludwig and his pupils and of Heidenhain.

occupy, emits no utterance of pleasure or distress. Its life processes continue so long as material remains, and are regulated mechanically.

To understand this all that is necessary is to extend the considerations which have been suggested to us in our very cursory study of the nervous mechanism by which the working of the heart and of arteries is governed, to those of locomotion and voice. Both of these we know, on experimental evidence similar to that which enables us to localise the vascular centre, to be regulated by a centre of the same kind. If the behaviour of the brainless frog is so natural that even the careful and intelligent observer finds it difficult to attribute it to anything less than intelligence, let us ask ourselves whether the chief reason of the difficulty does not lie in this, that the motions in question are habitually performed intelligently and consciously. Regarded as mere mechanisms, those of locomotion are no doubt more complicated than those of respiration or circulation, but the difference is one of degree, not of kind. And if the respiratory movements are so controlled and regulated by the automatic centre which governs them, that they adapt themselves perfectly to the varying requirements of the organism, there is no reason why we should hesitate in attributing to the centres which preside over locomotion powers which are somewhat more extended.

But perhaps the question has already presented itself to your minds, What does all this come to? Admitting that we are able to prove (1) that in the animal body, Product is always proportional to Process, and (2), as I have endeavoured to show you in the second part of my discourse, that Descartes' dream of animal automatism has been realised, what have we learnt thereby? Is it true that the work of the last generation is worth more than that of preceding ones?

If I only desired to convince you that during the last half-century there has been a greater accession of knowledge about the function of the living organism than during the previous one, I might arrange here in a small heap at one end of the table the physiological works of the Hunters, Spallanzani, Fontana, Thomas Young, Benjamin Brodie, Charles Bell, and others, and then proceed to cover the rest of it with the records of original research on physiological subjects since 1831, I should find that, even if I included only genuine work, I should have to heap my table up to the ceiling. But I apprehend this would not give us a true answer to our question. Although, etymologically, Science and Knowledge mean the same thing, their real meaning is different. By science we mean, first of all, that knowledge which enables us to sort the things known according to their true relations. On this ground we call Haller the father of physiology, because, regardless of existing theories, he brought together into a system all that was then known by observation or experiment as to the processes of the living body. But in the "Elementa Physiologiæ" we have rather that out of which science springs than science itself. Science can hardly be said to begin until we have by experiment acquired such a knowledge of the relation between events and their antecedents, between processes and their products, that in our own sphere we are able to forecast the operations of nature, even when they lie beyond the reach of direct observation. I would accordingly claim for physiology a place in the sisterhood of the sciences, not because so large a number of new facts have been brought to light, but because she has in her measure acquired that gift of prevision which has been long enjoyed by the higher branches of natural philosophy. In illustration of this I have endeavoured to show you that every step of the laborious investigations undertaken during the last thirty years as to the process of nutrition, has been inspired by the previsions of J. R. Mayer, and that what we have learnt with so much labour by experiments on animals is but the realisation of conceptions which existed two hundred years ago in the mind of Descartes as to the mechanism of the nervous system. If I wanted another example I might find it in the previsions of Dr. Thomas Young as to the mechanism of the circulation, which for thirty years were utterly disregarded, until, at the epoch to which I have so often adverted, they received their full justification from the experimental investigations of Ludwig.

But perhaps it will occur to some one that if physiology founds her claim to be regarded as a science on her power of anticipating the results of her own experiments, it is unnecessary to make experiments at all. Although this objection has been frequently heard lately from certain persons who call themselves philosophers, it is not very likely to be made seriously here. The answer is, that it is contrary to experience. Although we work in the certainty that every experimental result will come out in accordance with great principles (such as the principle that every plant or animal is both, as regards form and function, the outcome of its past and present conditions, and that in every vital process the same relations obtain between expenditure and product as hold outside of the organism), these principles do little more for us than indicate the direction in which we are to proceed. The history of science teaches us that a general principle is like a ripe seed, which may remain useless and inactive for an indefinite period, until the conditions favourable to its germination come into existence. Thus the conditions for which the theory of animal automatism of Descartes had to wait two centuries, were (1) the acquirement of an adequate knowledge of the structure of the animal organism, and (2) the development of the sciences of physics and chemistry; for at no earlier moment were these sciences competent to furnish either the knowledge or the methods necessary for its experimental realisation; and for a reason precisely similar Young's theory of the circulation was disregarded for thirty years.

I trust that the examples I have placed before you to-day may have been sufficient to show that the investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life, and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

The Science of Physiology has already afforded her aid to the Art of Medicine in furnishing her with a vast store of knowledge obtained by the experimental investigation of the action of remedies and of the causes of disease. These investigations are now being carried on in all parts of the world with great diligence, so that we may confidently anticipate that during the next generation the progress of pathology will be as rapid as that of physiology has been in the past, and that as time goes on the practice of medicine will gradually come more and more under the influence of scientific knowledge. That this change is already in progress we have abundant evidence. We need make no effort to hasten the process, for we may be quite sure that, as soon as science is competent to dictate, art will be ready to obey.

SECTION F

GEOGRAPHY

OPENING ADDRESS BY SIR JOSEPH D. HOOKER, C.B., K.C.S.I., F.R.S., &c., PRESIDENT OF THE SECTION

On Geographical Distribution

IT has been suggested that a leading feature of the sectional addresses to be delivered on the occasion of this, the fiftieth anniversary of the meetings of the British Association, should be a review of the progress made during the last half century in the branches of knowledge which the sections respectively represent.

It has further been arranged that, at so auspicious an epoch, the sections should, when possible, be presided over by past Presidents of the Association. This has resulted in almost every sectional chair being occupied by a President eminent as a cultivator of the science with which his section will be engaged, though not the one I have the honour of filling, which, from the fact of there being no professed geographer amongst the surviving past Presidents, has been confided to an amateur.

Under these circumstances I should be untrue to myself and to you, if I presumed to address you as one conversant with geography in any extended signification of the word, or if I attempted to deal with that important and attractive branch of it, topographical discovery, which claims more or less exclusively the time and attention of the geographers of this country. It is more fitting for me, and more in keeping with the objects of this Association, that I be allowed to discourse before you on one of the many branches of science the pursuit of which is involved in the higher aims of geographers, and which, as we are informed by an accomplished cultivator of the science, are

integral portions of scientific geography.[1] Of these none is more important than that of the distribution of animals and plants, which further recommends itself to you on this occasion from being a subject that owes its great progress during the last half-century as much to the theories advanced by celebrated voyagers and travellers as to their observations and collections.

Before, however, I proceed to offer you a sketch of the progress made during the lifetime of the Association in this one branch, I must digress to remind you, however briefly, of the even greater advances made in others, in many cases through the direct or indirect instrumentality of the Association itself, acting in concert with the Royal and with the Royal Geographical Societies.

In topography the knowledge obtained during this half century has been unprecedently great. The veil has been withdrawn from the sources of the Nile, and the lake systems of Central Africa have been approximately localised and outlined. Australia, never previously traversed, has been crossed and recrossed in various directions. New Guinea has had its coasts surveyed, and its previously utterly unknown interior has been here and there visited. The topography of Western China and Central Asia, which had been sealed books since the days of Marco Polo, has been explored in many quarters. The elevations of the highest mountains of both hemispheres have been accurately determined, and themselves ascended to heights never before attained; and the upper regions of the air have been ballooned to the extreme limit beyond which the life-sustaining organs of the human frame can no longer perform their functions. In hydrography the depths of the great oceans have been sounded, their shores mapped, and their physical and natural history explored from the Equator to beyond both polar circles. In the Arctic regions the highest hitherto attained latitudes have been reached; Greenland has been proved to be an island; and an archipelago has been discovered nearer to the Pole than any other land. In the Antarctic regions a new continent has been added to our maps, crowned with one of the loftiest known active volcanoes, and the Antarctic ocean has been twice traversed to the 79th parallel. Nor have some of the negative results of modern exploration been less important, for the Mountains of the Moon and many lesser chains have been expunged from our maps, and there are no longer believers in the inland sea of Australia or in the open ocean of the Arctic pole. Of these and many others of the geographical discoveries of the last half-century full accounts will be laid before you, prepared for this section by able geographers; of whom Mr. Markham will contribute Arctic discovery; Sir Richard Temple, Asiatic; Lieut.-Col. Sir James Grant and Mr. H. Waller, African; Mr. Moseley, Australian; Mr. Trelawny Saunders, Syrian (including the Holy Land); the Hydrographer of the Admiralty will undertake the great oceans, and Mr. F. Galton will discuss the improvements effected in the instruments, appliances, and methods of investigation employed in geographical researches.

Of other branches of science which are auxiliary to scientific geography, the majority will be treated of in the sections of the Association to which they belong; but there are a few which I must not, in justice to the geographers who have so largely contributed to their advance, leave unnoticed.

Such is Terrestrial Magnetism,[2] which had as its first investigators two of our earliest voyagers, the ill-fated Hudson and Halley, who determined the magnetic dip in the north polar and tropical regions respectively. Theirs were the precursors of a long series of scientific expeditions, during which the dipping needle was carried almost from Pole to Pole, and which culminated in the establishment, mainly under the auspices of this Association, of the magnetic survey of Great Britain, of fixed magnetic observatories in all quarters of the globe, and of the Antarctic expedition of Sir James Ross, who, since the foundation of the Association, planted the dipping needle over the northern Magnetic Pole, and carried it within 200 miles of the southern one.

[1] Major-General Strachey, in a lecture delivered before the Royal Geographical Society (*Proceedings*, vol. xxxi. p. 179, 1877), discusses, with just appreciation and admirable clearness, the interdependence of the sciences which enter into the study and aims of scientific geography, and which he enumerates under fourteen heads. This lecture contains the ablest review of the subject known to me. It might very well be entitled "The whole duty of the Geographer." Every traveller's outfit should include a copy of it, and one should accompany every prize given by the Geographical Society to students for proficiency in geographical knowledge.

[2] The subject of an able lecture "On the Magnetism of the Earth," delivered before the Royal Geographical Society by the Hydrographer of the Admiralty (*Proceedings*, vol. xxi. p. 20, 1876).

Nor is the geography of this half century less indebted to physicists, geologists, and naturalists. It is to a most learned traveller, and naturalist, Von Baer, that the conception is due that the westward deflection of all the South Russian rivers is caused by the revolution of the globe on its axis.[1] It was a geologist, Ramsay, who explained the formation of so many lake beds in mountain regions by the gouging action of glaciers. It was a physicist and mountaineer, Tyndal, who discovered those properties of ice upon which the formation and movement of glaciers depend. The greatest of naturalist-voyagers, Darwin, within the same half-century has produced the true theory of coral reefs and atolls, showed the relations between volcanic islands and the rising and sinking of the bottom of the ocean, and proved that along a coast line of 2480 miles the southern part of the continent of South America has been gradually elevated from the sea level to 600 feet above it. Within almost the same period Poulett Scrope and Lyell have revolutionised the theory of the formation of volcanic mountains, showing that these are not the long-taught upheavals of the crust of the earth, but are heaped up deposits from volcanic vents, and they have largely contributed to the abandonment of the venerable theory that mountain chains are sudden up-thrusts. Within the same period, the theory of the great oceans having occupied their present positions on the globe from very early geological times was first propounded by Dana,[2] the companion of Wilkes in his expedition round the world, and is supported by Darwin and by Wallace.

In Meteorology the advance is no less attributable to the labours of voyagers and travellers. The establishment of the Meteorological Office is due to the energy and perseverance of a great navigator, the late Admiral Fitzroy.

Another domain of knowledge that claims the strongest sympathies of the geographer is Anthropology. It is only within the last quarter of a century that the study of man under his physical aspect has been recognised as a distinct branch of science, and represented by a flourishing society, and by annual international congresses.

I must not conclude this notice without a passing tribute to a department of geography that has occupied the attention of too few of its cultivators. I mean that of literary research. Nevertheless, in this too the progress has been great; and I need only mention the publications of the Hakluyt Society, and two works of prodigious learning and the greatest value, "The Book of Marco Polo, the Venetian,"[3] and "A History of Ancient Geography,"[4] to prove to you that one need not to travel to new lands to be a profound and sagacious geographer.

I have asked you to accept the geographical distribution of organic beings as the subject which I have chosen for this address. It is the branch with which I am most familiar; it illustrates extremely well the interdependence of those sciences which the geographer should study, and as I have before observed, its progress has been in the main due to the labours of voyagers and travellers.

In the science of distribution, Botany took the lead. Humboldt, in one of his essays,[5] says that the germ of it is to be found in an idea of Tournefort, developed by Linnæus. Tournefort was a Frenchman of great learning, and, moreover, a great traveller. He was sent by the King of France in 1700 to explore the islands of Greece and mountains of Armenia, in the interests of the Jardin des Plantes, and his published narrative is full of valuable matter on the people, antiquities, and natural productions of the countries he visited. The idea attributed to him by Humboldt,[6] is that in ascending mountains we meet successively with vegetations that represent those of successively higher latitudes; upon which Humboldt observes: "Il ne fallut pas une grande sagacité pour observer que sur les pentes des hautes montagnes de l'Arménie, des végétaux des différentes latitudes se suivent comme les climats superposés l'un sur les autres"; but he goes on to remark, "cette idée de Tournefort developpée par Linné dans deux dissertations intéressantes (Stationes et Coloniæ Plantarum), renferment cependant le germe de la Géographie

[1] Von Baer, "Ueber ein allgemeines Gesetz in der Gestaltung der Flussbetten," *St. Petersb. Bull. Sc.* ii. (1860).

[2] Dana in *American Journal of Science*, ser. 2, vol. iii. p. 352 (1847), and various later publications.

[3] By Colonel Henry Yule, C.B. (ed. 1, 1871; ed. 2, 1875).

[4] By S. H. Bunbury (1879).

[5] "Sur les lois que l'on observe dans le distribution des formes végétales" (Mémoire lu à l'Institut de France, January 29, 1816).

[6] I have been unable to find any such idea expressed in Tournefort's works. Edward Forbes, however, also attributes the idea to Tournefort (Memoirs of the Geology Survey, vol i. p. 351).

Botanique." Tournefort's idea was, however, an advanced one for the age he lived in, and should not be judged by the light of the knowledge of a succeeding century. He had no experience of other latitudes than the few intervening between Paris and the Levant. Humboldt himself did not suspect the whole bearing of the idea on the principles of geographical distribution, and that the parallelism between the floras of mountains and of latitudes was the result of community of descent of the plants composing the floras, not that it was brought about by physical causes. The idea of the early part of the eighteenth century is, when rightly understood, found to be the forerunner of the matured knowledge of the middle of the nineteenth.

The labours of Linnæus, himself a traveller, and whose narratives give him high rank as such, paved the way to a correct study of botanical geography. Before his time little or no attention was paid to the topography of plants, and he was the first to distinguish, to lay down rules, and to supply models for these two important elements in their life-history—namely, their habitats or topographical localisation, and their stations, or the physical nature of their habitats. In his "Stationes Plantarum,"[1] Linnæus defines with precision twenty-four stations characterised by soil, moisture, exposure, climate, &c., which, with comparatively slight modifications and improvements, have been adopted by all subsequent authorities. Nor, indeed, was any marked advance in this subject made, till geological observation and chemical analysis supplemented its shortcomings. In his essay "De coloniis plantarum," published fourteen years after the "Stationes,"[2] he says, "Qui veram cunque et solidam plantarum scientiam aucupatur, patriam ipsarum ac sedem cujusque propriam haud sane ignorabit," and he proceeds to give an outline of the distribution of certain plants on the globe, according to climate, latitude, &c., and to indicate their means of transport by winds, birds, and other agencies. India (meaning the tropics of both worlds) he characterises as the region of palms; the temperate latitudes, of herbaceous plants; the northern, of mosses, algæ, and coniferæ; and America, of ferns;—thus preparing the way for the next great generaliser in the field.[3]

This was the most accomplished and prolific of modern travellers, Humboldt, who made botany a chief pursuit during all his journeys, and who seems, indeed, to have been devoted to it from a very early age. His first work was a botanical one, the "Flora Friburgensis," and we have it on his own authority that three years before its publication, when he was only just of age (in 1790) he communicated to his friend G. Förster, the companion of Cook in his second voyage, a sketch of a geography of plants. It was not, however, till his return from America that his first essay on Botanical Geography[4] appeared, which at once gave him a very high position as a philosophical naturalist. Up to the period of its appearance there had been nothing of the kind to compare with it for the wealth of facts, botanical, meteorological, and hypsometrical, derived from his own observations, from the works of travellers and naturalists, and from personal communication with his contemporaries, all correlated with consummate skill and discussed with that lucidity of exposition of which he was a master. The great feature of this essay is the exactness of the methods employed for estimating the conditions under which species, genera, and families are grouped geographically, and the precision with which they are expressed.

This was succeeded in 1815, and subsequently, by four other essays on the same subject. Of these the most valuable is the "Prolegomena,"[5] in which he dwells at length on the value of numerical data, and explains his "Arithmeticæ botanices," which consists in determining the proportion which the species of certain large families or groups of families bear to the whole number of species composing the floras in advancing from the Equator to the Poles, and in ascending mountains. Some kinds of plants, he says, increase in numbers relatively to others in proceeding from the Equator to the Poles, as ferns, grasses, amentiferous trees, &c.; others decrease, as Rubiaceæ, Malvaceæ, Compositæ, &c.; whilst others still, as Labiatæ, Cruciferæ, &c., find their maximum in temperate regions, and decrease in both directions. He adds that it is only by accurately measuring this decrease or increase that laws can be established, when it is found that these present constant relations to parallels of temperature.[1] Furthermore, he says that in many cases the whole number of plants contained in any given region of the globe may be approximately determined by ascertaining the number of species of such families.

The importance of this method of analysing the vegetation of a country in researches in geographical botany is obvious, for it affords the most instructive method of setting forth the relations that exist between a flora and its geographical position and climatal conditions.

Humboldt's labours on the laws of distribution were not limited to floras, they included man and the lower animals, cultivated and domesticated, as well as native; they may not be works of the greatest originality, but they show remarkable powers of observation and reflection, astonishing industry, conscientious exactitude in the collection of data, and sagacity in the use of them; he is indisputably the founder of this department of geographical science.

No material advance was made towards improving the laws of geographical distribution[2] so long as it was believed that the continents and oceans had experienced no great changes of surface or of climate since the introduction of the existing assemblages of animals and plants. This belief in the comparative stability of the surface was first dispersed by Lyell, who showed that a fauna may be older than the land it inhabits. To this conclusion he was led by the study of the recent and later tertiary molluscs of Sicily, which he found had migrated into that land before its separation from the continent of Italy. Just, he adds, as the plants and animals of the Phlegræan fields had colonised Monte Nuovo since that mountain was thrown up in the sixteenth century; whence, he goes on to say, we are brought to admit the curious result, that the fauna and flora of Val de Noto, and of some other mountain regions of Italy, are of higher antiquity than the country itself, having not only flourished before the lands were raised from the deep, but even before they were deposited beneath the waters.[3] The same idea occurred to Darwin, who, alluding to the very few species of living quadrupeds which are altogether terrestrial in habit, that are common to Asia and America, and to these few being confined to the extreme frozen regions of the North, adds, "We may safely look at this quarter (Behring's Straits), as the line of communication (now interrupted by the steady progress of geological change), by which the elephant, the ox, and the horse entered America, and peopled its wide extent."[4]

The belief in the stability of climatal conditions during the lifetime of the existing assemblages of animals and plants was also dispelled by the discovery, throughout the northern temperate regions of the old and new worlds, of Arctic and boreal plants on all their mountains, and of these fossilised on their lowlands, and which discoveries led to the recognition of the glacial period and glacial ocean.

The first and boldest attempt to press the results of geological and climatal changes into the service of botanical and zoological geography was that of the late Edward Forbes, a naturalist of genius, who, like Tournefort, chose the Levant as the field for his early labours. In the year 1846, Forbes communicated a paper to the Natural History section of this Association, on the distribution of endemic plants, especially those of the British

[1] *Amœnitates Academicæ*, vol. iv. p. 64, 1754.

[2] *Ibid.* vol. viii. p. 1, 1768.

[3] Between the dates of the writings of Linnæus and Humboldt, two notable works on geographical distribution appeared. One by Frid. Stromeyer ("Commentatio inauguralis sistens Historiæ Vegetabilium Geographicæ specimen," Göttingen, 1800), is an excellent syllabus of the points to be attended to in the study of distribution, but without examples; the other is a too general work by Zimmermann, entitled, "Specimen Zoologiæ Geographicæ, Quadrupedum Domicilia et Migrationes sistens," Lugd. Bat. 1777, which he followed by "Geographische Geschichte des Menschen und der allgemein verbreiteten vierfüssigen Thiere, nebst einer hieher gehörigen zoologischen Weltcharte, Leipzig, 1778-1783."

[4] "Essai sur la Géographie des Plantes," par A. de Humboldt et Aimé Bonpland; rédigée par A. de Humboldt, lu à la Classe des Sc. Phys. et Math. de l'Institut Nationale, 17 Nivôse de l'An 13. 1805.

[5] "De Distributione Geographica plantarum secundum Cœli temperiem et altitudinem Montium, Prolegomena." This appeared in quarto in the first volume of the "Nova Genera et Species Plantarum" in 1815, and separately in an octavo form in 1817. Humboldt's other works on geographical distribution are "Notationes ad Geographiam Plantarum spectantes," 1815; "Ansichten der Natur," 1808, and ed. 2, 1827; "Nouvelles Recherches sur les lois que l'on observe dans la Distribution des formes végétales" (1816); and an article with a similar title in the "Dictionnaire des Sciences Naturelles," vol. xviii p. 422, 1820.

[1] Humboldt's isothermal lines and laws of geographical distribution are obviously the twin results of the same researches, one physical, the other biological.

[2] I do not hereby imply that no progress was made in the knowledge of the facts of distribution, for, over and above many treatises on the distribution of the plants of local floras, there appeared, in 1816, Schouw's "Dissertatio de sedibus plantarum originariis"; which was followed in 1822 by his excellent "Grundtrack til el almendelig Plante-Geographie," of which the German edition is entitled, "Grundzüge einer allgemeinen Pflanzengeographie."

[3] "Principles of Geology," ed. 3, vol. iii. p. 376, 1834.

[4] *Journal of Researches in Geology and Natural History, &c.*, p. 151 1839.

Islands, considered with regard to geological changes.[1] In this paper the British flora is considered to consist of assemblages of plants from five distinct sources, which, with the exception of one, immigrated during periods when the British Isles were united to the continent of Europe, and have remained more or less localised in England, in Scotland, or in Ireland. Of these he considered the Pyrenean assemblage, which is confined to the west of Ireland, to be the oldest, and to have immigrated, after the eocene period, along a chain of now submerged mountains, that extended across the Atlantic from Spain to Ireland, and indeed formed the eastern boundary of an imaginary continent of miocene age, which extended to the Azores Islands, and beyond them. This, the "Atlantis" of speculative geologists, has long since been abandoned. The second assemblage is of plants characteristic of the South-West of France, which now prevail in Devon, Cornwall, and the Channel Islands; their immigration he assigns to a miocene date, probably corresponding to the red crag. The third assemblage is of plants of the North-East of France, which abound in the chalk districts of the South-Eastern counties of England; their immigration is referred to the era of the mammaliferous crag. The fourth is of Alpine plants now found on the mountains of Scotland, Wales, and England; these were introduced mainly by floating ice from Scandinavia during the glacial period, when the greater part of the British Isles were submerged, its mountain tops forming part of a chain of islands in the glacial sea that extended to the coast of Norway; this was during the newer pliocene period. Lastly, the Germanic plants were introduced during the upheaval of the British Islands from the glacial ocean, and as the temperature was gradually increasing; these are spread over the whole islands, though more abundant on the Eastern side. At the commencement of this immigration England was supposed to be continuous with the Germanic plains, from which it was subsequently severed by the formation of the English Channel. Also, at the commencement of this immigration, Ireland was assumed to be continuous with England, to be early severed by the formation of the Irish Sea; which severance, by interrupting the migration of Germanic types, accounts for the absence of so many British animals in the sister island.

I have thus briefly related Forbes' views, to show how profoundly he was impressed with the belief that geographical and climatal conditions were the all-powerful controllers of the migrations of animals and plants. Forbes was the reformer of the science of geographical distribution.[2]

Before the publication of the doctrine of the origin of species by variation and natural selection, all reasoning on their distribution was in subordination to the idea that these were permanent and special creations; just as, before it was shown that species were often older than the islands and mountains they inhabited, naturalists had to make their theories accord with the idea that all migration took place under existing conditions of land and sea. Hitherto the modes of dispersion of species, genera, and families had been traced; but the origin of representative species, genera, and families remained an enigma[3]; these could be explained only by the supposition that the localities where they occurred presented conditions so similar that they favoured the creation of similar organisms, which failed to account for representation occurring in the far more numerous cases where there is no discoverable similarity of physical conditions, and of their not occurring in places where the conditions are similar. Now under the theory of modification of species after migration and isolation, their representation in distant localities is only a question

[1] *British Association Reports*, 1845. pt. ii. p. 67, and *Annals and Magazine of Natural History*, vol. xvi. p. 126. This the author followed by a much fuller exposition of the subject, which appeared in the *Memoirs of the Geological Survey o' the United Kingdom*, vol. i. p. 336 (1846), entitled "On the Connection between the distribution of the existing flora and fauna of the British Isles, and the geological changes which have affected their area, especially during the epoch of the northern drift." After many years interval I have re-read this Memoir with increased pleasure and profit. The stores of exact information which he collected concerning the plants, the animals, and the geology of Europe and North America, appear to me to be no less remarkable than the skill with which he correlated them and educed from the whole so many very original and in great part incontrovertible conclusions.

[2] I cannot dismiss the subject of the geography of the British flora without an allusion to the labours of Hewett Cottrell Watson, who, after a life devoted to the topography of British plants, was laid in the grave only a month ago. Watson was the first botanist who measured the altitudinal range of each species, and, by a rigidly statistical method, traced their distribution in every county, and grouped them according to their continental affinities, as well as by the physical conditions of their habitats.

[3] The representation of species Forbes alludes to as "an accident . . . which has hitherto not been accounted for" (*Mem.* Geol. Survey, vol. i. p. 351).

of time and changed physical conditions. In fact, as Darwin well sums up, all[1] the leading facts of distribution are clearly explicable under this theory; such as the multiplication of new forms; the importance of barriers in forming and separating zoological and botanical provinces; the concentration of related species in the same area; the linking together under different latitudes of the inhabitants of the plains and mountains, of the forests, marshes, and deserts, and the linking of these with the extinct beings which formerly inhabited the same areas; and the fact of different forms of life occurring in areas having nearly the same physical conditions.

With the establishment of the doctrine of the orderly evolution of species under known laws, I close this list of those recognised principles of the science of geographical distribution which must guide all who enter upon its pursuit. As Humboldt was its founder, and Forbes its reformer, so we must regard Darwin as its latest and greatest lawgiver. With their example, and their conclusions to guide, advance becomes possible whenever discovery opens new paths, or study and reflection retraverse the old ones.

And it was not long before palæontology brought to the surface new data for the study of the present and past physical geography of the globe.

This was the discovery in Arctic latitudes of fossil plants whose existing representatives are to be found only in warm temperate ones. To Arctic travellers and voyagers this discovery is wholly due. Of these I believe I am correct in saying that Sir John Richardson was the earliest, for he, in the year 1848, when descending the McKenzie River to the Polar Sea in search of the Franklin Expedition, found in lat. 65° N. beds of coal, besides shales full of leaves of forest-trees belonging to such genera as the maple, poplar, taxodium, oak, &c. In the narrative of his journey[2] Richardson mentions these fossils, and figures some of them; and in a subsequent work[3] he speaks of them as "leaves of deciduous trees belonging to genera which do not in the present day come so far north on the American continent by ten or twelve degrees of latitude." This discovery was followed, in 1853, by the still more remarkable one, by Capt. M'Clure and Sir Alexander Armstrong (during another search for Sir John Franklin), of pine cones and acorns imbedded in the soil of Banksland, in lat. 75° N., at an elevation of 300 feet above the sea level. And again in 1854 Dr. Lyall found extensive accumulations of similar fossils near Discoe in Greenland (lat. 70° N.), during the return of Sir Edward Belcher's searching expedition. Nor are these fossils confined to America: they have been found in Spitzbergen, in Siberia, and in many other localities within the Polar area as well as south of it, proving that forests of deciduous trees, in all respects like those of the existing forests of the warm temperate regions, approached to within ten degrees of the Pole. The first of these collections critically examined was Dr. Lyall's; it was communicated to Prof. Heer of Zurich, the highest authority on the flora of the Tertiary period, and described by him,[4] as were also subsequently all the other collections brought from the Arctic regions.[5]

The examination of these fossil leaves revealed the wonderful fact that, not only did they belong to genera of trees common to the forests of all the three northern continents, such as planes, beeches, ashes, maples, &c., but that they also included what are now extremely rare and even local genera, as sequoia, liquidamber, magnolia, tulip-trees, gingkos, &c., proving that the forests were of a more mixed character than any now existing. These results opened up a new channel for investigating the problem of distribution, and the first naturalist to enter it as a botanist, Dr. Asa Gray, who pursued it with brilliant results, embodied in a series of memoirs on the vegetation of the United States of America, and of which my notice must be most brief.

When studying the collections of Japanese plants brought by the officers of Wilkes' expedition, Dr. Gray found cumulative evidence of the strong affinity between the flora of Eastern Asia

[1] Of the many pre-Darwinian writers on distribution who advocated the Lamarckian doctrine of evolution, I am not aware of any who suggested that it would explain the existence of representative species, or indeed any other of the phenomena of distribution. Von Baer, however, in the very year of the publication of the first edition of the "Origin of Species," expressed his conviction, chiefly grounded on the laws of geographical distribution, that forms now specifically distinct have descended from a single parent form. See "Origin of Species," ed. 5, Historical Sketch, p. 23

[2] "Boat Voyage through Rupert's Land and in the Arctic Sea," vol i. p. 186

[3] "Polar Regions," p. 289.

[4] "Ueber die von Dr. Lyall in Grönland entdeckten fossilen Pflanzen" *Zürich Vierteljahrschr.* vol vii. p 176 (1862).

[5] "Flora fossilis Arctica."

and Eastern North America, to the exclusion of the western half of that continent; and also that Europe and Western Asia did not share in this affinity. But what especially attracted his attention was, that this affinity did not depend only on a few identical or representative genera, but upon many endemic genera of exceptional character, and often consisting of only two almost identical species. This led to a rigorous comparison of those plants with the fossils from the Arctic regions whose affinities had been determined by Heer, and with others which had been meanwhile accumulating in the United States, and had been described by Lesquereux; and the result was what I may call an abridged outline history of the flora of North America in its relations to the physical geography of that country, from the Cretaceous to the present time.

The latest researches which have materially advanced our knowledge of the laws of distribution are those of Prof. Blytt of Christiania. His essay on "The Immigration of the Norwegian Flora during alternately Rainy and Dry Periods" has for its object to define and localise the various assemblages of plants of which that flora is composed, and to ascertain their mother-country and the sequence of their introduction. The problem is that of Prof. Forbes, which I have already described to you, only substituting Norway for the British Isles. Both these authors invoke the glacial period to account for the dispersion of Arctic plants, both deal with a rising land, both assume that immigration took place over land; but Prof. Blytt finds another and most powerful controlling agent, in alternating periods of greater moisture and comparative drought, of which the Norwegian peat bogs afford ample proof. These bogs were formed during the rise of the land, as the cold of the glacial period declined. They are found at various heights above the sea in Norway; the most elevated of them are of course the oldest, and contain remains of the earliest immigrants. The lowest are the newest, and contain remains of the latest introduced plants only. The proofs of the alternating wet and dry seasons rest on the fact that the different layers of peat in each bog present widely different characters, contain the remains of different assemblages of plants, and these characters recur in the same order in all the bogs. First there is a layer of wet spongy peat, with the remains of bog-mosses and aquatic plants; this gradually passes upwards into a layer of dry soil containing the remains of many land plants, and prostrate trunks of trees, showing that the country was forested. To this succeeds wet spongy peat as before, to be again covered with dry peaty soil and tree trunks, &c., and so on. From an examination of the plant remains in these formations Prof. Blytt draws the following conclusions:—

The Norwegian flora began with an immigration of Arctic plants during a dry period, evidence of which he finds in the presence of the remains of these beneath the lowest layer of peat. As the climate became warmer and the land rose, a rainy period set in, accompanied by an immigration of sub-Arctic plants (juniper, mountain ash, aconites, &c.), which to a great extent replaced the Arctic flora, which is impatient of great wet. This was the period of the first peat-bog formation. It was followed by a dry period, during which the bogs gradually dried up; while with the increasing warmth, deciduous trees and their accompanying herbaceous vegetation were introduced. The succeeding rainy season produced a second peat-formation, killing and burying the deciduous trees, the increasing warmth at the same time bringing in the Atlantic flora, characterised by the holly, foxglove, and other plants now confined in Norway to the rainy Atlantic coast. To this succeeded a third period of drought, when the bogs dried up, and pine-forests with their accompanying plants immigrated into Norway, to be in like manner destroyed and buried by bog earth during the next following rainy period; and it was during these last alternations that the subboreal plants now affecting the lowest south-eastern districts of Norway were introduced; and the sub-Atlantic plants, the most southern of all the types which are confined to the extreme south of the country.

It would be premature to regard all Prof. Blytt's recurrent periods as irrefragably established, or his correlations of these with the several floras as fully proved; but there is no doubt, I think, that he has brought forward a *vera causa* to account for the alternation of dry country with wet country plants in Norway, and one that must have both actively promoted the first introduction of these into that country, as also influenced their subsequent localisation. It would strengthen Prof. Blytt's conclusions very much if his alternating periods of rain and drought should be found to harmonise with Mr. Croll's recurrent astronomical periods, and with Mr. Geikie's fluctuations of temperature during the decline of the Glacial epoch: so would also the finding in the bogs of Scotland a repetition of the conditions which obtain in those of Norway; and there are so very many points of resemblance in the physical geography and vegetation of these two countries that I do not doubt a comparison of their peat formations would yield most instructive results.

Thus far all the knowledge we have obtained of the agents controlling geographical distribution have been derived from observations and researches on northern animals and plants, recent and tertiary. Turning now to the southern hemisphere, the phenomena of distribution are much more difficult of explanation. Geographically speaking, there is no Antarctic flora except a few lichens and sea-weeds. The plants called Antarctic,[1] from their analogy with the Arctic, are very few in number, and nowhere cross the 62° of south latitude. They are, in so far as they are endemic, confined to the southern islands of the great southern ocean, and the mountains of South Chili, Australia, Tasmania, and New Zealand; whilst the few non-endemic are species of the nearest continents, or are identical with temperate northern or with sub-Arctic or even Arctic species. Like the Arctic flora, the Antarctic is a very uniform one round the globe, the same species, in many cases, especially the non-endemic, occurring on every island, though there are sometimes thousands of miles of ocean between the nearest of these. And, as many of the island plants reappear on the mountains above mentioned, far to the north of their island homes, it is inferred on these grounds, as well as on astronomical and geological, that there was a glacial period in the southern temperate zone as well as in the northern.

The south temperate flora is a fourfold one. South America, South Africa, Australia, and New Zealand contain each an assemblage of plants differing more by far amongst themselves than do the floras of Europe, North Asia, and North America; they contain, in fact, few species in common, except the Antarctic ones that inhabit their mountains. These south temperate plants have their representative species and genera on the mountains of the tropics, each in their own meridian only, and there they meet immigrants from all latitudes of the northern hemisphere. Thus the plants of Fuegia extend northward along the Andes, ascending as they advance. Australian genera reappear on the lofty mountain of Kini-balu in Borneo; New Zealand ones on the mountains of New Caledonia; and the most interesting herbarium ever brought from Central Africa, that of Mr. Joseph Thomson, from the highlands of the lake districts, contains many of the endemic genera, and even species of the Cape of Good Hope. Nor does the northern representation of the south temperate flora cease within the tropics; it extends to the middle north temperate zone; Chilian genera reappearing in Mexico and California; South African in North Africa, in the Canary Islands, and even in Asia Minor[2]; and Australian in the Khasia Mountains of East Bengal, in East China and Japan.

So too there is a representation of genera in the southern temperate continents, feeble numerically compared to what the north presents, but strong in other respects. This is shown by the families of Proteaceæ, Cycadeæ, and Restiaceæ, abounding in South Africa and Australia alone, though not a single species or even genus of these families is common to the two countries; by New Zealand, with a flora differing in almost every element from the Chilian, yet having a few species of both calceolaria and fuchsia, genera otherwise purely American; whilst as regards Australia and New Zealand, it is difficult to say which are the most puzzling, the contrasts or the similarities which their animal and vegetable productions present.

These features of the vegetation of the south temperate and Antarctic regions, though they simulate those of the north temperate and Arctic, may not originate from precisely similar causes. In the absence of such evidence as the fossil animals and plants of the north affords,[3] there is no proof that the Ant-

[1] For accounts of the Antarctic flora see the "Botany" of the Antarctic Expedition of Sir James Ross, where the relations of the floras of the southern hemisphere with the Antarctic are discussed in introductory chapters.

[2] *Pelargonium Endlicherianum* in the Taurus is a remarkable instance.

[3] The only fossil leaves hitherto found in higher southern latitudes are those of beeches, closely allied to existing southern species, brought by Darwin from Fuegia. In one locality alone beyond the forest region of the south have fossil plants been found: there were silicified trunks of trees in lava beds of Kerguelen's Island (discovered by myself forty years ago). It is deeply to be regretted that searches for shales containing fossils were not

arctic plants found on the south temperate Alps, or the south temperate plants found in the mountains of the tropics, originated in the south; though this appears probable from the absence in the south of so many of the leading families of plants and animals of the north, no less than from the number of endemic forms the south contains. These considerations have favoured the speculation of the former existence, during a warmer period than the present, of a centre of creation in the Southern Ocean, in the form of either a continent or of an archipelago, from which both the Antarctic and southern endemic forms radiated. I have myself suggested continental or insular extension[1] as a means of aiding that wide dispersion of species over the Southern Ocean, which it is difficult to explain without such intervention; and the discovery of beds of fossil trunks of trees in Kerguelen's Island, testifies to that place having enjoyed a warmer climate than its present one.

The rarity in the existing Archipelago (Kerguelen's Island, the Crozets, and Prince Edward's Island) of any of the endemic genera of the south temperate flora, or of representatives of them, is, however, an argument against such land, if it ever existed, having been the birthplace of that flora; and there are two reasons for adopting the opposite theory, that the southern flora came from the north temperate zone. Of these, one is the number of northern genera and species (which, from their all inhabiting north-east Europe, I have denominated Scandinavian)[2] that are found in all Antarctic and south temperate regions, the majority of them in Fuegia, the flora of which country is, by means of the Andes, in the most direct communication with the northern one. The other is the fact I have stated above, that the several south temperate floras are more intimately related to those of the countries north of them than they are to one another.

And this brings me to the latest propounded theoretical application of the laws of geographical distribution. It is that recently advanced by Mr. Thiselton Dyer, in a lecture "On Plant Distribution as a Field of Geographical Research"[3]; wherein he argues that the floras of all the countries of the globe may be traced back at some time of their history to the northern hemisphere, and that they may be regarded in point of affinity and specialisation as the natural results of the conditions to which they must have been subjected during recent geological times, on continents and islands with the configuration of those of our globe. This hypothesis derives its principal support from the fact that many of the most peculiar endemic plants of the south have representatives in the north, some of them living and all of them in a fossil state, whilst the northern endemic forms have not hitherto been found fossil in the southern regions. So that, given time, evolution, continental continuity, changes of climate and elevations of the land, and all the southern types may be traced back to one region of the globe, and that one palæontology teaches us is the northern.

A very similar view has been held and published at the same time by Count Saporta,[4] a most eminent palæontologist, in a suggestive essay entitled "L'Ancienne Végétation Polaire." Starting from Buffon's thesis, that the cooling of the globe having been a gradual process, and the Polar regions having cooled first, these must have first become fit for organic life, Count Saporta proceeds to assume that the termination of the azoic period coincided with a cooling of the waters to the point at which coagulation of albumen does not take place, when organic life appeared in the water itself. I have discussed Count Saporta's speculations elsewhere[5]; it is sufficient here to indicate the more important ones as bearing upon distribution. These are that the Polar area was the centre of origination of all the successive phases of vegetation that have appeared on the globe, all being developed in the north; and that the development of flowering plants was enormously augmented by the introduction during the latter part of the secondary period of flower-feeding insects, which brought about cross-fertilisation.

It remains to allude briefly to the most important general works on distribution that have appeared since the foundation of this Association. Of these, the two which take the first rank are Prof. Alphonse de Candolle's "Géographie Botanique" and Mr. Wallace's "Geographical Distribution of Animals." Prof. de Candolle's work[1] appeared at a critical period, when the doctrine of evolution with natural selection had only just been announced, and before the great influence of geological and climatal changes on the dispersion of living species had been fully appreciated; nevertheless it is a great and truly philosophical work, replete with important facts, discussed with full knowledge, judgment, and scrupulous caution. Of its numerous valuable and novel features, two claim particular notice, namely, the chapters on the history of cultivated and introduced plants; and the further development of Humboldt's "Arithmeticæ Botanicæ," by taking into account the sums of temperatures as well as the maxima, minima, and means, in determining the amount of heat required to satisfy all the conditions of a plant's life, at the various periods of its existence, and especially the maturation of its seeds.

Of Mr. Wallace's great work, "The Geographical Distribution of Animals," I cannot speak with sufficient knowledge of the subject, and can only appreciate and echo the high praises accorded to it by zoologists for its scientific treatment of a vast subject.

The "Géographie Botanique" was followed by the late Dr. Grisebach's "Die Vegetation der Erde,"[2] which contains an admirable summary of the vegetation of the different regions of the globe as limited by their physical features, divested of all theoretical considerations.

For the largest treatment in outline of the whole subject of distribution, I must refer to the chapters of Darwin's "Origin of Species" which are devoted to it.

In reference to these and other works, very able and instructive discussions of the principles of geographical distribution are to be found in the presidential addresses delivered before the Linnean Society, in 1869, 1870, and 1872, by the veteran botanist, G. Bentham.

With Mr. Wallace's "Island Life" I must conclude this notice, and very fittingly, for besides presenting an admirable account of the origin and migrations of animals and vegetables in oceanic and continental islands, it contains a complete and comprehensive analysis of those past and present conditions of the globe, astronomical, geological, geographical, and biological, which have been the earlier and later directors and controllers of the ever-warring forces of organic nature. In this work Mr. Wallace independently advocates the view of the northern origin of both the faunas and floras of the world.

I conclude with the hope that I have made the subject of the distribution of organic life on the globe interesting to you as geographers, by showing on the one hand how much it owes its advance to the observations made and materials collected by geographical explorers, and on the other how greatly the student of distribution has, by the use he has made of these observations and materials, advanced the science of physical geography.

SECTION G

MECHANICAL SCIENCE

OPENING ADDRESS BY SIR W. ARMSTRONG, C.B., D.C.L., LL.D., F.R.S., PRESIDENT OF THE SECTION

THE astonishing progress which has been made in the construction and application of machinery during the half century which has elapsed since the nativity of the British Association for the Avancement of Science, is a theme which I might with much complacency adopt in this address, but instead of reviewing the past and exulting in our successes, it will be more profitable to look to the future and to dwell on our failures. It is but justice to say that by growing experience, by increasing facilities of manufacture, and by the exercise of much skill and ingenuity, we have succeeded in multiplying and expanding the applications of our chief motor, the steam-engine, to an extent that would have appeared incredible fifty years ago; but the

made either by the *Challenger* expedition or by the various "transit of Venus expeditions" that have recently visited this interesting island.

[1] "Flora Antarctica," pp. 230, 240. See also Moseley in *Journ.* Linn. Soc. Botany, vol. xv. p. 485, and "Observations on the Botany of Kerguelen's Island," by myself, in the *Philosophical Transactions*, vol. 168, p. 15.

[2] See "Outlines of the Distribution of Arctic Plants," *Transactions* of the Linnean Society, vol. xxiii. p. 257. Read June, 1860.

[3] *Proceedings* of the Royal Geographical Society, vol. xxii. p. 415 (1878)

[4] *Comptes rendus* of the International Congress of Geographical Science, which met in Paris in 1875, but apparently not published till 1877.

[5] Address of the President delivered at the anniversary meeting of the Royal Society of London, November 30, 1878.

[1] Prof. Alph. de Candolle divides his subject into botanical geography and geographical botany; the distinction is obvious and sound, but the two expressions have been so long used and regarded as synonymous, and as embracing both branches, that they cannot now be limited each to one. Perhaps the terms topographical botany and geographical botany would prove more acceptable designations.

[2] Published in 1872. Translated into French under the title of "La Végétation du Globe," by P. de Tchihachef, Paris, 1875.

gratulation inspired by this success is clouded by the reflection that the steam-engine, even in its best form, remains to this day a most wasteful apparatus for converting the energy of heat into motive power. Our predecessors of that period had not the advantage of the knowledge which we possess of the true nature of heat, and the conditions and limits affecting its utilisation. In their time heat was almost universally regarded as a fluid which, under the name of caloric, was supposed to lie dormant in the interstices of matter until forced out by chemical or mechanical means. Although Bacon, Newton, Cavendish, and Boyle all maintained that heat was only internal motion, and although Davy and Rumford not only held that view, but proved its accuracy by experiment, yet the old notion of calorie continued to hold its ground, until in more recent times Joule, Meyer, Codling, and others put an end to all doubt on the subject, and established the all-important fact that heat is a mode of motion having, like any other kind of motion, its exact equivalent in terms of work. By their reasonings and experiments it has been definitely proved that the quantity of heat which raises the temperature of a pound of water 1° Fahrenheit, has a mechanical value equal to lifting 772 lbs. one foot high, and that conversely the descent of that weight from that height is capable of exactly reproducing the heat expended.

The mechanical theory of heat is now universally accepted, although a remnant of the old doctrine is displayed in the continued use of the misleading term "latent heat." According to the new theory, heat is an internal motion of molecules capable of being communicated from the molecules of one body to those of another, the result of the imparted motion being either an increase of temperature, or the performance of work. The work may be either external, as where heat, in expanding a gas, pushes away a resisting body, or it may be internal, as where heat pulls asunder the cohering particles of ice in the process of liquefaction, or it may be partly internal and partly external, as it is in the steam engine, where the first effect of the heat is to separate the particles of water into vapour, and the second to give motion to the piston. Internal as well as external work may be reconverted into heat, but until the reconversion takes place, the heat which did the work does not exist as heat, and it is delusive to call it "latent heat." All heat problems are comprised under the three leading ideas of internal work, external work, and temperature, and no phraseology should be used that conflicts with those ideas.

The modern theory of heat has thrown new light upon the theory of the steam-engine. We now know what is the mechanical value in foot-pounds of the heat evolved in the combustion of one pound of coal. In practice we can determine how much of that heat is transmitted to the water in the boiler, and we are taught how to calculate the quantity which in the process of vaporisation takes the form of internal work. We can determine how much disappears in the engine in the shape of external work, including friction, and the remainder, with the exception of the trifling quantity saved in the feed-water, we know to be lost. Taking a good condensing engine as an example, we may roughly say that, dividing the whole heat energy into ten equal parts, two escape by the chimney, one is lost by radiation and friction, six remain unused when the steam is discharged, and only one is realised in useful work. It may be fully admitted that the greater part of the aggregate loss is inevitable; but are we to suppose that the resources of science, ingenuity, and skill have been exhausted in the attainment of so miserable a result? Nothing but radical changes can be expected to produce any great mitigation of the present monstrous waste, and without presuming to say what measures are practicable and what are not, I will briefly point out the directions in which amelioration is theoretically possible, and shall afterwards advert to the question whether we may hope to evade the difficulties of the steam-engine by resorting to electrical methods of obtaining power.

To begin with the loss which takes place in the application of heat to the boiler; why is it that we have to throw away, at the very outset of our operations, twice as much heat as we succeed in utilising in the engine? The answer is, that in order to force a transmission of heat from the fire to the water in the boiler, a certain excess of temperature over that of the water must exist in the furnace and flues, and the whole of the heat below the required excess must pass away unused, except the trifling portion of it which disappears in the production of draught. Further, that since we cannot avoid admitting the nitrogen of the air along with the oxygen, we have to heat a large volume of neutral gas which has no other effect than to rob the fire. Considering what efforts have been made to facilitate the transmission of the heat by augmenting the evaporative surface, and using thin tubes as flues, it is vain to expect any great result from further perseverance in that direction, and unless a method can be devised of burning the fuel inside instead of outside the apparatus, so as to use the heated gases conjointly with the steam as a working medium in the engine, a remedy appears to be hopeless. We already practise internal combustion in the gas-engine, and it is clear that with gaseous fuel, at all events, we could associate such a mode of combustion with the vaporisation of water. We may even regard a gun as an engine with internally-burnt fuel, and here I may remark that the action of heat in a gun is strictly analogous to that of heat in a steam-engine. In both cases the heat is evolved from chemical combination, and the resulting pressures differ only in degree. The gun is the equivalent of the cylinder, and the shot of the piston, and the diagrams representing the pressure exerted in the two cases bear a close resemblance to each other. While the powder is burning in the gun we have a nearly uniform pressure, just as we have in the cylinder while the steam is entering, and in both cases the uniform pressure is followed by a diminishing pressure, represented by the usual curve of expansion. If in the steam-engine we allowed the piston to be blown out it would act as a projectile, and if in the gun we opposed mechanical resistance to the shot, we might utilise the effect in a quieter form of motive power. But it is a remarkable fact that such is the richness of coal as a store of mechanical energy that a pound of coal, even as used in the steam-engine, produces a dynamic effect about five times greater than a pound of gunpowder burnt in a gun. I cannot, however, on this account encourage the idea that steam may be advantageously substitute for gunpowder in the practice of gunnery.

And now to turn from the fire which is the birthplace of the motive energy, let us follow it in the steam, to the condenser, where most of it finds a premature tomb. From the point at which expansion commences in the cylinder the temperature and pressure of the steam begin to run down, and if we could continue to expand indefinitely, the entire heat would be exhausted, and the energy previously expended in separating the water into steam would be wholly given up in external effect; but this exhaustion would not be complete until the absolute zero of temperature was reached (viz. 461° below the zero of Fahrenheit). I do not mean to say that an ideally perfect engine necessarily involves unlimited expansion, seeing that if instead of discharging the steam at the end of a given expansion, we made the engine itself do work in compressing it, we might, under the conditions of Carnot's reversible cycle, so justly celebrated as the foundation of the theory of the steam-engine, recommence the action with all the unutilised heat in an available form. But an engine upon this principle could only give an amount of useful effect corresponding to the difference between the whole work done by the engine, and that very large portion of it expended in the operation of compression, and this difference viewed in relation to the necessary size of the engine, would be quite insignificant, and would in fact be wholly swallowed up in friction. Carnot did not intend to suggest a real engine, and his hypothesis therefore takes no cognisance of losses incident to the application of an actual fire to an actual boiler. His ideal engine is also supposed to be frictionless, and impervious to heat except at the point where heat has to be transmitted to the water, and there the condition of perfect conduction is assumed. In short an engine which would even approximately conform to the conditions of Carnot's cycle is an impossibility, and a perfect steam-engine is alike a phantom whether it be sought for in the cyclical process of Carnot, or under the condition of indefinite expansion. Practically we have to deal with a machine which, like all other machines, is subject to friction, and in expanding the steam we quickly arrive at a point at which the reduced pressure on the piston is so little in excess of the friction of the machine as to render the steam not worth retaining, and at this point we reject it. In figurative language we take the cream off the bowl and throw away the milk. We do save a little by heating the feed water, but this gain is very small in comparison with the whole loss. What happens in the condenser is, that all the remaining energy which has taken the form of internal work is reconverted into heat, but it is heat of so low a grade that we cannot apply it to the vaporisation of water. But although the heat is too low to vaporise water it is not too low to vaporise Ether. If instead of condensing by the external application of water we did so by the similar application of ether, as proposed and practised by

M. du Trembley twenty-five years ago, the ether would be vaporised, and we should be able to start afresh with high tension vapour, which in its turn would be expanded until the frictional limit was again reached. At that point the ether would have to be condensed by the outward application of cold water and pumped back, in the liquid state, to act over again in a similar manner. This method of working was extensively tried in France when introduced by M. du Trembley, and the results were sufficiently encouraging to justify a resumption of the trials at the present time, when they could be made under much more favourable conditions. There was no question as to the economy effected, but in the discussions which took place on the subject it was contended that equally good results might be attained by improved applications of the steam, without resorting to an additional medium. The compound engine of the present day does in fact equal the efficiency of Du Trembley's combined steam- and ether-engine, but there is no reason why the ether apparatus should not confer the same advantage on the modern engine that attended its application to the older form. The objections to its use are purely of a practical nature, and might very possibly yield to persevering efforts at removal.

I need scarcely notice the advantage to be derived from increasing the initial pressure of the steam so as to widen the range of expansion by raising the upper limit of temperature instead of reducing the lower one. It must be remembered however that an increase of temperature is attended with the serious drawback of increasing the quantity of heat carried off by the gases from the fire, and also the loss by radiation, so that we have not so much to gain by increase of pressure as is commonly imagined.

But even supposing the steam-engine to be improved to the utmost extent that practical considerations give us reason to hope for, we should still have to adjudge it a wasteful though a valuable servant. Nor does there appear to be any prospect of substituting with advantage any other form of thermodynamic engine, and thus we are led to inquire whether any other kind of energy is likely to serve us better than heat, for motive power.

Most people, especially those who are least competent to judge, look to electricity as the coming panacea for all mechanical deficiency, and certainly the astonishing progress of electricity as applied to telegraphy, and to those marvellous instruments of recent invention which the British Post Office claims to include in its monopoly of the electric telegraph, as well as the wonderful advance which electricity has made as an illuminating agent, does tend to impress us with faith in its future greatness in the realm of motive power as well.

The difference between heat and electricity in their modes of mechanical action is very wide. Heat acts by expansion of volume which we know to be a necessarily wasteful principle, while electricity operates by attraction and repulsion, and thus produces motion in a manner which is subject to no greater loss of effect than attends the motive action of gravity as exemplified in the ponderable application of falling water in hydraulic machines. If then we could produce electricity with the same facility and economy as heat, the gain would be enormous, but this, as yet at least, we cannot do. At present by far the cheapest method of generating electricity is by the dynamic process. Instead of beginning with electricity to produce power, we begin with power to produce electricity. As a secondary motor an electric engine may, and assuredly will, play an important part in future applications of power, but our present inquiry relates to a primary, and not a secondary, employment of electricity. Thus we are brought to the question, From what source, other than mechanical action, can we hope to obtain a supply of electricity sufficiently cheap and abundant to enable it to take the place of heat as a motive energy? It is commonly said that we know so little of the nature of electricity that it is impossible to set bounds to the means of obtaining it; but ignorance is at least as liable to mislead in the direction of exaggerated expectation as in that of incredulity. It may be freely admitted that the nature of electricity is much less understood than that of heat, but we know that the two are very nearly allied. The doctrine that heat consists of internal motion of molecules may be accepted with almost absolute certainty of its truth. The old idea of heat being a separate entity is no longer held except by those who prefer the fallacious evidence of their senses to the demonstrations of science. So also the old idea of electricity having a separate existence from tangible matter must be discarded, and we are justified in concluding that it is merely a strained or tensional condition of the molecules of matter. Although electricity is more prone to pass into heat than heat into electricity, yet we know that they are mutually convertible. In short I need scarcely remind you, that according to that magnificent generalisation of modern times, so pregnant with great consequences, and for which we are indebted to many illustrious investigators, we now know that heat, electricity, and mechanical action, are all equivalent and transposable forms of energy, of which motion is the essence.

To take a cursory view of our available sources of energy, we have, firstly, the direct heating power of the sun's rays, which as yet we have not succeeded in applying to motive purposes. Secondly we have water power, wind power, and tidal power, all depending upon influences lying outside of our planet. And thirdly we have chemical attraction or affinity. Beyond these there is nothing worth naming. Of the radiant heat of the sun I shall have to speak hereafter, and bearing in mind that we are in search of electricity as a cause, and not an effect, of motive power we may pass over the dynamical agencies comprised under the second head, and direct our attention to chemical affinity as the sole remaining source of energy available for our purpose. At present we derive motive power from chemical attraction through the medium of heat only, and the question is, can we with advantage draw upon the same source through the medium of electricity. The process by which we obtain our supply of heat from the exercise of affinity is that of combustion, in which the substances used consist, on the one hand, of those we call fuel, of which coal is the most important, and on the other, of oxygen, which we derive from the atmosphere. The oxygen has an immense advantage over every other available substance in being omnipresent and costless. The only money value involved is that of the fuel, and in using coal we employ the cheapest oxidisable substance to be found in nature. Moreover the weight of coal used in the combination is only about one-third of the weight of oxygen, so that we only pay upon one-fourth of the whole material consumed. Thus we have conditions of the most favourable description for the production of energy, in the form of heat, and if we could only use the affinities of the same substances with equal facility to evolve electric energy instead of heat energy, there would be nothing more to desire; but as yet there is no appearance of our being able to do this. According to our present practice we consume zinc, instead of coal, in the voltaic production of electricity, and not only is zinc thirty or forty times dearer than coal, but it requires to be used in about six-fold larger quantity in order to develop an equal amount of energy. Some people are bold enough to say that with our present imperfect knowledge of electricity we have no right to condemn all plentiful substances, other than coal, as impracticable substitutes for metallic zinc, but it is manifest that we cannot get energy from affinity, where affinity has already been satisfied. The numerous bodies which constitute the mass of our globe, and which we call earths, are bodies in this inert condition. They have already, by the union of the two elements composing them, evolved the energy due to combination, and that energy has ages ago been dissipated in space in the form of heat, never again to be available to us. As well might we try to make fire with ashes, as to use such bodies over again as sources of either heat or electricity. To make them fit for our purpose we should first have to annul their state of combination, and this would require the expenditure of more energy upon them than we could derive from their recombination. Water, being oxidised hydrogen, must be placed in the same category as the earths. In short the only abundant substances in nature possessing strong unsatisfied affinities are those of organic origin, and in the absence of coal, which is the accumulated product of a past vegetation, our supply of such substances would be insignificant. This being the case, until a means be found of making the combination of coal with oxygen directly available for the development of electric energy, as it now is of heat energy, there seems to be no probability of our obtaining electricity from chemical action at such a cost as to supplant heat as a motive agent.

But while still looking to heat as the fountain-head of our power, we may very possibly learn to transmute it, economically, into the more available form of electricity. One method of transformation we already possess, and we have every reason to believe there are others yet to be discovered. We know that when dissimilar metals are joined at opposite ends, and heated at one set of junctions while they are cooled at the other, part of the heat applied disappears in the process, and assumes the form of an electric current. Each couple of metals may be treated as

the cell of a voltaic battery, and we may multiply them to any extent, and group them in series or in parallels, with the same results as are obtained by similar combinations of voltaic cells. The electricity so produced we term Thermo-electricity, and the apparatus by which the current is evolved is the thermo-electric battery. At present this apparatus is even more wasteful of heat than the steam-engine, but considering the very recent origin of this branch of electrical science, and our extremely imperfect knowledge of the actions involved, we may reasonably regard the present thermo-electric battery as the infant condition of a discovery, which, if it follow the rule of all previous discoveries in electricity, only requires time to develop into great practical importance. Now if we possessed an efficient apparatus of this description we could at once apply it to the steam-engine for the purpose of converting into electric energy the heat which now escapes with the rejected steam, and the gases from the fire. The vice of the steam-engine lies in its inability to utilise heat of comparatively low grade, but if we could use up the leavings of the steam-engine by a supplemental machine acting on thermo-electric principles, the present excessive waste would be avoided. We may even anticipate that in the distant future a thermo-electric engine may not only be used as an auxiliary, but in complete substitution of the steam-engine. Such an expectation certainly seems to be countenanced by what we may observe in animated nature. An animal is a living machine dependent upon food both for its formation and its action. That portion of the food which is not used for growth or structural repair, acts strictly as fuel in the production of heat. Part of that heat goes to the maintenance of the animal temperature, and the remainder gives rise to mechanical action. The only analogy between the steam-engine and this living engine is that both are dependent upon the combustion of fuel, the combustion in the one case being extremely slow, and in the other very rapid. In the steam-engine the motion is produced by pressure, but in the animal machine it is effected by muscular contraction. The energy which causes that contraction, if not purely electrical, is so much of that nature that we can produce the same effect by electricity. The conductive system of the nerves is also in harmony with our conception of an electrical arrangement. In fact a description of the animal machine so closely coincides with that of an electrodynamic machine actuated by thermo-electricity, that we may conceive them to be substantially the same thing. At all events, the animal process begins with combustion and ends with electrical action, or something so nearly allied to it as to differ only in kind. And now observe how superior the result is in nature's engine to what it is in ours. Nature only uses heat of low grade, such as we find wholly unavailable. We reject our steam, as useless, at a temperature that would cook the animal substance, while nature works with a heat so mild as not to hurt the most delicate tissue. And yet, notwithstanding the greater availability of high-grade temperature, the quantity of work performed by the living engine relatively to the fuel consumed, puts the steam-engine to shame. How all this is done in the animal organisation we do not yet understand, but the result points to the attainability of an efficient means of converting low-grade heat into electricity, and in striving after a method of accomplishing that object we shall do well to study nature, and profit by the excellence which is there displayed.

But it is not alone in connection with a better utilisation of the heat of combustion that thermo-electricity bears so important an aspect, for it is only the want of an efficient apparatus for converting heat into electricity, that prevents our using the direct heating action of the sun's rays for motive power. In our climate, it is true, we shall never be able to depend upon sunshine for power, nor need we repine on that account so long as we have the preserved sunbeams which we possess in the condensed and portable form of coal, but in regions more favoured with sun and less provided with coal the case would be different. The actual power of the sun's rays is enormous, being computed to be equal to melting a crust of ice 103 feet thick over the whole earth in a year. Within the tropics it would be a great deal more, but a large deduction would everywhere have to be made for absorption of heat by the atmosphere. Taking all things into account, however, we shall not be far from the truth in assuming the solar heat, in that part of the world, to be capable of melting annually, at the surface of the ground, a layer of ice 85 feet thick. Now let us see what this means in mechanical effect. To melt 1 lb. of ice requires 142·4 English units of heat, which, multiplied by 772, gives us 109,932 foot pounds as the mechanical equivalent of the heat consumed in melting a pound of ice. Hence we find that the solar heat, operating upon an area of one acre, in the tropics, and competent to melt a layer of ice 85 feet thick in a year, would, if fully utilised, exert the amazing power of 4000 horses acting for nearly nine hours every day. In dealing with the sun's energy we could afford to be wasteful. Waste of coal means waste of money and premature exhaustion of coal-beds. But the sun's heat is poured upon the earth in endless profusion—endless at all events in a practical sense, for whatever anxiety we may feel as to the duration of coal, we need have none as to the duration of the sun. We have therefore only to consider whether we can divert to our use so much of the sun's motive energy as will repay the cost of the necessary apparatus, and whenever such an apparatus is forthcoming we may expect to bring into subjection a very considerable proportion of the 4000 invisible horses which science tells us are to be found within every acre of tropical ground.

But whatever may be the future of electricity as a prime mover, either in a dominant or subordinate relation to heat, it is certain to be largely used for mechanical purposes in a secondary capacity, that is to say, as the offspring instead of the parent of motive power. The most distinctive characteristic of electricity is that which we express by the word "current," and this gives it great value in cases where power is required in a transmissible form. The term may be objected to as implying a motion of translation analogous to the flow of a liquid through a pipe, whereas the passage of electricity through a conductor must be regarded as a wave-like action communicated from particle to particle. In the case of a fluid current through a pipe, the resistance to the flow increases as the square of the velocity, while in the case of an electric current the resistance through a given conductor is a constant proportion of the energy transmitted. So far therefore as resistance is concerned electricity has a great advantage over water for the transmission of power. The cost of the conductor will however be a grave consideration where the length is great, because its section must be increased in proportion to the length to keep the resistance the same. It must also be large enough in section to prevent heating, which not only represents loss but impairs conductivity. To work advantageously on this system, a high electromotive force must be used, and this will involve loss by imperfect insulation, increasing in amount with the length of the line. For these reasons there will be a limit to the distance to which electricity may be profitably conveyed, but within that limit there will be wide scope for its employment transmissively. Whenever the time arrives for utilising the power of great waterfalls the transmission of power by electricity will become a system of vast importance. Even now small streams of water inconveniently situated for direct application may, by the adoption of this principle, be brought into useful operation.

For locomotive purposes also we find the dynamo-electric principle to be available, as instanced in the very interesting example presented in Siemens' electric railway, which has already attained that degree of success which generally foreshadows an important future. It forms a combined fixed engine and locomotive system of traction, the fixed engine being the generator of the power and the electric engine representing the locomotive.

Steam power may both be transmitted and distributed, by the intervention of electricity, but it will labour under great disadvantage when thus applied, until a thoroughly effective electric accumulator be provided, capable of giving out electric energy with almost unlimited rapidity. How far the secondary battery of M. Faure will fulfil the necessary conditions remains to be seen, and it is to be hoped that the discussions which may be expected to take place at this meeting of the British Association will enable a just estimate of its capabilities to be formed. The introduction of the Faure battery is at any rate a very important step in electrical progress. It will enable motors of small power, whatever their nature may be, to accomplish, by uninterrupted action, the effect of much larger machines acting for short periods, and by this means the value of very small streams of water will be greatly enhanced. This will be especially the case where the power of the stream is required for electric lighting, which, in summer, when the springs are low, will only be required during the brief hours of darkness, while in winter the longer nights will be met by a more abundant supply of water. Even the fitful power of wind, now so little used, will probably acquire new life when aided by a system which will not only collect, but equalise, the variable and uncertain power exerted by the air.

It would greatly add to the utility of the Faure battery if its weight and size could be considerably reduced, for in that case it might be applicable to many purposes of locomotion. We may easily conceive its becoming available in a lighter form for all sorts of carriages on common roads, thereby saving to a vast extent the labour of horses. Even the nobler animal that strides a bicycle, or the one of fainter courage that prefers the safer seat of a tricycle, may ere long be spared the labour of propulsion, and the time may not be distant when an electric horse, far more amenable to discipline than the living one, may be added to the bounteous gifts which science has bestowed on civilised man.

In conclusion I may observe that we can scarcely sufficiently admire the profound investigations which have revealed to us the strict dynamical relation of heat and electricity to outward mechanical motion. It would be a delicate task to apportion praise amongst those whose labours have contributed, in various degrees, to our present knowledge; but I shall do no injustice in saying that of those who have expounded the modern doctrine of energy, in special relation to mechanical practice, the names of Joule, Clausius, Rankine, and William Thomson, will always be conspicuous. But up to this time our knowledge of energy is almost confined to its inorganic aspect. Of its physiological action we remain in deep ignorance, and as we may expect to derive much valuable guidance from a knowledge of Nature's methods of dealing with energy in her wondrous mechanisms, it is to be hoped that future research will be directed to the elucidation of that branch of science which as yet has not even a name, but which I may provisionally term "Animal Energetics."

THE RISE AND PROGRESS OF PALÆONTOLOGY[1]

THAT application of the sciences of biology and geology which is commonly known as palæontology took its origin in the mind of the first person who, finding something like a shell or a bone naturally imbedded in gravel or in rock, indulged in speculations upon the nature of this thing which he had dug out—this "fossil"—and upon the causes which had brought it into such a position. In this rudimentary form, a high antiquity may safely be ascribed to palæontology, inasmuch as we know that, 500 years before the Christian era, the philosophic doctrines of Xenophanes were influenced by his observations upon the fossil remains exposed in the quarries of Syracuse. From this time forth, not only the philosophers, but the poets, the historians, the geographers of antiquity occasionally refer to fossils; and after the revival of learning lively controversies arose respecting their real nature. But hardly more than two centuries have elapsed since this fundamental problem was first exhaustively treated; it was only in the last century that the archæological value of fossils—their importance, I mean, as records of the history of the earth—was fully recognised; the first adequate investigation of the fossil remains of any large group of vertebrated animals is to be found in Cuvier's "Recherches sur les Ossemens Fossiles," completed in 1822; and, so modern is stratigraphical palæontology, that its founder, William Smith, lived to receive the just recognition of his services by the award of the first Wollaston Medal in 1831.

But, although palæontology is a comparatively youthful scientific speciality, the mass of materials with which it has to deal is already prodigious. In the last fifty years the number of known fossil remains of invertebrated animals has been trebled or quadrupled. The work of interpretation of vertebrate fossils, the foundations of which were so solidly laid by Cuvier, was carried on, with wonderful vigour and success, by Agassiz, in Switzerland, by Von Meyer, in Germany, and last, but not least, by Owen in this country, while, in later years, a multitude of workers have laboured in the same field. In many groups of the animal kingdom the number of fossil forms already known is as great as that of the existing species. In some cases it is much greater; and there are entire orders of animals of the existence of which we should know nothing except for the evidence afforded by fossil remains. With all this it may be safely assumed that, at the present moment, we are not acquainted with a tithe of the fossils which will sooner or later be discovered. If we may judge by the profusion yielded within the last few years by the Tertiary formations of North America, there seems to be no limit to the multitude of Mammalian remains to be expected from that continent, and analogy leads us to expect similar riches in Eastern Asia whenever the Tertiary formations of that region are as carefully explored. Again, we have as yet almost everything to learn respecting the terrestrial population of the Mesozoic epoch—and it seems as if the Western Territories of the United States were about to prove as instructive in regard to this point as they have in respect of Tertiary life. My friend Prof. Marsh informs me that, within two years, remains of more than 160 distinct individuals of mammals, belonging to twenty species and nine genera, have been found in a space not larger than the floor of a good-sized room; while beds of the same age have yielded 300 reptiles, varying in size from a length of 60 feet or 80 feet to the dimensions of a rabbit.

The task which I have set myself to-night is to endeavour to lay before you, as briefly as possible, a sketch of the successive steps by which our present knowledge of the facts of palæontology and of those conclusions from them which are indisputable has been attained; and I beg leave to remind you, at the outset, that in attempting to sketch the progress of a branch of knowledge to which innumerable labours have contributed, my business is rather with generalisations than with details. It is my object to mark the epochs of palæontology, not to recount all the events of its history.

That which I just now called the fundamental problem of palæontology, the question which has to be settled before any other can be profitably discussed, is this,—What is the nature of fossils? Are they, as the healthy common sense of the ancient Greeks appears to have led them to assume without hesitation, the remains of animals and plants? Or are they, as was so generally maintained in the fifteenth, sixteenth, and seventeenth centuries, mere figured stones, portions of mineral matter which have assumed the forms of leaves and shells and bones, just as those portions of mineral matter which we call crystals take on the form of regular geometrical solids? Or, again, are they, as others thought, the products of the germs of animals and of the seeds of plants which have lost their way, as it were, in the bowels of the earth, and have achieved only an imperfect and abortive development? It is easy to sneer at our ancestors for being disposed to reject the first in favour of one or other of the last two hypotheses; but it is much more profitable to try to discover why they, who were really not one whit less sensible persons than our excellent selves, should have been led to entertain views which strike us as absurd. The belief in what is erroneously called spontaneous generation—that is to say, in the development of living matter out of mineral matter, apart from the agency of pre-existing living matter, as an ordinary occurrence at the present day—which is still held by some of us, was universally accepted as an obvious truth by them. They could point to the arborescent forms assumed by hoar-frost and by sundry metallic minerals as evidence of the existence in nature of a "plastic force" competent to enable inorganic matter to assume the form of organised bodies. Then, as every one who is familiar with fossils knows, they present innumerable gradations, from shells and bones which exactly resemble the recent objects, to masses of mere stone which, however accurately they repeat the outward form of the organic body, have nothing else in common with it; and, thence, to mere traces and faint impressions in the continuous substance of the rock. What we now know to be the results of the chemical changes which take place in the course of fossilization, by which mineral is substituted for organic substance, might, in the absence of such knowledge, be fairly interpreted as the expression of a process of development in the opposite direction—from the mineral to the organic. Moreover, in an age when it would have seemed the most absurd of paradoxes to suggest that the general level of the sea is constant, while that of the solid land fluctuates up and down through thousands of feet in a secular ground swell, it may well have appeared far less hazardous to conceive that fossils are sports of nature than to accept the necessary alternative, that all the inland regions and highlands, in the rocks of which marine shells had been found, had once been covered by the ocean. It is not so surprising, therefore, as it may at first seem, that although such men as Leonardo da Vinci and Bernard Palissy took just views of the nature of fossils, the opinion of the majority of their contemporaries set strongly the other way; nor even that error maintained itself long after the scientific grounds of the true interpretation of fossils had been stated, in a manner that left nothing to be desired, in the latter half of the seventeenth century. The person who rendered this good service to palæontology was Nicholas Steno,

[1] Discourse given at the York meeting of the British Association by Prof. T. H. Huxley, Sec. R. S. Revised by the author.

professor of anatomy in Florence, though a Dane by birth. Collectors of fossils at that day were familiar with certain bodies termed "glossopetræ," and speculation was rife as to their nature. In the first half of the seventeenth century, Fabio Colonna had tried to convince his colleagues of the famous Accademia dei Lincei that the glossopetræ were merely fossil sharks' teeth, but his arguments made no impression. Fifty years later Steno re-opened the question, and, by dissecting the head of a shark and pointing out the very exact correspondence of its teeth with the glossopetræ, left no rational doubt as to the origin of the latter. Thus far, the work of Steno went little further than that of Colonna, but it fortunately occurred to him to think out the whole subject of the interpretation of fossils, and the results of his meditations was the publication, in 1669, of a little treatise with the very quaint title of "De Solido intra Solidum naturaliter contento." The general course of Steno's argument may be stated in a few words. Fossils are solid bodies which by some natural process have come to be contained within other solid bodies—namely, the rocks in which they are imbedded; and the fundamental problem of palæontology, stated generally, is this—"Given a body endowed with a certain shape and produced in accordance with natural laws, to find in that body itself the evidence of the place and manner of its production."[1] The only way of solving this problem is by the application of the axiom that "like effects imply like causes," or as Steno puts it, in reference to this particular case, that "bodies which are altogether similar have been produced in the same way."[2] Hence, since the glossopetræ are altogether similar to sharks' teeth, they must have been produced by shark-like fishes; and since many fossil shells correspond, down to the minutest details of structure, with the shells of existing marine or freshwater animals, they must have been produced by similar animals; and the like reasoning is applied by Steno to the fossil bones of vertebrated animals, whether aquatic or terrestrial. To the obvious objection that many fossils are not altogether similar to their living analogues, differing in substance while agreeing in form, or being mere hollows or impressions, the surfaces of which are figured in the same way as those of animal or vegetable organisms, Steno replies by pointing out the changes which take place in organic remains imbedded in the earth, and how their solid substance may be dissolved away entirely, or replaced by mineral matter, until nothing is left of the original but a cast, an impression, or a mere trace of its contours. The principles of investigation thus excellently stated and illustrated by Steno in 1669, are those which have, consciously, or unconsciously, guided the researches of palæontologists ever since. Even that feat of palæontology which has so powerfully impressed the popular imagination, the reconstruction of an extinct animal from a tooth or a bone, is based upon the simplest imaginable application of the logic of Steno. A moment's consideration will show, in fact, that Steno's conclusion that the glossopetræ are sharks' teeth implies the reconstruction of an animal from its tooth. It is equivalent to the assertion that the animal of which the glossopetræ are relics had the form and organisation of a shark; that it had a skull, a vertebral column, and limbs similar to those which are characteristic of this group of fishes; that its heart, gills, and intestines presented the peculiarities which those of all sharks exhibit; nay, even that any hard parts which its integument contained were of a totally different character from the scales of ordinary fishes. These conclusions are as certain as any based upon probable reasonings can be. And they are so, simply because a very large experience justifies us in believing that teeth of this particular form and structure are invariably associated with the peculiar organisation of sharks, and are never found in connection with other organisms. Why this should be we are not at present in a position even to imagine; we must take the fact as an empirical law of animal morphology, the reason of which may possibly be one day found in the history of the evolution of the shark tribe, but for which it is hopeless to seek for an explanation in ordinary physiological reasonings. Every one practically acquainted with palæontology is aware that it is not every tooth nor every bone which enables us to form a judgment of the character of the animal to which it belonged, and that it is possible to possess many teeth, and even a large portion of the skeleton of an extinct animal, and yet be unable to reconstruct its skull or its limbs. It is only when the tooth or bone presents peculiarities which we know by previous experience to be characteristic of a certain group that we can safely predict that the fossil belonged to an animal of the same group. Any one who finds a cow's grinder may be perfectly sure that it belonged to an animal which had two complete toes on each foot, and ruminated; any one who finds a horse's grinder may be as sure that it had one complete toe on each foot and did not ruminate; but, if ruminants and horses were extinct animals of which nothing but the grinders had ever been discovered, no amount of physiological reasoning could have enabled us to reconstruct either animal, still less to have divined the wide differences between the two. Cuvier, in the "Discours sur les Révolutions de la Surface du Globe," strangely credits himself, and has ever since been credited by others, with the invention of a new method of palæontological research. But if you will turn to the "Recherches sur les Ossemens Fossiles" and watch Cuvier, not speculating, but working, you will find that his method is neither more nor less than that of Steno. If he was able to make his famous prophecy from the jaw which lay upon the surface of a block of stone to the pelvis of the same animal which lay hidden in it, it was not because either he, or any one else, knew, or knows, why a certain form of jaw is, as a rule, constantly accompanied by the presence of marsupial bones—but simply because experience has shown that these two structures are co-ordinated.

The settlement of the nature of fossils led at once to the next advance of palæontology—viz., its application to the deciphering of the history of the earth. When it was admitted that fossils are remains of animals and plants, it followed that, in so far as they resemble terrestrial or freshwater animals and plants, they are evidences of the existence of land or fresh water, and in so far as they resemble marine organisms, they are evidences of the existence of the sea at the time at which they were parts of actually living animals and plants. Moreover, in the absence of evidence to the contrary, it must be admitted that the terrestrial or the marine organisms implied the existence of land or sea at the place in which they were found while they were ye living. In fact, such conclusions were immediately drawn by everybody, from the time of Xenophanes downwards, who believed that fossils were really organic remains. Steno discusses their value as evidence of repeated alteration of marine and terrestrial conditions upon the soil of Tuscany in a manner worthy of a modern geologist. The speculations of De Maillet in the beginning of the eighteenth century turn upon fossils, and Buffon follows him very closely in those two remarkable works, the "Théorie de la Terre" and the "Époques de la Nature," with which he commenced and ended his career as a naturalist.

The opening sentences of the "Époques de la Nature" show us how fully Buffon recognised the analogy of geological with archæological inquiries. "As in civil history we consult deeds, seek for coins, or decipher antique inscriptions in order to determine the epochs of human revolutions and fix the date of moral events; so, in natural history, we must search the archives of the world, recover old monuments from the bowels of the earth, collect their fragmentary remains, and gather into one body of evidence all the signs of physical change which may enable us to look back upon the different ages of nature. It is our only means of fixing some points in the immensity of space and of setting a certain number of waymarks along the eternal path of time."

Buffon enumerates five classes of these monuments of the past history of the earth, and they are all facts of palæontology. In the first place, he says, shells and other marine productions are found all over the surface and in the interior of the dry land; and all calcareous rocks are made up of their remains. Secondly, a great many of these shells which are found in Europe are not now to be met with in the adjacent seas; and, in the slates and other deep-seated deposits, there are remains of fishes and of plants of which no species now exist in our latitudes, and which are either extinct or exist only in more northern climates. Thirdly, in Siberia and in other northern regions of Europe and of Asia, bones and teeth of ele hants, rhinoceroses, and hippopotamuses occur in such numbers that these animals must once have lived and multiplied in those regions, although at the present day they are confined to southern climates. The deposits in which these remains are found are superficial, while those which contain shells and other marine remains lie much deeper. Fourthly, tusks and bones of elephants and hippopotamuses are found not only in the northern regions of the

[1] "De Solido intra Solidum," p 5.—"Dato corpore certâ figurâ prædito et juxta leges naturæ producto, in ipso corpore argumenta invenire locum et modum productionis detegentia."

[2] "Corpora sibi invicem omnino similia simili etiam modo producta sunt."

old world, but also in those of the new world, although, at present, neither elephants nor hippopotamuses occur in America. Fifthly, in the middle of the continents, in regions most remote from the sea, we find an infinite number of shells, of which the most part belong to animals of those kinds which still exist in southern seas, but of which many others have no living analogues; so that these species appear to be lost, destroyed by some unknown cause. It is needless to inquire how far these statements are strictly accurate; they are sufficiently so to justify Buffon's conclusions that the dry land was once beneath the sea; that the formation of the fossiliferous rocks must have occupied a vastly greater lapse of time than that traditionally ascribed to the age of the earth; that fossil remains indicate different climatal conditions to have obtained in former times, and especially that the polar regions were once warmer; that many species of animals and plants have become extinct; and that geological change has had something to do with geographical distribution.

But these propositions almost constitute the framework of palæontology. In order to complete it but one addition was needed, and that was made, in the last years of the eighteenth century, by William Smith, whose work comes so near our own times that many living men may have been personally acquainted with him. This modest land surveyor, whose business took him into many parts of England, profited by the peculiarly favourable conditions offered by the arrangement of our secondary strata to make a careful examination and comparison of their fossil contents at different points of the large area over which they extend. The result of his accurate and widely-extended observations was to establish the important truth that each stratum contained certain fossils which are peculiar to it; and that the order in which the strata, characterised by these fossils, are superimposed one upon the other is always the same. This most important generalisation was rapidly verified and extended to all parts of the world accessible to geologists; and, now, it rests upon such an immense mass of observations as to be one of the best established truths of natural science. To the geologist this discovery was of infinite importance, as it enabled him to identify rocks of the same relative age, however their continuity might be interrupted or their composition altered. But to the biologist it had a still deeper meaning, for it demonstrated that, throughout the prodigious duration of time registered by the fossiliferous rocks, the living population of the earth had undergone continual changes, not merely by the extinction of a certain number of the species which at first existed, but by the continual generation of new species, and the no less constant extinction of old ones.

Thus, the broad outlines of palæontology, in so far as it is the common property of both the geologist and the biologist, were marked out at the close of the last century. In tracing its subsequent progress I must confine myself to the province of biology, and, indeed, to the influence of palæontology upon zoological morphology. And I accept this limitation the more willingly as the no less important topic of the bearing of geology and of palæontology upon distribution has been luminously treated in the address of the President of the Geographical Section.

The succession of the species of animals and plants in time being established, the first question which the zoologist or the botanist had to ask himself was, What is the relation of these successive species one to another? And it is a curious circumstance that the most important event in the history of palæontology which immediately succeeded William Smith's generalisation was a discovery which, could it have been rightly appreciated at the time, would have gone far towards suggesting the answer, which was in fact delayed for more than half a century. I refer to Cuvier's investigation of the Mammalian fossils yielded by the quarries in the older Tertiary rocks of Montmartre, among the chief results of which was the bringing to light of two genera of extinct hoofed quadrupeds, the *Anoplotherium* and the *Palæotherium*. The rich materials at Cuvier's disposition enabled him to obtain a full knowledge of the osteology and of the dentition of these two forms, and consequently to compare their structure critically with that of existing hoofed animals. The effect of this comparison was to prove that the *Anoplotherium*, though it presented many points of resemblance with the pigs on the one hand, and with the ruminants on the other, differed from both to such an extent that it could find a place in neither group. In fact, it held, in some respects, an intermediate position, tending to bridge over the interval between these two groups, which in the existing fauna are so distinct. In the same way, the *Palæotherium* tended to connect forms so different as the tapir, the rhinoceros, and the horse. Subsequent investigations have brought to light a variety of facts of the same order, the most curious and striking of which are those which prove the existence, in the mesozoic epoch, of a series of forms intermediate between birds and reptiles—two classes of vertebrate animals which at present appear to be more widely separated than any others. Yet the interval between them is completely filled, in the mesozoic fauna, by birds which have reptilian characters on the one side, and reptiles which have ornithic characters, on the other. So, again, while the group of fishes termed ganoids is at the present time so distinct from that of the dipnoi, or mudfishes, that they have been reckoned as distinct orders, the Devonian strata present us with forms of which it is impossible to say with certainty whether they are dipnoi or whether they are ganoids.

Agassiz's long and elaborate researches upon fossil fishes, published between 1833 and 1842, led him to suggest the existence of another kind of relation between ancient and modern forms of life. He observed that the oldest fishes presented many characters which recall the embryonic conditions of existing fishes; and that, not only among fishes, but in several groups of the invertebrata which have a long palæontological history, the latest forms are more modified, more specialised, than the earlier. The fact that the dentition of the older tertiary ungulate and carnivorous mammals is always complete, noticed by Prof. Owen, illustrated the same generalisation.

Another no less suggestive observation was made by Mr. Darwin, whose personal investigations during the voyage of the *Beagle* led him to remark upon the singular fact, that the fauna which immediately precedes that at present existing in any geographical province of distribution presents the same peculiarities as its successor. Thus, in South America and in Australia, the later tertiary or quaternary fossils show that the fauna which immediately preceded that of the present day was, in the one case, as much characterised by edentates and in the other by marsupials as it is now, although the species of the older are largely different from those of the newer fauna.

However clearly these indications might point in one direction, the question of the exact relation of the successive forms of animal and vegetable life could be satisfactorily settled only in one way—namely, by comparing, stage by stage, the series of forms presented by one and the same type throughout a long space of time. Within the last few years this has been done fully in the case of the horse, less completely in the case of the other principal types of the ungulata and of the carnivora, and all these investigations tend to one general result—namely, that in any given series the successive members of that series present a gradually increasing specialisation of structure. That is to say, if any such mammal at present existing has specially modified and reduced limbs or dentition and complicated brain, its predecessors in time show less and less modification and reduction in limbs and teeth and a less highly developed brain. The labours of Gaudry, Marsh, and Cope furnish abundant illustrations of this law from the marvellous fossil wealth of Pikermi and the vast uninterrupted series of tertiary rocks in the territories of North America.

I will now sum up the results of this sketch of the rise and progress of palæontology. The whole fabric of palæontology is based upon two propositions: the first is, that fossils are the remains of animals and plants; and the second is, that the stratified rocks in which they are found are sedimentary deposits; and each of these propositions is founded upon the same axiom that like effects imply like causes. If there is any cause competent to produce a fossil stem, or shell, or bone, except a living being, then palæontology has no foundation; if the stratification of the rocks is not the effect of such causes as at present produce stratification, we have no means of judging of the duration of past time, or of the order in which the forms of life have succeeded one another. But, if these two propositions are granted, there is no escape, as it appears to me, from three very important conclusions. The first is that living matter has existed upon the earth for a vast length of time, certainly for millions of years. The second is that, during this lapse of time, the forms of living matter have undergone repeated changes, the effect of which has been that the animal and vegetable population at any period of the earth's history contains some species which did not exist at some antecedent period, and others which ceased to exist at some subsequent period. The third is that in the case of many groups of mammals and some of reptiles, in which one type can be followed through a considerable extent of geological time, the series of different forms by which the type is represented at successive intervals of this time is exactly such as it would be if

they had been produced by the gradual modification of the earliest form of the series. These are facts of the history of the earth guaranteed by as good evidence as any facts in civil history.

Hitherto I have kept carefully clear of all the hypotheses to which men have at various times endeavoured to fit the facts of palæontology, or by which they have endeavoured to connect as many of these facts as they happened to be acquainted with. I do not think it would be a profitable employment of our time to discuss conceptions which doubtless have had their justification and even their use, but which are now obviously incompatible with the well-ascertained truths of palæontology. At present these truths leave room for only two hypotheses. The first is that, in the course of the history of the earth, innumerable species of animals and plants have come into existence, independently of one another, innumerable times. This, of course, implies either that spontaneous generation on the most astounding scale, and of animals such as horses and elephants, has been going on, as a natural process, through all the time recorded by the fossiliferous rocks; or it necessitates the belief in innumerable acts of creation repeated innumerable times. The other hypothesis is, that the successive species of animals and plants have arisen, the later by the gradual modification of the earlier. This is the hypothesis of evolution; and the palæontological discoveries of the last decade are so completely in accordance with the requirements of this hypothesis that, if it had not existed, the palæontologist would have had to invent it.

I have always had a certain horror of presuming to set a limit upon the possibilities of things. Therefore, I will not venture to say that it is impossible that the multitudinous species of animals and plants may have been produced one separately from the other by spontaneous generation, nor that it is impossible that they should have been independently originated by an endless succession of miraculous creative acts. But I must confess that both these hypotheses strike me as so astoundingly improbable, so devoid of a shred of either scientific or traditional support, that even if there were no other evidence than that of palæontology in its favour, I should feel compelled to adopt the hypothesis of evolution. Happily, the future of palæontology is independent of all hypothetical considerations. Fifty years hence, whoever undertakes to record the progress of palæontology will note the present time as the epoch in which the law of succession of the forms of the higher animals was determined by the observation of palæontological facts. He will point out that, just as Steno and as Cuvier were enabled from their knowledge of the empirical laws of coexistence of the parts of animals to conclude from a part to the whole, so the knowledge of the law of succession of forms empowered their successors to conclude, from one or two terms of such a succession, to the whole series, and thus to divine the existence of forms of life, of which, perhaps, no trace remains, at epochs of inconceivable remoteness in the past.

NOTES

MOST of the foreign Governments have appointed their delegates to the International Congress of Electricians at Paris. Among the German delegates are M. Wiedemann, editor of *Wiedemann's Annalen*, Helmholtz, Du Bois-Reymond, and Weber, who, as we stated in our last issue, has received a medal in commemoration of the fiftieth anniversary of his professoriate in Halle. The name of Weber is the only one among living men which has been inscribed on the walls of the Palais de l'Industrie. The original instrument which Weber invented with Gauss in 1833 is exhibited in the German section. Amongst the names of English men of science who are said to have been delegated by the English Government are those of Sir William Thomson and Dr. Siemens. One, if not the principal, object of the deliberations of the Congress will be the adoption of a universal system of electric and magnetic measures, as advocated by the British Association. The work of the Commission which has been appointed by it will be discussed, and practical suggestions are to be made relating to it. It is supposed that the electrical and magnetic units are to be considered as a sequel to the metric system of weights and measures. Another question will relate to the laying of submarine cables, viz., the establishment of an international codex of signals for telegraphic steamers, and the necessity of adopting rules for parallel or transversal lines, liable to endanger the existing ones. But it does not appear that any allusion is to be made to the neutralisation in war time, although it has been recommended by M. Barthélemy St. Hilaire, the Minister of Foreign Affairs. All the sittings are to be private, to the exclusion of the public and Press, except a few lectures given by some members on selected topics. *Procès-verbaux* are to be written and published by a select body of authorised secretaries.

THE telephonic audition of the Opera at the Paris Electrical Exhibition is very popular. Not less than 1500 people are admitted by relays of twenty-four, during two minutes at a time, to enjoy it every opera night. It was contemplated to transmit the performances from the Théâtre Français on the same principle, but it has not been successful. The receipts of the Exhibition exceed 4000*l.* daily.

A SIXTH electrical paper has been started in Paris. It is a large folio issued every Saturday, and called *Moniteur officiel de l'Électricité*. It is conducted by M. Barbieny, a gentleman connected with the political Press, and who has founded several periodicals. Electricity has now more papers in Paris than general science.

THE will of the late Sir Josiah Mason of Birmingham ha just been proved. The personal estate was sworn to be of the value of 56,729*l.* The testator had no real property, having in his lifetime disposed of his real estate, worth upwards of 10,000*l.* per annum, either to his orphanage or college trustees, or his great nephew. After legacies and bequests amounting to 7500*l.*, the whole of the testator's personal estate by law applicable to charitable purposes is bequeathed to the trustees of the Mason Science College, for the general purposes of the institution. Elaborate provisions are made for charging the debts, annuities, and legacies on the property which cannot legally be bequeathed to charitable purposes, so as to secure the whole residue for the college.

DR. ARCHIBALD BILLING, M.A., F.R.S., the author of the "First Principles of Medicine," died in London on Friday, at the age of ninety. The deceased physician, who was a native of Ireland, was born in 1791, and was educated at Trinity College, Dublin, and at Oxford, graduating at the first-named University. While engaged at the London Hospital, he instituted the series of chemical lectures which have since become an established feature of the medical school at that institution, but resigned his appointment at the close of 1836, upon the establishment of the University of London. Dr. Billing was a large contributor to the medical Press. He was a member of a large number of learned societies, both in this country and on the Continent.

A CONVENTION of American photographers has recently concluded its sittings at New York. Before separating the members appointed a committee to consider the feasibility of forming an International Photographic Association, and to confer with foreign societies with that view. A report upon the subject will be presented at the next meeting of the Convention, which is to take place at Indianopolis.

THE American Association for the Advancement of Science, at its meeting last month in Cincinnati, took action in reference to the scandal of American degrees, by resolving to unite with the American Philological Association in presenting a memorial to all colleges in the United States empowered to confer degrees, stating the objections to conferring the degree of Ph.D. *honoris causâ*, and praying them to discontinue the practice, if it exists. It seems that the reprehensible practice has been growing of late in the United States. There are, it would seem, in the United

States, 360 institutions of a collegiate grade; these colleges and universities receive their charters from the Legislature of their several States, these charters giving them the unlimited right to confer degrees. The president of the college near Cincinnati told one of the speakers, with a face shining with pride, that his college gave seventeen different degrees. One of these was M.P., which in interpretation meant, not Member of Parliament, but Master of Penmanship. It would seem, moreover, that even the degree of S.D. (equivalent, we believe, to our D.Sc.) has actually been granted by some of these American institutions *honoris causâ*. We trust that the action of the American Association will have some influence with the peccant colleges; it will, at any rate, put people on their guard against American Ph.D.'s and S.D.'s, as well as D.D.'s.

IN the *Revue Scientifique* of September 3, Mr. G. Delaunay has a paper on the "Equality and Inequality of the Two Sexes," in which he endeavours to show that except in some of the lowest forms of animal life, and in the lowest stages of human society, the inferiority of the female sex to the male is unmistakable in all respects—that physically, mentally, and morally, woman is the inferior of man.

A HUGE mass of rock and earth fell the other day from a mountain side at Somnix in the Grisons, blocked up the course of the Jobel, an affluent of the Rhine, and converted the valley into a lake. The village of Surrheim, hard by, is in great danger.

THE additions to the Zoological Society's Gardens during the past week include two Malbrouck Monkeys (*Cercopithecus cynosurus*) from West Africa, presented by Mr. H. P. Sherlock; a Central American Agouti (*Dasyprocta isthmica*) from Central America, presented by Mr. J. E. Sharp; two Spotted Cavys (*Cœlogenys paca*) from South America, presented by Dr. Portella; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. James W. Duncan; two Domestic Pigeons (*Columba œnas*, var.) from Arabia, presented by Mr. Reginald Zohrab; three Common Chamæleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. Alfred R. Rogers; two Greater Sulphur-crested Cockatoos (*Cacatua galerita*) from Australia, deposited; a Black-headed Caique (*Caica melanocephala*) from Demerara, purchased. The additions to the Insectarium for the past week include *Attacus pernyi*, second brood of larvæ hatched; also *Attacus cynthia*, imago second brood, and the Death's-Head Moth (*Acherontia atropos*) larva, presented by Master Kingchurch; second brood of Ant Lions (Myrmeleons), and a brood of the Edible Snail (*Helix pomatia*) from specimens presented by Lord A. Russell, F.Z.S., in April last.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 29.—M. Decaisne in the chair.—M. Faye presented the first volume of his "Cours d'Astronomie de l'École Polytechnique," treating of the diurnal motion, the theory of instruments and errors, organisation of great observatories, mathematics and geodesy. The second volume will be devoted to the solar system.—Dioptric studies, by M. Zenger. He constructs tables which give, in algebraic form, the relation between the radii of curvature and refractive indices of two media forming the objective of a microscope or telescope. Any one may make his own telescope or microscope, without calculation, taking a lens of quartz or crown glass, and a mixture of aromatic substances giving it a dispersion twice as great, or equal for all spectral rays. The lens being corrected, it is combined with one or two other symmetrical lenses, according to the well-known process for getting an aplanetic and achromatic doublet or triplet.—MM. Tresca and Breguet were requested to represent the Academy at the inauguration of the monument to Frederic Sauvage in Boulogne on the 12th inst.—On a very old application of the screw as an organ of propulsion, by M. Govi. This was by Leonardo da Vinci, about the end of the fifteenth century. In one of his works is a sketch of a device for rising in the air, consisting of a helix formed of wire and cloth to be rotated about a vertical axis. He seems to have made small paper models actuated by thin slips of steel, twisted, then left to themselves. Another sketch shows that Leonardo da Vinci conceived the idea of the parachute.—On some new cases of equipotential figures, realised electro-chemically, by M. Guébhard.—On the absorption of ultra-violet rays by some media, by M. de Chardonnet. Two methods are described. The liquids which circulate in plants or impregnate roots and fruits show a great avidity for chemical rays. Fluorescence does not appear to be in direct ratio to the intensity of actinic absorption; thus, *e.g.* the decoction of radish is a less powerful absorbent than that of potatoes; yet the former is fluorescent, the latter not. White wine is weakly fluorescent, red wine lacks the property. The few animal substances studied gave very varied results. While blood, even very dilute, is a strong absorbent, the (fresh) aqueous humour of a calf's eye and the albumen of hen's eggs have no action on the chemical rays (at least up to 20 mm. thickness). Distilled water, alcohol, sulphuric ether, normal collodion, and solution of cane-sugar are also without action. Gelatine appropriates readily all the actinic rays. An object-glass of Dallmeyer projected an invisible spectrum 25 to 40 per cent. longer than one of Darlot, of Paris, of equal focus.—Figures produced by fall of a drop of water holding minium in suspension, by M. Decharme. Minium, in fine powder, is mixed with water and spread uniformly on a horizontal glass plate; then a drop of the mixture is let fall on this layer. Figures resembling those of the three systems Caladni observed on vibrating plates are produced; the three types usually coexist, but one or other may be made to predominate at will.—On the composition of buck-wheat, by M. Lechartier. Marked differences appear between the crops of 1879 and of 1880. Thus the ashes of the straw in 1880 had twice as much potash as in 1879, and phosphoric acid was still more increased; and there was also more chlorine. The composition of the grain is little modified. The straw may contain more of mineral matter than the grain. Buck-wheat removes more of the fertilising principles from the soil than corn.—On hydrosulphurous acid; reply to M. Schutzenberger's note, by M. Bernthsen.—On the dissolution of silver in presence of alkaline iodides, by M. Ditte.—On the constitution of glyceric ether, and on the transformation of epichlorhydrine into normal propylic alcohol, by M. Silva.—On pyruvic alcohol and its derivatives, by M. Henry.—Action of triethylamine on epichlorhydrine; compounds of oxallyltriethylammonium, by M. Reboul.—Biological evolution of the pucerons of the alder tree, by M. Lichtenstein.—Observations on a new enunciation of the second law of Gay-Lussac concerning combinations of gas, by M. Garcia de la Cruz. He indicates some of the numerous exceptions to M. Verschaffel's proposition: "The space occupied by a gaseous compound is always double the space occupied by that one of the components which enters with less volume into the combination." This law he regards as less general than the laws of contraction long accepted.

CONTENTS

THURSDAY, SEPTEMBER 15, 1881

THE THEORY OF DESCENT

Studies in the Theory of Descent. By Dr. Aug. Weismann, Professor in the University of Freiburg. Translated and Edited by Raphael Meldola, F.C.S. Part II. On the Origin of the Markings of Caterpillars. On Phyletic Parallelism in Metamorphic Species. With Six Coloured Plates. (London: Sampson Low, Marston, Searle, and Rivington, 1881.)

THE first part of this work, devoted to an examination of the phenomena of seasonal dimorphism in butterflies, was noticed a little more than a year ago (NATURE, vol. xxii. p. 141). We now have a second instalment of much greater bulk, comprising two separate essays. In the first and most important of these Dr. Weismann gives us the results of a detailed study of the changes in the markings of the caterpillars of the Sphingidæ during the course of their growth and development, and enters at great length into the various questions to which the phenomena observed give rise. Accepting the doctrine that the *ontogeny* or development of the individual gives us a more or less accurate notion of the *phylogeny* or course of development of the race, he endeavours with some success to determine the ancestral forms of the various genera of the Sphingidæ by means of the successive changes of form and coloration of the larvæ. The main facts which he has here established are, that all the larvæ are born of a uniform tint—that the first markings are longitudinal lines—that the oblique lines when they exist always appear later, and the ringed or ocellated spots last of all. Great changes of colour also occur in some species, but all the more important changes, whether of colour or marking, only take place after the larvæ have acquired a considerable size. From the whole assemblage of facts in this branch of the inquiry he deduces the following three laws of development:—

"1. The development commences with a state of simplicity, and advances gradually to one of complexity.

"2. New characters first make their appearance in the last stage of the ontogeny.

"3. Such characters then become gradually carried back to the earlier ontogenetic stages, thus displacing the older characters until the latter disappear completely."

These laws are liable to be modified in various ways by the influence of natural selection, and especially by the need for protection, whence arise the various markings of the different groups, and the peculiar divergences often noted in their development at corresponding ages.

Having thus established the general developmental history of the markings of caterpillars, and explained by a few simple principles the chief anomalies they present, Dr. Weismann passes on to the still more interesting inquiry as to the biological value or actual meaning and use of the markings in each individual case. He first shows that colour itself, irrespective of marking, has a distinct biological value, being always either protective or a signal of uneatableness. The Sphinx larvæ when young are almost always green, resembling the leaves of the plants on which they feed and rest. When they get larger, however, they frequently change to brown, and this change is always accompanied by a change in habits, the insect feeding at night, but during the day concealing itself on the ground or amidst dead leaves and branches. This occurs chiefly among the species which feed on low shrubs or herbs and can thus easily descend to the ground to conceal themselves during the day; while those which feed on large trees acquire markings which assimilate them more completely to the foliage or flowers which surround them. The simplest form of marking—longitudinal stripes—is common on all caterpillars which feed on grasses or other plants in which straight *lines* are a prevalent feature, and this style of marking is that which usually appears in the young sphinx larvæ. But as they grow larger diagonal stripes or bands variously tinted or shaded appear, and this style of marking is found to assimilate so well with the oblique veining of the leaves that the caterpillars are very difficult to see when resting among them. This is the case even when the oblique lines are margined with violet or other bright colours, since, however conspicuous these markings may be when the insect is examined in captivity, they are found to blend perfectly with the lights and shadows of the foliage which surround it in its natural habitat. As an example we have the following account of the brilliantly coloured larva of the Death's-Head Moth on one of its natural food-plants:—

"At Cadiz on the hot, sandy shore, *Solanum violaceum* grows to the height of three feet, and on a single plant I often found more than a dozen *Atropos* larvæ resting with the head retracted. It can easily be understood why the lateral stripes are blue when one has seen the South European *Solaneæ*, on which this larva is at home. *Solanum violaceum* is scarcely green: violet tints alternate with brown, green, and yellow over the whole plant, and between these appear the yellow-anthered flowers, and golden yellow berries the size of a greengage. Thus it happens that the numerous thorns, an inch long, between which the caterpillar rests on the stem, pass from violet into shades of blue, red, green, and yellow."

Many of the adult sphinx larvæ however are adorned with ring-spots or eye-spots, and these have been found to serve two distinct purposes. Sometimes they occur on several of the segments, but of slightly different sizes, as in a North American species (*Chærocampa tersa*), the red spots on which imitate the small red flowers of the plant on which it feeds; while in the European *Deilephila hippophaes*, the grey-green larva with orange-red spots so exactly assimilates to the foliage and fruit of the sea-buckthorn on which it feeds, that Dr. Weismann has often shown to people as many as six or eight of the large caterpillars on one buckthorn branch without their being able at once to detect them. In other cases we find very large eye-spots on the fourth or fifth segments only, coupled with the habit of retracting the head and first three segments, so that an appearance is produced of a broad head with two very conspicuous eyes. Whenever the insect is disturbed it thus retracts itself, and it has often been conjectured that the effect is to frighten away its enemies. This Dr. Weismann has proved to be actually the case. On placing the larva of the Elephant Hawk-Moth (*Chærocampa Elpenor*) in a trough used for feeding fowls, a number of sparrows and chaffinches flew down from the neighbouring trees to pick up some stray food.

"One bird soon flew on to the edge of the trough, and

was just about to hop into it when it caught sight of the caterpillar, and stood jerking its head from side to side, but did not venture to enter. Another bird soon came, and behaved in a precisely similar manner; then a third, and a fourth; others settled on the perch over the trough, and a flock of ten or twelve were finally perched around. They all stretched their heads and looked into the trough, but none flew into it."

On removing the caterpillar the birds again assembled, and at once entered the trough to feed. Fowls were also frightened at first, and would draw back just as they were going to peck at the caterpillar. At last, after several had tried, and even made ineffectual attempts to peck, one more courageous than the rest would actually touch it, and after a time, finding nothing disagreeable, the insect would be swallowed. In the genus Deilephila, however, there are uneatable caterpillars, and these have strongly contrasted black and white or yellow spots combined with the habit of fully exposing themselves upon their food plants. Dr. Weismann experimented with two species (*D. galii* and *D. euphorbiæ*) and found that they were refused by birds, though the latter was eaten by lizards. It is to be noted however that the experiments were made with a South European species of lizard, not that of Germany, so that the result has not a direct bearing on the point.

The general conclusion at which Dr. Weismann arrives is, that all the varieties of colour and marking occurring in the Sphingidæ have a distinct biological value, and can in every case be traced to the action of natural selection and correlation of growth.

The next essay is not quite so interesting or important. It is an endeavour to prove, by a distinct line of inquiry, that the markings of the larvæ are not due to a "phyletic vital force" or to general laws of growth and development. The different groups of Sphingidæ are minutely examined and compared in their three stages of larva, pupa, and imago, and it is shown that the changes that occur from species to species in each stage are to a great extent independent of the changes in the other stages. Numerous examples of this want of phyletic parallelism are given, and it is hence argued that the modifications which occur must be due to an adaptation to the special conditions to which the insect is exposed in one or other of its states, not to any innate law of variation and development, which, it is argued, would affect all the stages *pari passu* and produce a "phyletic parallelism" which does not actually exist. The same general facts are shown to prevail, not only among Lepidoptera generally, but among all insects and crustacea—or generally among all organisms which undergo a metamorphosis.

This instalment of the work has been admirably translated and edited by Mr. Meldola, who, in a series of valuable notes and an Appendix, has brought up the information on every branch of the inquiry to the latest date. The six coloured plates of larvæ in their several stages are very well executed, and serve to illustrate the somewhat complex discussion in a clear and effective manner.

ALFRED R. WALLACE

OUR BOOK SHELF

The Wandering Jew. By Moncure Daniel Conway. (London: Chatto and Windus, 1881.)

THIS last volume of Mr. Conway's is a study, not only of the legend of the Wandering Jew, but with it of the large group of analogous myths of undying men who from age to age wander over the earth, or sleep in caverns, or are translated from among men into divine regions, whence however they come back and show themselves still living men. The interest of these stories in the history of philosophy lies in their keeping up men's early ideas of life and death. One of Mr. Conway's purposes in discussing them is to draw attention to their being relics of the primitive period when men were still so far from definitely realising the nature of death, that they had no difficulty in regarding kings, heroes, and prophets as having only departed for a while from among them, to return in a future age to rule and protect their expectant nations. In comparative mythology this group of stories has some importance. They show the beliefs of various races running curiously into one another, as where the Lancashire peasant still hears in the cry of the plover the wail of the Wandering Jew, or in the Harz Mountains his myth has got mixed with that of a grander wanderer, the Wild Huntsman, who courses with his storm-clouds across the sky. The storm-demon whom mythic fancy imagines rushing through the air is often called a *Maccabee*, and Mr. Conway points out why he has this name. It is because of a verse in the Second Book of Maccabees, chap. v., which, by the way, is a good instance of the personal forms taken by the fancy of an excited people: "And then it happened, that through all the city, for the space almost of forty days, there were seen horsemen running in the air, in cloth of gold, and armed with lances, like a band of soldiers." Unfortunately some other etymologies made or quoted by Mr. Conway are not so reasonable as this. When the names of biblical personages, *Herod* and *Ahasuerus*, find their way into European myths, it may not be easy to explain how they got there, but at any rate it is better to leave them alone than to make up imaginary and even impossible German or Scandinavian forms, *Haar-Rote*, *As-Vidar*, to account for their presence. It would be easy to take exception to many of the arguments in this volume, but at any rate there are many interesting points in it.

A Short Sketch of the Geology of Yorkshire. By Charles Bird, B.A. (Univ. Lond.) (London: Simpkin, Marshall, and Co.; Bradford: Thomas Brear, 1881; pp. 187 and Map.)

Geological Map of Yorkshire. By the same Author. (Edinburgh and London: W. and A. K. Johnston; Bradford: T. Brear, 1881.)

IN the preface of this book, written by way of dedication, it is represented to be a "small and cheap volume suitable to the 'general reader' and tourist." It is impossible to say that it is not a useful and interesting one. So much good work has been done on the county, though scattered through very various publications, that a short *résumé* cannot fail to be of value; but there are books and books, and if we measure this by what it might have been, it is poor indeed. It resembles, in fact, geologically speaking, a kind of boulder clay, full of fragments of solid rock, brought from a distance—we will not say to be deposited in mud—but certainly scratched and rubbed in the process. In the beginning of the volume is a list of the surrounding mountains whence the boulders have been derived, but it is not a complete one; and the source of each fragment is not indicated in the body of the text. Its great defect is that it is unstratified; in other words, the extracts are not duly digested, but thrown together without sorting, and with very little alteration; so little indeed that it would not be difficult to trace them to their sources. Thus under the head of "The Carboniferous Period" we have a brief explanation, from a popular lecture, "how from the general mineral character of a rock the circumstances under which it was formed can often be predicated." Then under the head of "Salt water deposits" we have twelve pages on the origin and contents of the Victoria

Cave, which ought surely to belong to the chapter on the "Recent Geological History of Yorkshire," only that the latter happened to be written by one who confined himself to the Holderness drift. Under the head of "The Permian Rocks" there is an exposition of the views of those who would reintroduce the old (not recently suggested) name Poikilitic to include the Trias. It was a pity the author was not acquainted with any recent papers on the series above the Lias, for there are no good boulders in this part of the book. Mr. Hudleston's admirable papers on the Yorkshire Oolites seem to have been written in vain, and there have been modern papers also on the Yorkshire Chalk. It was perhaps excusable for our author to conclude that the third edition of Prof. Phillips' "Yorkshire Coast" contained all the most recent information, though every East-Yorkshire geologist knew that it did not. In examining a work on local geology it is always well to see where the author lived, for the surrounding country will be the best described. So it is here; the best part of the book is the description of the Middle and Upper Coal-measures, which are well developed in the neighbourhood of Bradford. For East Yorkshire and the coast the book is of little value.

The topography of the map requires no other guarantee than the name of the constructors for its excellency. The south-western part of the geological colouring derived from the Geological Survey maps is also very good. Nor can we complain when lack of published material prevents accuracy elsewhere, though it is a reason for regretting the slow publication of the Geological Survey maps which have been long ago completed; but when the whole of the Vale of Pickering is coloured Neocomian, and a patch of the same is placed in the south near Cave, scarcely an acre of rocks of that age being discoverable in the former, and none in the latter locality, one is led still more to regret that the author's map should be spoiled by his not knowing Mr. Hudleston's papers and relying on Prof. Phillips. But he has surely introduced a little mistake of his own, which will be very serious to visitors to the popular watering-places of Scarborough and Filey. The Castle Rock and Filey Brig are coloured—one Lower Oolite and the other Neocomian, whereas they are both what the author would call "Middle Oolite"! It will take more than Mr. Bird to write a good "Geology of Yorkshire."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Leaves Injured at Night by Free Radiation

FRITZ MÜLLER, in a letter to me from Sta. Catharina in Brazil, dated August 9, supports the view which I have advanced with respect to leaves placing themselves in a vertical position at night, during their so-called sleep, in order to escape being chilled and injured by radiation into the open sky. He says: "We have had last week some rather cold nights (2° to 3° C. at sunrise), and these have given me a new confirmation of your view on the meaning of the nyctitropic movements of plants. Near my house there are some Pandanus trees, about a dozen years old; the youngest terminal leaves stand upright, whereas the older ones are bent down so as to expose their upper surfaces to the sky. These young leaves, though of course the most tender, are still as fresh and green as before; on the contrary, the older ones have suffered from the cold, and have become quite yellowish. Again, the leaves of *Oxalis sepium* were observed by me to sleep in a very imperfect manner during the summer, even after the most sunny days; but now, in winter, every leaflet hangs down in a perpendicular position during the whole night." It is a new fact to me that leaves should sleep in a more or less perfect manner at different seasons of the year.

CHARLES DARWIN

Red Rainbows

THE account in NATURE, vol. xxiv. p. 431, of a pink rainbow seen from Mr. Tennyson's house, recalls to me a rainbow which I witnessed in July 1877 over the Lake of Lucerne from the promenade in front of the Schweitzerhof. The bow in question appeared at sunset, when the whole sky, east and west, was lit up with ruddy tints; and just before it faded out, the bow itself, which was a very brilliant one, showed only red and orange colours in place of its usual array of hues. No fewer than five supernumerary arcs were visible at the inner edge of the primary bow, and these showed red only. I fancy that the phenomenon cannot be very rare, from the circumstance that in pictures of the rainbow red and yellow are frequently the only colours set down by the artist. A few months ago Mr. C. Brocke Branwhite of Clifton showed me a very beautiful sketch in oils by his father, the late Mr. Charles Branwhite, a colourist of no mean power, in which a beautiful and exquisitely pellucid rainbow was drawn with red and yellow tints only. It may also be mentioned that in the copy of Rafael's "Madonna di Foligno" in the Dresden Gallery, there is a semi-circular red and yellow rainbow. I have not seen the original Foligno Madonna in Rome; and should be interested to know whether in this also red and yellow are the only tints accorded by the colourist.

Haslemere House, Clifton SILVANUS P. THOMPSON

IN your issue of the 8th inst. (vol. xxiv. p. 431) your correspondent "A. M." describes what he calls a "pink rainbow" seen by him at Aldworth, near Haslemere, and as a painter I am interested in his description, as it exactly corresponds with the same phenomenon as seen by me here, same date, and viewed with curiosity by myself and friends.

Corrie Hotel, Arran, September 11 DAVID MURRAY

Atoms

ALTHOUGH I am not an "eminent" authority, perhaps you will excuse my troubling you with the following extract from a paper read by me before the Philosophical Society of Glasgow in November, 1875, a copy of which paper I posted to the Editor of NATURE:—"I have long been of opinion that the most probable hypothesis of the origin of atoms is that there is only one kind of matter—ether or its constituents—and that atoms are merely congeries of units of ether circling at enormous speeds round each other, differently grouped, in different numbers, at different velocities, and at different distances, even as the different members of our sun systems. . . . The numbers of units in each similar atom need not be always the same; a few dozens more or less will not be appreciable by us. On the other hand, if a so-called element show a plurality of spectroscopic lines or hues, I do not think it at all doubtful that there is a plurality of units moving to produce these, since they thus show effects of different modes of moving of bodies; all our different states of sensual consciousness of colours are necessarily dependent on differences in the modes of moving of the agents that excite in us such plurality of lines and hues. As the motions of atom, or rather of groups of atoms, excite in us sensations of sound, so the motions of units, or rather of groups of units, excite in us sensations of colour, and of course the lower-pitched movements of dark heat. Then again, we may hold that the more lines that persist in a spark or a sun, the less easily reducible are the portions of the elements showing them, as far as these lines' constituents are concerned—the lines being still undissociated material." (*Proc. Phil. Soc. of Glas.*, vol. x. p. 61.) HENRY MUIRHEAD

Cambuslang, August 26

Luminous Phenomena on Rupture of Sea-Ice

IN my diary for January 20, 1881, occurs the following passage. I make no attempt to account for the phenomenon, but am certain it was not caused by any reflection of the lights on board the vessel:—

"Started from Christiania at about 2 a.m. in the *Nyland* steamer bound for Christiansand. At Krujero the steamer forced its way through the ice for half an hour till within about a mile of the land, where sleighs met it on the ice. The passengers and cargo were discharged or taken up on the ice, out of which we backed in close proximity to the *Kong Hacon*, which steamer had followed us in. A beautiful sunset and Arctic winter view, clear air, and rich sky, also a distant ship fast in the ice. The *Nyland* stopped at Arendal for the night, having got to the quay through much ice. We observe often phosphorescence or phosphorescence-like sparks and flashes in the ice as it is broken up by the steamer."

I think that the average thickness of the ice might have been about eight inches. I cannot give the temperature, but on the previous day at Christiania the thermometer indicated about 8° or 10° below zero Fahrenheit (about 40° Fahrenheit of frost). The diary from which the above extract is taken was kept jointly by myself and my travelling-companion, Mr. Winter, of the Indian Civil Service, who of course also saw the flashes referred to. I should like to have been able to talk the matter over again with him, but he is now in India. J. ALLEN ALLEN

[The question raised in this letter is a very interesting one. The phenomenon is possibly analogous to the electric flashes which are produced when loaf-sugar is crushed or when mica is rapidly split. It appears very improbable that it can be due to phosphorescent creatures in the water under the ice.—ED.]

Tidal Currents *versus* Wind Waves

IN NATURE, vol. xxiv. p. 286, a writer on "sea-shore alluvion" positively asserts that the travelling of sea beaches is due to wind-waves, and not to tidal currents, and calls a writer in the *Engineer* to task for having stated the latter. Notwithstanding this assertion, I would suggest that the writer in the *Engineer* is right. Twenty-five years ago, when an engineering student, I was taught that sea-beach travelling was due to wind-waves. Afterwards, while knocking about during fifteen years in the vicinity of the south and west coasts of Ireland, I noted facts that went to show that such a theory was not universally correct. This led me to study wind-action on the sea and lakes, also all I could find that had been written on the subject; the result being that as *good* evidence was so contradictory, no opinion could be come to from the evidence of others. But it was not till about ten years ago, when I was so circumstanced that I could properly study wave-action, and after six years' careful observation on the south-east coast of Ireland, that I found that tidal currents were the principal motive power; and on again reading what had been written on the subject, that I found that nearly all the advocates for the driftage of sea beaches by wind-waves had studied on beaches *where the most continuous and powerful winds acted in conjunction with the flow-tide current*. As the results of my observations have been published in the *Proceedings* of the Royal Irish Academy, English and Irish Institutions of Civil Engineers, the Geological Societies of London, Dublin, &c., during the last six or eight years, it is unnecessary to repeat them here. I would, however, point out that when there are only wind-waves and no tidal currents, the beaches as a general rule are banked up, but do not travel (the writer in NATURE seems to have observed this, but does not appear to see the importance of it). This may be seen in the tideless Mediterranean, as pointed out by the late Dr. Ansted in his paper on the Lagoons at the Delta of the Rhone; it may also be seen in Malcombe, or any other bay where there is a "head of the tide" but no tidal current; and in the different freshwater lakes, when the wind-waves are the *only* motive power. But wherever there are tidal currents acting on a coast the beach must travel. Such tidal currents are those that most perplex the erectors of groynes. If there was only the travelling augmented by wind-waves, the erection of groynes would be very simple; but, as a general rule, they are most necessary where there are strong tidal currents (or conflicting currents) due to the regular "flow" tide, "half counter" tides, or "on-shore" tides; which conflicting currents, combined with the action of wind-waves, let them be direct or as "ground swells," make up all the "cutting-out tides." The greater the complications the greater the "cutting out," and the more ingenious have to be the groynes. "Falls" accumulate with the wind-waves, but rapidly disappear when the wind ceases. I presume the writer of the article in question is aware that the greatest rise of tide and the least current is at the "heads of the tides," while the least rise and greatest current is at the "nodal or hinge lines"; and I would be interested to know where permanent beaches accumulate in the latter localities, as from what I have seen those that form rapidly disappear when the wind ceases. G. H. KINAHAN

H.M. Geological Survey

Glaciation

IN NATURE, vol. xxiv. p. 364, I see a notice of a paper by Dr. Woeikoff on the glacial climate, in which it is shown that "the difference of mean temperature at the lower ends of glaciers (in different parts of the world) reaches fully 20°." This might be expected. The extent of glaciation depends not at all on mean or on winter temperature, but chiefly on summer temperature. *Perpetual* snow means *summer* snow, so that summer temperature is what determines the extent of the snow-fields remaining unmelted in the summer, and consequently of the glaciers which are fed by the snow-fields. The extent of glaciation is also much influenced by the amount of snow-fall. All this is stated in Forbes's "Norway and its Glaciers."

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

Yellow Glass in Fog

SOME years ago I was staying at an hotel on the Lake of Constance. One morning a fog came on which completely obscured the opposite shore, but looking through a strip of yellow glass, which formed the border of the window, I was able, to my surprise, to see it distinctly. I presume the yellow glass choked the blue rays reflected by the fog, just as a Nicol's prism, held at the proper angle, chokes the rays reflected from the glass and enables us to see clearly the picture behind it. On my way home I stopped in Paris, and, happening to call on one of the principal opticians, mentioned the circumstance to him. He forthwith showed me a naval telescope provided with a cap at the eye end containing a yellow glass, which could be removed at pleasure. I should like to know if the same simple contrivance has ever been used in our own navy. R.

The New Museum of Natural History

IN your article on "The New Museum of Natural History" (NATURE, vol. xxiii. p. 549 *et seq.*) it is stated that the specimen of *Archaeopteryx macrura* in the British Museum is headless. Will you permit me to draw attention to a nodule projecting from the slab in which the fossil lies, which bears a striking resemblance to the cerebral portion of a bird's skull? It is some years since I visited the museum, but I recollect feeling satisfied at the time that the nodule was the missing head, and worth while disinterring from its surrounding slate. E. H. PRINGLE

Calicut, July 31

[The nodule referred to by our correspondent is well known, and has been frequently criticised. Mr. John Evans, D.C.L., F.R.S., drew attention to it in an article published by him in the *Natural History Review*, 1865, pp. 415-421: "On portions of a cranium and of a jaw in the slab containing the fossil remains of the *Archaeopteryx*." Although these fragments which occur in the slab in question undoubtedly belong to *Archaeopteryx*, yet, as stated in our article, vol. xxiii. p. 551, "The original specimen described by Prof. Owen is headless," whereas the newly-discovered Berlin specimen has the head entire, and fairly well preserved, and still attached by the neck to the trunk.—ED.]

On the Velocity of Light

IN view of the experiments of Young and Forbes on the velocity of light, and of the article published by Lord Rayleigh on the subject, it may not be out of place to state as a fact which seemed at the time too evident to require special mention in my paper "On the Velocity of Light," that if the velocity of red and of blue light in air differed by as much as one-tenth of 1 per cent., the image of the slit which served as the source of light, instead of being white, would be spread out into a spectrum which could not fail to be observed. The total displacement in these experiments amounted to 133 millimetres; therefore, a difference of velocity of the red and the blue rays of 1·8 per cent. would necessitate a spectrum 2·4 millimetres in length.

It is needless to say that no spectrum was observed. These facts appear to be utterly irreconcilable with the conclusion drawn by Messrs. Young and Forbes.

ALBERT A. MICHELSON

Schluchsee, Prussia, August 28

Salmon in Preserved Rivers

HAVING resided for some time lately near one of our salmon rivers which is at present preserved by a club, I have at different times had conversations with men who knew it before its so-called preservation. They all say that when they were allowed to fish when and how they pleased, the supply of fish was much better in regard both to size and quantity. They account for it in the following manner:—Firstly, when the river was free, the people living near used to make spawning-beds for the fish, by placing large stones across the river and throwing gravel where deficient, and where gravel was naturally they used to loosen it with forks and remove the large stones. Secondly, they used to watch the fish at spawning time, and catch and kill all very large fish, say about 16 to 30 lbs. weight, after they had partially or wholly finished spawning, as they say the large fish destroy the salmon fry. Neither this nor the formation of spawning-beds is done at present. Would the above reasons account for the diminution in the size and number of salmon caught in our rivers? The diminution, in the river I speak of, cannot be accounted for by pollution, as the number of houses near enough to send their drainage into the river is too small to affect it, and as the river has a very quick fall and rocky bed, it is subject to such very rapid rises and falls in quantity of water that would prevent any settlement of noxious sediment.

F. C. S.

New Seismometer

IN NATURE, vol. xxiv. p. 113. there is a notice of a new seismometer which has several advantages claimed for it. Might I suggest what seems an obvious and important improvement? As a rule pendulums cannot record vertical or oblique motions, and yet these are often the most necessary and valuable to record. 1. To do this, and yet as easily allow of lateral registration, I would say, support a heavy (leaden) ball of some 100 lbs. by a 30 or 40 feet spiral or rubber spring of suitable strength. It will be found that a very considerable amount of vertical play can take place, especially vertical effort, ere the ball can be affected, and that lateral play of the support will produce very little effect indeed, unless, as is most unlikely, the motion is prolonged and is *continuous* in one direction. 2. Around the sphere, and at a very short distance from its surface, radial rods actuated like the key-plugs of a cornet are supported, say at every 30° all over the surface, contact with any one of which will electrically record *time*, and the pencil attached to the plunger record distance of stroke on revolving paper attached to plunger-tube.

Asam, July 6

S. E. PEAL

THE BRITISH ASSOCIATION

THE actual number of persons who attended the York Meeting of the British Association, as announced at the last meeting of the General Committee, was 2556; divided between 272 old life-members, 27 new life-members, 312 old annual members, 175 new annual members, 1232 associates, 514 ladies, and 24 foreigners. The seven previous occasions on which this number has been exceeded were:—Newcastle-on-Tyne, 1863 (3335); Manchester, 1861 (3138); Liverpool, 1870 (2878); Bath, 1864 (2802); Glasgow, 1876 (2774); Dublin, 1873 (2578); Aberdeen, 1859 (2564). The number fell below 1000 at Cambridge, Plymouth, Southampton, Ipswich, Hull, and Swansea. 1280*l.* were paid out by the Council for scientific purposes after the last meeting, a larger sum than on any occasion since 1873; while between 1873 and 1861 that sum was always exceeded, and at Norwich, in 1868, it amounted to 1940*l.*

The following foreigners were present at the meeting:—Professrs Barker of Pennsylvania; Bergeron, Paris; Bojanowski; Carbonnelle, Brussels; Chemin, Paris; Craig, Johns Hopkins University, U.S.; Dohrn, Naples; Eads, St. Louis, U.S.; Gariel, Paris; Dr. Asa Gray, Harvard University; Halphen, Paris; Dr. Edwin Hall, Baltimore, U.S.; Hubrecht, Leyden; Prof. W. W. Johnson, Annapolis, U.S.; Prof. O. C. Marsh, Yale College; Moser, Berlin; Prof. H. A. Rowland, Baltimore; Stephanos, Paris; Sturm, Münster, Westphalia; Prof. H. M. Whitney, Beloit College, Wisconsin, U.S.A.

We ought to have stated in our report of the doings of the Association in our last number, that Prof. Huxley's lecture on Palæontology, which we gave in the same number, was delivered on the evening of Friday the 9th.

Nearly 350 papers or reports were read before the several sections. Of these the Physical and Mathematical Section received 89; the Chemical Section 49; Geology 59; Biology 79; Geography 16; Economic Science and Statistics 26; and Mechanical Science 29. Of the papers in Section A 23 related to Electricity; 21 were Mathematical; Optics claimed 12; Meteorology 11; Astronomy and Physical Geography 12; Heat 5; and miscellaneous physical subjects 5. Of course prominent subjects of interest were electric lighting, electric measurements, and Faure's cells. Such subjects were thoroughly ventilated by discussions both in Section and Committee, and more intimately during the thousand and one opportunities for interchange of ideas which occurred in the afternoon and evening. Again, the storage of energy, the nature of meteoric dust, the existence of intra-Mercurial planets, the lunar disturbance of gravity, the nature of colours, and the contact theory were each severally discussed. Among the 49 Chemical papers several theoretical matters were introduced—specially the atomic theory, chemical nomenclature, vapour densities, molecular weights, Mendeleeff's law, and molecular attraction; processes of analysis and technical operations were described, and new experiments were explained. Of course a good deal of the geological work bore reference to Yorkshire, especially to the evidences of glacial action which it presents. The geological papers were of a very general and interesting character, and embraced every branch of the subject, from the vulcanology of Japan to the minerals found at Laurium, and from the Cheshire salt beds to the evolution of the Plesiosaurus. Section D furnished a larger number of papers than any other Section except A, but we must bear in mind that it really consists of three sub-sections, devoted respectively to Zoology and Botany, Anatomy and Physiology, and to Anthropology. The latter subject has developed extraordinarily, more than half the papers contributed to the Section were read before this Sub-Section. The report of the Anthropometric Committee, which evoked a good deal of discussion, was read in the Section of Economic Science and Statistics. In this section Mr. Grant Duff delivered a very able address, which was warmly received. A tendency to introduce matter which has a political bearing and which may be discussed from a political standpoint is sometimes apparent in this section, and should be carefully guarded against by the Committee. The Mechanical Section furnished some important reports on patent laws, wind pressure, tides in the English Channel, and the steering of screw steamers. Here also were papers on the different forms of electric lamp, the electrical transmission of force, and the illumination of lighthouses.

Thus it will be seen that all the prominent subjects of science have received their share of attention, and at the hands of one or other of the sections have been either expanded or discussed. The interchange of ideas has been incessantly going on, and many men have become acquainted who might otherwise have remained unknown to each other for years. Some 500 scientific men have been gathered together from various parts of the British Islands; and some 2000 persons have been brought face to face with the burning scientific questions of the day, and have had new interests awakened, or old knowledge resuscitated. There can be little doubt as to the

expediency of continuing the work of the Association, if it keep at all near to the standard of the York meeting. The German Society, founded nine years before our Association, and its prototype, still continues to meet annually; and scientific congresses are becoming more and more general every year in Europe.

Canada has been proposed as the place of meeting for 1885. The difficulties of time and place and expense are far less formidable than they appear at the outset. Great facilities would be put in our way by steamboat companies; and, once arrived, the Colony would receive us with open arms. Again, the Americans wish us to join their Association on some convenient occasion, and *àpropos* of this a practical American observed a few days since, "From the moment you set foot on American soil to the moment of departure, you should not put down a cent." One other fact remains to be noticed in regard to the York meeting. *Thirty-four* local societies and institutions were represented at the meeting by forty-nine delegates; and the Council have under consideration the conditions under which these delegates were present, and their object in attending. Cannot the Association do something for them? Cannot some organisation be introduced to influence the local societies through the Association, and cannot a committee of delegates be appointed to discuss matters connected with their respective institutions?

REPORTS

Report of the Committee, consisting of Dr. J. H. Gladstone, Dr. W. R. E. Hodgkinson, Mr. W. Carleton Williams, and Dr. P. P. Bedson (secretary), appointed for the purpose of investigating the Method of Determining the Specific Refraction of Solids from their Solutions.—Mr. P. P. Bedson, D.Sc., read the Report, and stated that the object of this report was to submit to further examination the method proposed some years ago by Messrs. Gladstone and Dale. According to this method the specific refraction of a solid may be deduced from that of a solution containing it, provided the specific refraction of the solvent is known, as also that of the solution and the composition of the solution. The experiments, of which an account is given in the report, appear to confirm this statement of the above-mentioned authors. The first case examined was that of liquid phenol. Its specific refraction for a ray of light of infinite wave-length was determined at 40° and 45°. The values obtained for the specific refraction of liquid phenol at 40° and 45°, viz., ·4850 and ·4848, are closely approximate to that obtained by Brühl (*Journ.* Chem. Soc., abst., 1880, p. 782) for phenol at 20°, viz., ·4862. Further, these results agree very well with the mean of the specific refractions obtained from the alcoholic and acetic acids solutions. The specific refraction of rock-salt in the solid state has also been determined and compared, with its specific refraction as deduced from its aqueous solutions; and it was found that the specific refraction obtained from the aqueous solution is substantially the same as that obtained from a prism of rock-salt. Further, the specific refractions of fused borax and boric acid have been determined, and in these cases also the specific refraction obtained from their aqueous solutions was found to be approximately the same as the specific refractions of fused borax and boric acid. The indices of fused borax and of fused boric acid were determined by means of prisms of these materials, which were cast in a mould of silver plates and afterwards ground and polished.

Report of Committee on Meteoric Dust, by Prof. Schuster.—This Committee was appointed for the double purpose of examining the observations hitherto recorded on the subject of meteoric dust and of discussing the possibility of future more systematic investigations. With regard to the first point we note that in a paper presented to the Royal Astronomical Society in 1879, Mr. Ranyard has given what appears to be a pretty complete account of the known observations as to the presence of meteoric dust in the atmosphere. It appears that in the year 1852 Prof. Andrews found native iron in the basalt of the Giant's Causeway. Nordenskjöld found particles of iron which in all probability had a cosmic origin in the snows of Finland and in the ice-fields of the Arctic regions. Dr. T. L. Phipson, and more recently Tissandier, found similar particles deposited by the winds on plates exposed in different localities. Finally, Mr. John Murray discovered magnetic particles raised from deposits at the bottom of the sea by H.M.S. *Challenger*. These particles were examined by Prof. Alexander Herschel, who agreed with Mr. Murray in ascribing a cosmic origin to them. For fuller details and all references we must refer to Mr. Ranyard's paper. There cannot be any doubt that magnetic dust, which in all probability derives its origin from meteors, has often been observed, and the question arises, in what way we can increase our knowledge on these points to an appreciable extent. A further series of occasional observations would in all probability lead to no result of great value, unless they were carried on for a great length of time in suitable places. Meteoric dust, we know, does fall, and observations ought if possible to be directed rather towards an approximate estimate of the quantity which falls within a given time. Difficulties very likely will be found in the determination of the locality in which the observations should be conducted. The place ought to be sheltered as much as possible against any ordinary dust not of meteoric origin. The lonely spots best fitted for these observations are generally accessible to occasional experiments only, and do not lend themselves easily to a regular series of observations. Nevertheless experiments continued for a few months at some elevated spot in the Alps might lead to valuable results. The Committee would like to draw attention to an instrument which is well fitted for such observations. It was devised by Dr. Pierre Miquel for the purpose of examining, not the meteoric particles, but organic and organised matters floating about in the air. A description, with illustrations, will be found in the *Annuaire de Montsouris* for 1879. Two forms of the instrument are given. In the first form, which is only adapted to permanent places of observations, an aspirator draws a quantity of air through a fine hole. The air impinges on a plate coated with glycerine, which retains all solid matter. By means of this instrument we may determine the quantity of solid particles within a given volume of air. The second, more portable, form does not allow such an accurate quantitative air analysis. The instrument is attached to a weathercock, and thus is always directed against the wind, which traverses it, and deposits, as in the other permanent form, its solid matter on a glycerine plate. An anemometer placed in the vicinity serves to give an approximate idea of the quantity of air which has passed through the apparatus. These instruments have been called aëroscopes by their inventor. It is likely that the second form given to the apparatus will be best fitted for the purpose which the Committee has in view.

Seventh Report of the Committee on Underground Water Supply, consisting of Prof. E. Hull, the Rev. H. W. Crosskey, Capt. Douglas Galton, C.B., Mr. James Glaisher, F.R.S., Prof. G. A. Lebour, Mr. W. Molyneux, Mr. G. H. Morton, Mr. W. Pengelly, Prof. J. Prestwich, Mr. James Plant, Mr. James Parker, Mr. T. Roberts, Mr. S. Stooke, Mr. G. J. Symons, Mr. W. Whitaker, was read by Mr. C. E. de Rance, of H.M.'s Geological Survey, the Secretary.—The Committee was appointed in 1874 at the Belfast Meeting of the Association, with Prof. Hull, LL.D., F.R.S., as Chairman, and Mr. De Rance, F.G.S., as Secretary and Reporter; its six published reports occupy 125 pages of the Society's *Proceedings*, and the results of the investigations of the Committee show that the Permian, Triassic, and Jurassic formations of England and Wales are capable of absorbing from five to ten inches of annual rainfall, giving a daily average yield of from 200,000 to 400,000 gallons per square mile per day. The area occupied by these formations is, in round numbers, Permian and Trias, 8600 square miles, and Oolites, 6600 square miles, capable of yielding 1720 millions and 1320 million gallons respectively, at the lowest rate of absorption, or, united, a supply for 100 million people, at thirty gallons a head. Mr. De Rance then described the water-bearing condition of the Yorkshire area, and stated that the investigation would now be extended to all the porous rocks of South Britain.

Report on the Earthquakes of Japan, by Prof. John Milne.—The author arrives at the following conclusions:—1. That the actual back and forth motion of the ground is seldom more than a few millimetres (usually not equal to 1mm.), even though chimneys have fallen. 2. The motion usually commences gently, but is very irregular. 3. The number of vibrations per second usually vary between three and six. 4. During one shock *its direction* of motion may be irregular. 5. East and west vibrations, as recorded in Yedo, have in some cases been shown by time observations to have travelled up from the south. 6. Many of the shocks which visit Yedo appear to have come

from the district which is faulted, and which shows distinct evidence of *very recent* elevation.

Second Report of the Committee consisting of Prof. P. M. Duncan and Mr. G. R. Vine, appointed for the purpose of reporting on Fossil Polyzoa; drawn up by Mr. Vine.—The order is divided into three subdivisions:—

1. *Cheilostoma*, Bark. = *Celleporina*, Ehrenberg.
2. *Cyclostomata*, „ = *Tabuliporina*, Milne-Ed., Hagenow, Johnston.
3. *Ctenostomata*, „

The following terms are used in this Report in describing the genera:—

ZOARIUM.—"The composite structure formed by repeated gemmation" = Polyzoarium and Polypidom of authors.

ZOŒCIUM or cell.—"The chamber in which the Polypide is lodged."

CŒNŒCIUM.—"The common dermal system of a colony." Applicable alike to the "Frond," or "Polyzoary," of Fenestella, Polypora, Phyllopora, or Synocladia: or to the associated Zoœcia and their connecting "interstitial tubuli," of Ceriopora, Hyphasmapora, and Archæopora, or species allied to these.

FENESTRULES.—The square, oblong, or partially rounded openings in the zoarium—*connected by non-cellular dissepiments*—of Fenestella, Polypora, and species allied to these.

FENESTRÆ applied to similar openings, whenever connected by the general substance of the zoarium—as in Phyllopora, Clathropora, and the Permian Synocladia.

BRANCHES.—The CELL-bearing portions of the zoarium of Glauconome, Fenestella, Polypora, or Synocladia; or the offshoots from the main stem of any species.

GONÆCIUM.—"A modified zoœcium or cell, set apart for the purposes of reproduction."

GONOCYST.—"An inflation of the surface of the zoarium in which the embryos are developed." Modern terms from the Rev. Thos. Hincks.

Report of the Committee on Erratic Blocks, drawn up by the Rev. B. W. Crosskey.—Many additional instances of the occurrence of erratic blocks were recorded. Particulars were given respecting granite and sandstone boulders found while excavating for the new dock at Maryport, Cumberland. The granite specimens vary in size from small pebbles to a ton in weight, and are rounded. The New Red Sandstone boulders vary from half a ton to two tons or more, and have sharp angles. The nearest granite occurs in the Kirkudbrightshire Hills, on the other side of the Solway, fifteen or twenty miles distant; the New Red Sandstone is the stone of the district. A boulder of Shap granite found near Filey has been removed to the University Museum, Oxford. It rested on Oolitic strata at a height of about 150 feet above the sea. The nearest place where a granite of the same character is found is 108 miles distant, bearing west-north-west from Filey. The attention of the committee was drawn by Prof. T. McK. Hughes to a boulder of porphyritic hornblende diabase, near the centre of Anglesea. It is chiefly interesting as having been considered an inscribed stone, but the supposed characters are entirely due to rock structure. A detailed description of the great erratic called the "Holy Stone," at Humberstone, Leicestershire, was given. Its weight is about twenty-one tons. It rests on a denuded surface of the Rhætic formation. The height from which it travelled is about 400 feet above the sea, and is situated six miles north-west. The present height at which the block now rests is about 240 feet above the sea, and there is a river valley between these two points, running at right angles to the line of transit of the block, which is only 110 feet above the level of the sea. Various groups of boulders in Leicestershire were also described, some containing millstone grit blocks derived from Derbyshire, which must have travelled about thirty-five miles. A catalogue of 191 blocks in the parish of Ashwell, County of Hertford, was given. None of these blocks are local. Their general derivation is from the Oolites of the Midlands and from the Carboniferous and other rocks of more northern districts. The report concluded with an appeal to local observers to give assistance in cataloguing the rapidly-disappearing erratic blocks of the country.

Report on Thermal Conductivity of Certain Rocks, showing especially the Geological Aspects of the Investigations, by Prof. A. S. Herschel and Prof. Lebour.—This is the seventh and final Report of the Committee, and comprises a *résumé* of the results given in the preceding ones, with numerous additions and corrections. A bibliographical list of all papers on the subject, by Mr. J. T. Dunn, B.Sc., is given as an appendix. The apparatus and specimens employed during the investigations of the Committee are preserved in the museum of the College of Physical Science at Newcastle-on-Tyne.

SECTION A—MATHEMATICAL AND PHYSICAL

On the Possibility of the Existence of Intra-Mercurial Planets, by Balfour Stewart, LL.D., F.R.S.—It is a somewhat frequent speculation amongst those who are engaged in sun-spot research to regard the state of the solar surface as influenced in some way by the positions of the planets.

In order to verify this hypothesis observers have tried whether there appear to be solar periods exactly coinciding with certain well-known planetary periods. This method has been adopted by the Kew observers (Messrs. De La Rue, Stewart, and Loewy), who had an unusually large mass of material at their disposal, and they have obtained from it the following results:—

1. An apparent maximum and minimum of spotted area approximately corresponding in time to the perihelion and aphelion of Mercury.

2. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Mercury and Jupiter.

3. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Venus and Jupiter.

4. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Venus and Mercury.

The Kew observers make the following remarks upon these results:—

"There appears to be a certain amount of likeness between the march of the numbers in the four periods which we have investigated, but we desire to record this rather as a result brought out by a certain specified method of treating the material at our disposal than as a fact from which we are at present prepared to draw conclusions. As the investigation of these and similar phenomena proceeds, it may be hoped that much light will be thrown upon the causes of sun-spot periodicity."

The Kew observers have likewise produced evidence of a different kind in favour of the planetary hypothesis, for they have detected a periodicity in the behaviour of sun-spots with regard to increase and diminution apparently depending upon the positions of the two nearer planets, Mercury and Venus. The law seems to be that as a portion of the sun's surface is carried by rotation nearer to one of these two influential planets, there is a tendency for spots to become less and disappear, while on the other hand, when it is carried away from the neighbourhood of one of these planets, there is a tendency for spots to break out and increase.

But whatever truth may be in these conclusions, it appears to be quite certain that periodical relations between the various *known* planets will not account for *all* the sun-spot inequalities with which we are acquainted. They may account for some, but certainly not for all. For there are solar inequalities of short duration which, presuming them to be real, can only be accounted for on the planetary hypothesis by supposing the existence of several unknown intra-Mercurial planets.

Indeed these short-period inequalities in sun-spots and the allied phenomena of terrestrial magnetism and meteorology have so augmented in number of late years as to make some observers inclined to question their reality; while others again resort to the above-mentioned hypothesis, and attribute them to intra-Mercurial planetary agency.

The method to be pursued in detecting the existence of inequalities will be easily understood by an illustration. Suppose that we had in our possession extensive records of the temperature of the earth's atmosphere at some one place in middle latitudes, and that, independently of astronomical knowledge, we were to make use of these for the purpose of investigating the natural inequalities of terrestrial temperature. We should begin by grouping the observations according to various periods taken, say, at small but definite time-intervals from each other. Now if our series of observations were sufficiently extensive, and if some one of our various groupings together of this series

should correspond to a real inequality, we should expect it to exhibit a well-defined and prominent fluctuation, whose departures above and below the mean should be of considerable amount.

Suppose, for instance, that we have twenty-four points in our series, and that we group a long series of temperature observations in rows of twenty four each, the time-distance between two contiguous members of one row being one hour. The series would thus represent the mean solar day, and we should without doubt obtain from a final summation of our rows a result exhibiting a prominent temperature fluctuation of a well-defined character, which we might measure (as long as we keep to twenty-four points) by simply adding together all the departures of its various points from the mean, whether these points lie above or below; in fine, by obtaining the area of the curve, which is the graphical representation of the inequality above and below the line of abscissæ taken to represent the mean of all the points. Suppose next that, still keeping to rows of twenty-four, we should make the time-interval between two contiguous members of a row somewhat different from one hour, whether greater or less, we should now in either case obtain a result exhibiting, when measured as above, a much smaller inequality than that given when the interval was exactly one hour; and it is even possible that, if our series of observations were sufficiently extensive, we should obtain hardly any traces of an inequality whatever.

In fine, when each row accurately represented a solar day, the result would be an inequality of large amount; but when each row represented a period either slightly less or greater than a day, the result would be an inequality of small amount. This process, as far as I have described it, is not new, inasmuch as something of this kind must be pursued in all attempts to detect inequalities. In the present instance we should by its means, after bestowing enormous labour in variously grouping, in accordance with a great number of periods taken at small intervals from each other, obtain definite results. These might be graphically represented in the following manner:—

The line of abscissæ might be taken to denote the exact values of the various periods, forming a time-scale, in fact, while the ordinates might represent the areas or summations obtained as above by employing these various periods. There would thus be in the case now used for illustration a very prominent peak, corresponding to twenty-four hours, which would fall off very rapidly on either side.

By means of the process now described we should at length, after enormous labour, obtain a graphical result, showing the exact position in the time-scale of the observed maximum inequality. In conjunction with Mr. William Dodgson, I have devised a method by which this labour is very greatly reduced, and the process so modified has been applied by us in order to determine whether there be inequalities of short period in the observed areas of the sun-spots occurring on the visible hemisphere of the sun. We have detected an inequality of this nature corresponding in period to 24·011 days, which, when subjected to a certain purifying treatment, appears to us to exhibit the marks of a true periodicity. But it has been suggested by Prof. Stokes that a method of this nature for detecting inequalities might with greater propriety be employed as a crucible for testing the value of some hypothesis introduced into it from without.

Acting upon this suggestion I have ventured to introduce the planetary hypothesis, and to ask whether the above sun-spot inequality of short period may not in reality be caused by an intra-Mercurial planet. It is quite easy to put this hypothesis to a test, taking for our guidance the results obtained by the Kew observers. For what do these results exhibit? In the first place they exhibit the probability of a sun-spot inequality corresponding to the period of Mercury round the sun; and in the next they exhibit the probability of similar inequalities corresponding to the synodic period of Mercury and Venus, and to the synodic period of Mercury and Jupiter.

Now if there be an intra-Mercurial planet of period 24·011 days, it will have the following synodic periods:—

With Mercury	... ...	33·025 days.
With Venus	... ...	26·884 days.
With Jupiter	... ...	24·145 days.

In conjunction with Mr. Dodgson I have applied the above method of analysis with the view of ascertaining whether there be well-marked sun-spot inequalities nearly corresponding to these periods, and we have obtained the following results:—

A very prominent inequality of period	...	32·955 days.
A very prominent inequality of period	...	26·871 days.
A less prominent inequality of period	...	24·142 days.

It will thus be noticed that there are prominent sun-spot inequalities, the periods of which agree very well with the synodic periods of the supposed planet with Mercury, Venus, and Jupiter, more especially if we bear in mind that this is only a first approximation.

The test, however, is not yet complete. Referring once more to the results of the Kew observers, it will be noticed that we have approximately maxima of sun-spot areas when Mercury and Venus, or when Mercury and Jupiter are in conjunction. Now if we assume that there is an intra-Mercurial planet of period 24·011 days, we are as yet unable to assign its exact position in ecliptical longitude at any moment. We know its period, and we may presume that it has considerable excentricity, but we know nothing else. We may, however, assume as most probable that the maximum point of the inequality of period 32·955 days corresponds to the conjunction of the planet with Mercury, the maximum point of the inequality of period 26·871 days to its conjunction with Venus, and the maximum point of the inequality of period 24·142 days to its conjunction with Jupiter. On this assumption, and knowing the average rate of motion of the planet in its orbit, we may deduce approximately its position at a given epoch independently from each of the three synodic periods above mentioned, and these positions ought to agree together, if our hypothesis be correct.

I have done this approximately, but am not able to bring exact figures before this meeting. The agreement is as great as can be expected, bearing in mind that we know only the average rate of motion of the planet, and not the variations of its rate, inasmuch as we are ignorant of its eccentricity. I think I may state that three independent values of its position corresponding to January 1, 1832, will be obtained, and that the mean difference of a single value from the mean of the whole will probably not be more than twenty degrees. It would thus appear from this investigation that the evidence is in favour of the sun-spot inequality of 24·011 days being due to an intra-Mercurial planet. Of course a single research of this nature is insufficient to establish a theory of this importance, but as there are several short-period solar inequalities, the same method may be pursued for each, an operation which demands nothing but time and labour. It appears to me of great importance that these short-period solar inequalities should be systematically examined after this method.

The Effects of Gulf Streams upon Climates, by Dr. S. Haughton.—The author said that the Gulf Stream, and its counter current, the Labrador Current, produced important effects upon climate. The northern hemisphere was warmer than the southern from lat. 0° to lat. 30°, and it was colder than the southern from lat. 40° to 60°. The higher temperature of the southern hemisphere in the temperate latitudes was explained by the existence of three gulf streams in that hemisphere, while there was only one in the North Atlantic, and a partial one through Behring's Straits in the northern hemisphere. The general climatal effect of the Gulf Stream was therefore to make the annual range of temperature less, but it had no effect whatever upon summer heat, or upon the fruiting of plants and trees, that required a given July temperature for reproduction. The January temperatures in the North Atlantic at 70° were raised by the Gulf Stream, whilst the July temperatures remain unaffected. The effect of the cold currents, which were indirectly caused by the warm currents to preserve the proper condition of equilibrium, was nothing at all upon the January temperatures, but they lowered the July temperatures. The effect of the cold water was to lower the July temperature and to leave the January untouched, and the effect of the warm current from the south was to raise January and to leave July unaltered.

The Photographic Spectrum of Comet B 1881, by Dr. W. Huggins.—The author stated that in 1868 he applied the spectroscope to the light of comets, the result of his observations being to show the presence of carbon probably in conjunction with hydrogen in the cometary matter. Since then, until the present year, no comet of sufficient brilliancy to admit of observations being made had appeared. On the evening of June 24 last he directed the spectroscope to the head of Comet B with an exposure of an hour; and on the following night he obtained a second photograph with an exposure of an hour and a half. As it happened, the photograph which was the result of the longer period of exposure was the weaker of the two, but, taken together, an examination of the bands confirmed his

previous observations, and showed that part of the light of the comet was reflected sunlight and part original light; and further, that carbon was present in the cometary matter, with strong evidence also of the presence of nitrogen, in addition to carbon and hydrogen.

The Electric Discharge through Colza Oil, by A. Macfarlane, D.Sc., F.R.S.E.—The electrical properties of colza oil which I have examined are its dielectric strength and some phenomena which accompany the passage of the spark. By the dielectric strength of a substance I mean the ratio of the difference of potential required to pass a spark through air under the same conditions. The electrodes used were two parallel brass plates each 4 inches in diameter. When comparing the gases the standard distance of the plate chosen was 5 mm. In the case of liquids it is convenient to observe for a shorter distance, and reduce the result by the law which previous experiments of mine have established, namely, that in the case of the discharge between parallel plates through a liquid dielectric the difference of potential required is proportional to the distance between the plates (*Trans.* R.S.E., vol. xxix. p. 563). One set of observations gave the ratio for colza oil to be 2·7, another gave 2·5. Hence 2·6 may be taken. I have now obtained the following table of dielectric strengths for liquids (1 being unity).

Substance.	Dielectric Strength.
Paraffin oil	3·7
Oil of turpentine	4·0
Paraffin liquefied	2·4
Olive oil	3·5
Colza oil	2·6

The specific gravity of the colza oil is ·91. The passage of the spark was accompanied by the formation of gas bubbles, but there was no deposition of solid particles. As the 4-inch plates were placed horizontally in the oil a bubble produced by the discharge was prevented from escaping by the upper plate. When the upper plate is again electrified such a bubble behaves in the following manner. If it is large enough it will extend itself somewhat like an hour-glass between the plates, but if it is smaller it takes the form of an acorn with a flat base, the base resting on one or other of the plates. When the upper plate is charged positively the bubble is repelled so as to place its base on the lower plate; when the electricity is charged to negative the bubble remains with its base on the upper plate. A reversal of the order of charging did not change the effect. After a few electrifications a sufficient number of solid particles collect to form a chain, and thus interferes with the phenomenon, the bubbles then being lengthened out in a remarkable manner, but never repelled to the lower plate. When the upper plate was charged negatively, gas bubbles appeared to me to rise from the lower plate, as if they had been formed there. To test this point further I took some sparks between two smaller disks placed vertically in the oil. The gas-bubbles were observed to rise up at the negative surface as if they had been formed at the positive surface, and had been repelled or carried straight across, and then rose up at the negative surface. When the spark was taken between two points bent at right angles to two rods dipping into the oil, the bubbles were observed to shoot out in the direction from the positively charged point, and to circulate round the earth-rod some time before rising to the surface. These phenomena indicate that the bubble is positively electrified.

On the Electric Conductivity and Dichroic Absorption of Tourmaline, by Prof. Silvanus P. Thompson.—The electric conductivity of tourmaline differs in different directions; being, according to the author's experiments, a minimum along the optic axis. Tourmaline also possesses the optical property of dichroism, its absorption being a maximum for rays parallel to the axis, and greater for blue rays than for red, equal thicknesses of crystal being considered. According to the electromagnetic theory of light, bodies which are good conductors of electricity should be opaque to light. The author has in the August number of the *Philosophical Magazine* rewritten the equations of Maxwell's electromagnetic theory for the case of crystalline media possessing different conductivities in different directions. From these equations it appears that in tourmaline and negative uniaxial crystals electric displacements at right angles to the axis will be more absorbed than electric displacements parallel to the axis. This accounts for the well-known greater absorption of the ordinary ray, provided the views of Stokes and Fresnel are correct, that these displacements are at right angles to the so-called plane of polarisation. The difference of velocity between rays of different colour accounts for the difference of absorption being greater in that direction in which the conductivity is a minimum. It was also pointed out that in positive uniaxial crystals, in which the electric conductivity is a maximum along the axis, there will be maximum absorption of the extraordinary ray, and there will be least opacity along the axis. Smoky quartz and magnesic platinocyanide fulfil the latter condition. Specimens of tourmaline cut into cubes to show the colours in different directions were shown, and also specimens of magnesic platinocyanide and of herapathite. Mechanico-optical models were also shown illustrating the theory; a tourmaline being represented by a cube built up of layers of glass and wire-gauze. In conclusion it was shown that crystals in which the electric conductivity differs in three different directions will exhibit *trichroism*; and that di- or tri-chroic absorption is a general property of all coloured crystals other than those of the cubical system.

On the Application of Electricity to the Localisation of a Bullet in a Wound, by W. H. Preece.—The author showed how an electric current could be made an invisible and immaterial probe localising the position of a bullet in the human body without touching or giving the slightest sensation of pain. The conception of using electricity alone as the tool occurred to Prof. Graham Bell in Washington, who at once telegraphed to the author to consult him in reference to the use of Hughes' induction balance. In order to apply this apparatus to the localisation of a bullet in a wound, Prof. Hughes recommended that a pair of exploring coils should be made movable and portable, in order that they might be moved over the body of the wounded man. If the coils were brought within three inches of the bullet its presence could be detected, the direction in which the bullet was situated could be determined by observing the position of maximum sound, for in that position the bullet would be in a line with the axis of the coil. In order to ascertain the depth of the bullet a similar bullet is moved along in the direction of the axis of the other coil until neutrality is obtained; the depth of the trial bullet then will be equal to the depth of the buried one.

On the General Coincidence between Sun-spot Activity and Terrestrial Magnetic Disturbance, by the Rev. F. Howlett, F.R.A.S.—The object of this paper was to inquire how far solar activity, more especially as regards sun-spots, is wont to be accompanied by terrestrial magnetic disturbances, as recorded by the automatic magnetic declination curves at Kew and Greenwich. The data for such an investigation were furnished by comparisons instituted between the most striking instances of sun-spots gathered out of a long series of solar observations carried on by Mr. Howlett from the year 1859 to the present epoch, and the synchronous conditions of the magnetic curves at the observatories above mentioned. The telescopic drawings of the spots were obtained with an achromatic of three inches aperture by Dollond, of forty-eight inches focal distance, projecting the sun's image on a large white screen in a darkened chamber. By employing a Huygenian eyepiece magnifying 120 linear, and placing the screen at the distance of five feet two inches from the eyepiece, a very distinct image of the sun was obtained of about five feet four inches in diameter, and of which every inch corresponded to just 30″ of the celestial arc. Not only were the measurements of all the solar phenomena rendered thereby exceedingly easy, but the conditions of amplification, illumination, and definition of details were combined in about the best possible manner for the observer's purpose, which was to maintain an accurate record of the solar spots, and very frequently of the faculæ also, on a large scale, and which have been collected into five volumes and presented to the Royal Astronomical Society. The comparisons commence with the very remarkable and cyclonic group of August, 1859, which was uniquely distinguished by the remarkable outburst of intense white light, far brighter than the photosphere itself, which fortunately was witnessed by the late Messrs. Carrington and Hodgson on the forenoon of September 1, but which Mr. Howlett missed seeing by only a few minutes, having completed his drawings, and left the telescope. Other striking and, if they may be so termed, crucial groups were compared with the magnetic records—very notably the great spot of October, 1865, engravings of which may be found in the volume of the *Proceedings* of the Royal Astronomical Society for the year last mentioned, as also the large groups of February, 1870, which were observed and drawn on the occasion of the recurrences by revolution of the same groups in the three consecutive months of February, March, and April of that year, and on the last of which months the total displacement, at one and the same time,

of the solar photosphere—or in other words, the total area occupied by the sun-spots—was no less than five thousand two hundred million square miles, or about twenty-seven times that of the superficies of the earth! So again in August and September, 1870, immense groups, occupying from four to five thousand million square miles, were observed to make two consecutive revolutions, and on the latter of which two occasions a beautifully enlarged photograph of the sun, twenty-four inches in diameter, was made by Mr. Titterton of Ely, under the auspices of the late Canon Selwyn, and exhibited to Section A. On all these occasions great magnetic disturbances, amounting often to absolute magnetic storms, were unequivocally manifested; and in fact out of twenty-four comparisons instituted, the following is the summary of results, as showing the coincidence of extensive solar activity and synchronous magnetic disturbances:—

Intensely	5	
Very decidedly	3	
Decidedly	9	= 21 affirmatively
Moderately	3	
Negatively (no spots, no storms)	1	
Questionable	1	3 contradictory
Contradictory	2	
	24	

Thus then, from the data collected, it would certainly appear that marked periods of solar activity are wont to coincide with marked periods of terrestrial magnetic disturbances; but yet from a careful comparison of the days and hours of the magnetic records appealed to, it also appeared that the disturbances were manifested in a variety of ways, not only as regarded the extent of the magnetic excursions of the needle, the rapidity of the oscillations, or the persistency of the more moderate disturbances, but also they were found to follow at considerably different intervals of time after the commencement of the observed solar outbursts. With respect, lastly, to reactionary influences, Mr. Howlett stated, on the authority of Mr. Whipple, the director of the Kew Observatory, that on the occasion of the perihelion passage of comet *b* 1881, on the 16th day of June last, the terrestrial magnetic curves were unusually quiescent.

On Artificial Flight, by Fred. W. Brearey.—The author proceeded to argue that the weight of the bird plays an active part in its flight, and that this result arises from the action of that portion of the pectoral muscle which depresses the wing. So great is the tension of this muscle that it is highly probable that, in the case of those long-winged and heavy birds which are able to fly without apparently moving a feather, the wings are kept extended against the resistance of the air underneath without any voluntary effort of the bird. Its weight pressing upon the air causes this muscle to expand in raising the wing, and aids in the effect of the downward stroke by its contraction. The author exhibited a model with wings 4 feet from tip to tip and 3 feet 2 inches from head to tail. The wings are moved by M. Penaud's plan of strands of india-rubber previously put into a state of tension, which in unwinding create a flapping of the wings. By an india-rubber cord attached to the under part of the wing and passing under the shaft to which the mechanism is attached an equilibrium between the two forces is attained; that is to say, the india-rubber strands are wound up to that extent that the wings in rising stretch the india-rubber cord—or, as the author calls it, the pectoral cord—until one force neutralises the other; so that, held in the hand, there is no action. When liberated, and committed to the pressure of the air, the weight of the model causes the wings to be elevated, and therefore stretches the pectoral cord, which in its contraction assists the power derived from the twisted rubber in depressing the wings against the weight of the model. During this action the flight is well sustained for 40 feet or more. The author states that an apparatus of the nature of a longitudinal parachute was liberated from a balloon which rose from Woolwich arsenal, and it travelled back, by the aid of gravity alone, to the arsenal, a distance of half a mile. From this he argued that if the fabric can be manipulated so that propulsion also can be imparted to it, then some encouraging results would be likely to follow. He showed a model of large size upon this principle, and how, by the action of the wing-arms, a wave is transmitted from head to tail along a loose surface in shape like a kite. This loose surface requires a fall before it can be inflated by the air underneath; the wave-motion of the wings is then found adequate to its propulsion.

On the Arrestation of Infusorial Life, by Prof. Tyndall.—Three years ago I brought with me to the Alps a number of flasks charged with animal and vegetable infusions. The flasks had been boiled from three to five minutes in London, and hermetically sealed during ebullition. Two years ago I had sent to me to Switzerland a batch of similar flasks containing other infusions. On my arrival here this year 120 of these flasks lay upon the shelves in my little library. Though eminently putrescible, the animal and vegetable juices had remained as sweet and clear as when they were prepared in London. Still an expert taking up one of the flasks containing an infusion of beef or mutton would infallibly pronounce it to be charged with organisms. He would find it more or less turbid throughout, with massive flocculi moving heavily in the liquid. Exposure of the flask for a minute or two to lukewarm water would cause both turbidity and flocculi to disappear, and render the infusion as clear as the purest distilled water. The turbidity and flocculi are simply due to the coagulation of the liquid to a jelly. This fact is some guarantee for the strength of the infusions. I took advantage of the clear weather this year to investigate the action of solar light on the development of life in these infusions, being prompted thereto by the interesting observations brought before the Royal Society by Dr. Downs and Mr. Blunt in 1877. The sealed ends of the flasks being broken off, they were infected in part by the water of an adjacent brook, and in part by an infusion well charged with organisms. Hung up in rows upon a board, half the flasks of each row were securely shaded from the sun, the other half being exposed to the light. In some cases, moreover, flasks were placed in a darkened room within the house, while their companions were exposed in the sunshine outside. The clear result of these experiments, of which a considerable number were made, is that by some constituent or constituents of the solar radiation an influence is exercised inimical to the development of the lowest infusoria. Twenty-four hours usually sufficed to cause the shaded flasks to pass from clearness to turbidity, while thrice this time left the exposed ones without sensible damage to their transparency. This result is not due to mere differences of temperature between the infusions. On many occasions the temperature of the exposed flasks was far more favourable to the development of life than that of the shaded ones. The energy which in the cases here referred to prevented putrefaction was energy in the radiant form. In no case have I found the flasks sterilised by insolation, for on removing the exposed ones from the open air to a warm kitchen they infallibly changed from clearness to turbidity. Four and twenty hours were in most cases sufficient to produce this change. Life is, therefore, prevented from developing itself in the infusions as long as they are exposed to the solar light, and the paralysis thus produced enables them to pass through the night-time without alteration. It is, however, a suspension, not a destruction, of the germinal power, for, as before stated, when placed in a warm room life was invariably developed. Had I had the requisite materials I should like to have determined by means of coloured media or otherwise the particular constituents of the solar radiation which are concerned in this result. The rays, moreover, which thus interfere with life must be absorbed by the liquid or by its germinal matter. It would therefore be interesting to ascertain whether, after transmission through a layer of any infusion, the radiation still possessed the power of arresting the development of life in the same infusion. It would also be interesting to examine how far insolation may be employed in the preservation of meat from putrefaction. I would not be understood to say that it is impossible to sterilise an infusion by insolation, but merely to indicate that I have thus far noticed no case of the kind.

The Sun-Spot Period and Planetary Tides in the Solar Atmosphere, by F. B. Edmonds.—The author said that the influence of the planet may be localised on a surface or stratum of small thickness, so that the disturbing force would vary as the square of the distance of the planet. Under this supposition the predominance of Jupiter seemed to shut out out the idea that sun-spot maxima and minima could depend simply on the opposition and conjunction of the planets. The consequence of such a supposition was not to be lost sight of, but may be taken together with the more general supposition that the attractive force is exercised on a gaseous envelope, of which the altitude is not insignificant. Again, the mass of the sun is acted on by the planets, and such parts as are fluid, whether in the liquid or

gaseous form, are subject to a disturbance of a tidal character as a matter of course. The author argued that a disturbing body would therefore raise a tide on the sun more than one hundred times greater than the same force would raise it if acting on a globe the size of the earth, the other circumstances being the same. Looking at the sun-spot numbers as a record of spring tides and as a first approximation, recognising only such tides as would be due to the conjunction and opposition of Venus and the earth, it remained to establish a relation between these tides and the tide due to Jupiter in the form of special tides varying in magnitude with the sun-spot numbers.

On a New Integrating Anemometer, by H. S. Hele Shaw and Dr. Wilson.—An ordinary Robinson's cup anemometer is used to drive a train of wheels and thus ultimately a serrated roller, which moves a board in the direction of, and with a velocity proportional to, that of the wind. On the board, which is horizontal and about two feet square, is placed a sheet of paper, upon which the roller presses, and in turning leaves the required trace, at the same time moving the paper underneath it. The board is prevented from having a rotary motion by means of a pair of frames, the upper moving by means of wheels on the lower, each of which can only move in one direction, and these directions are perpendicular to each other. By a clockwork adjustment the time element is able to be introduced, which, taken in connection with space, gives velocity. A method of performing this was shown, as also a proposed form of the instrument for observatories.

On a Universal Sunshine Recorder, by G. M. Whipple.—The author gave a description of a new form of card-supporter for the Campbell sunshine recorder. It consisted of a light frame capable of holding the slip of cardboard, to be burned by the sun in any position. It was arranged so as to receive ordinary parallel strips of card at all times of the year, and to allow of the instrument being employed on any part of the earth's surface without detriment to its efficiency. The card-holders themselves are movable, so as to permit of the cards being changed indoors or dried, if wet, before removal, in order to avoid mutilating the record of the observation. The instrument also has an appliance for placing the card correctly in position to receive the sun's image.

On the Calibration of Mercurial Thermometers by Bessel's Method, by Prof. Rucker.—The author stated that the late Mr. Welsh of Kew Observatory described to the British Association in 1853 the methods which he introduced of making and correcting mercurial thermometers. The correction with which the author dealt was that due to the variations in the bore of the tube. Mr. Welsh's method of making this correction, which is still employed at Kew, is less theoretically perfect than others, and has been unfavourably criticised abroad. The author, in conjunction with Prof. Thorpe, has recently corrected a number of thermometers with great care by Bessel's method, which is the most elaborate and perfect hitherto proposed. One set of three thermometers were made for them at Kew, and were calibrated according to Welsh's method. Afterwards the measurements necessary for the application of Bessel's method were made by the Kew authorities, the calculations being performed by the author and Prof. Thorpe. The Kew thermometers were thus subjected to the most rigorous possible test, and they were able to announce that in one instrument the errors left after the application of Welsh's method were not greater than four-thousandths of a degree Centigrade, and in no case did they exceed one-hundredth of a degree. As it is impossible to read on these thermometers less than a hundredth of a degree with certainty, Welsh's method, as applied at Kew, is practically perfect.

SECTION B—CHEMICAL SCIENCE

On a Process for Utilising Waste Products and Economising Fuel in the Extraction of Copper, by J. Dixon (Adelaide, South Australia).—This paper contains an account of a process for extracting copper from sulphurous ores, in which the heat generated by the combination of the oxygen of the air with the sulphur of the ore is utilised for the smelting of the ore. This process is based upon experiments, which, although the author regards as incomplete, show (1) that the charge grows visibly hotter by simply blowing air through it; (2) that the melting of the raw ore or regulus and its reduction can be carried on in the same furnace; (3) that if the ore is in lumps, and fed at the top whilst the air is admitted by the side, a practically clean slagg can be obtained; but if added in a coarse powder, as it is generally found in the market, it either blows out again or chokes the furnace; (4) that a rough copper of about 96 per cent. pure metal can be obtained by the successful working of this process.

On the Chemical Action between Solids, by Prof. Thorpe, Ph.D., F.R.S.—The author drew attention to the extremely rare instances of such action hitherto observed, showing how many of these might be explained on the supposition that combination actually occurred between the bodies either in solution or in a state of gas. For example, the formation of cement steel, by the combination of carbon with iron, which had long been adduced as an example of such combination between solids, was now explained by the fact that iron at a high temperature was permeable to gases, and that in the actual process of cementation oxides of carbon were formed, which were in reality conveyors of carbon to the metal. He then illustrated by experiments the formation of several compounds by bringing together the components in solid form, choosing as examples such as would manifest their formation by characteristic colouring. Thus, as instances, potassium iodide and mercuric chloride, potassium iodide and lead nitrate, and silver nitrate and potassium chromate, were powdered together in a mortar, and in each case evidence of an action was exhibited by the production of characteristic colours of the product of the reaction of these compounds. The author referred to the memoir of the Belgian physicist, Prof. Spring, on the same subject, some of whose experiments he had repeated and in the main confirmed. One of the most remarkable results obtained by the Belgian professor was the formation of coal from peat by subjecting the latter material to a high pressure. Peat from Holland and Belgium, when exposed to a pressure of about 6000 atmospheres, was, according to Spring, changed into a mass which in all physical characters resembled ordinary coal. Experiments of the same nature made by Dr. Thorpe with various samples of British peat yielded, however, a very dissimilar result. These experiments were made with pressures which were considerably less and more than those employed by Spring. Although solid, compact masses, hard and very much changed in structure, were attained, in no case was any product obtained which could be confounded with bituminous coal. He said it was highly improbable, on purely chemical grounds, that mere pressure had been little more than an important factor in the transformation of woody matter into coal.

Metallic Compounds containing Bivalent Hydrocarbon Radicals, Part ii., by J. Sakurai.—This is a continuation of the work, an account of which was given at the last meeting (NATURE, vol. xxii. p. 448, or *British Association Report*, 1880). *Dimercury methylene iodide*, $CH_2(HgI)_2$, is obtained by exposing methylene iodide with an excess of mercury to the action of light. It is a yellowish crystalline powder insoluble in ordinary solvents, but soluble in hot methylene iodide; it melts at 230° with partial decomposition. Iodine converts this compound into methylene iodine and mercuric iodide. This same compound is easily obtained by the exposure of the mono-mercuro-compound described last year (*loc. cit.*), mixed with mercury and mercuric iodide, to the sunlight. Hydric chloride reacts on dimercury methylene iodide, producing mercury iodomethide. The insoluble compound mentioned in the former publications (*loc. cit.*) the author regards $CH(HgI)_3$, and therefore contains a trivalent hydrocarbon radical. We have thus the following series of organo-mercury compounds:—

$$CH_3(HgI),\ CH_2(HgI)_2,\ CH_2HgI_2,\ \text{and}\ CH(HgI)_3.$$

On the Occlusion of Gaseous Matter by Fused Silicates at High Temperatures, and its Possible Connection with Volcanic Agency, by I. Lowthian Bell, F.R.S.

On the Siliceous and other Hot Springs in the Volcanic District of the North Island of New Zealand (with Photographic Illustrations), by W. Lant Carpenter, B.A., B.Sc., F.C.S. The author gives an account of his visit to this district in December, 1880; analyses of the water of many of the springs in the district are also given. The water of the springs in the neighbourhood of Lake Taupo were found to be chiefly siliceous; they are all more or less impregnated with free iodine, and possess a medicinal value. The water of one spring was found to be strongly impregnated with sulphates of iron and alumina. The water of the springs in the Hot Lake district of Rotona and Rotomahana contain large quantities of silica; the deposits from two of these form large siliceous terraces. The water of the springs in the White Island, which is the summit of an extinct volcano,

contains free hydrochloric acid in large quantities. The water of these springs deposits sulphur and sulphate of lime.

On the Two First Lines of Mendeleeff's Table of Atomic Weights, by W. Weldon, F.R.S.E. The author draws attention to certain relations exhibited by the fourth and eighth powers of the atomic weights of the elements in these two lines of Mendeleeff's series. The atomic weights used to exhibit these relations differ but slightly from those generally accepted. The fourth powers of the atomic weights of the elements from lithium to fluorine, viz. lithium, beryllium, boron, carbon, nitrogen, oxygen, and fluorine, are, with the exception of carbon, related to one another as the whole numbers 1, 3, 6, 16, 27, and 54. In order to establish a similar relationship for the atomic weight of all these elements, it is necessary to raise their atomic weights to the eighth power; when the following relationship is found to exist:—

$$Li^8 = tN^8 = 2^8$$
$$G^8 = 3^2 \qquad O^8 = 3^6$$
$$B^8 = 3^2 \times 2^2 \qquad F^8 = 3^6 \times 2^8$$
$$C^8 = 3^2 \times 2^8$$

Similar relationships are established for the fourth powers of the atomic weights of the elements in the next line of Mendeleeff's table. The author concludes that, in the case of each of these fourteen elements some power of their atomic weight is a simple multiple of the corresponding power of the atomic weight of lithium; further, that this multiple is a function of 2 or 3, or of 2 and 3 combined.

Note on the Chrome Iron Ore of Japan, by E. Divers, M.D.—This paper contains a description and analysis of a specimen of chrome iron ore found in serpentine rocks, in the prefecture Oita. Analysis shows it to contain magnesia, replacing ferrous oxide, and the formula $MgO2FeO.2Cr_2O_3$ is attributed to it.

On the Oxides of Manganese, by V. H. Veley, B.A.—The author at the outset gives an historical sketch of the researches on this subject, in order to show how far it has been satisfactorily proved by them—(1) that manganese forms a series of definite oxides (apart from those present in manganic and permanganic acids); (2) that manganese dioxide forms a series of dioxides. An account is also given of the researches of Dittmar, Wright, and others on the conditions of formations of these oxides, and their behaviour when heated to various temperatures in certain gases. The author has studied the action of air, oxygen, nitrogen, and hydrogen at temperatures varying from 60°–200°, on an oxide having the formula Mn_6O_{11}. Hydrates of the following higher oxides, $Mn_{24}O_{39}$, $Mn_{24}O_{41}$, $Mn_{24}O_{43}$, $Mn_{24}O_{46}$, $Mn_{24}O_{47}$, have been prepared and analysed. When these oxides are heated in air or oxygen, at low temperatures, two changes are observed: (1) a loss of water of hydration; (2) an absorption of oxygen. When heated in nitrogen they are dehydrated, and at higher temperatures lose available oxygen. Heated in hydrogen, they are simultaneously dehydrated and reduced. The author regards these oxides as distinct chemical entities, and not mere combinations of molecules, or molecular compounds.

On the Inferences deducible from High Molecular Weights as exhibited by the Oxides of Manganese, by Prof. Odling, M.B., F.R.S.—In the course of his remarks Prof. Odling brought under the notice of the Section the various considerations which affected the determination of the relative weights of the reacting units of chemical substances. He contended that estimations of vapour-density had not had, and could not have, an absolutely determining influence, but thought these estimations always required to be checked by purely chemical considerations. In particular he referred to the cases of bodies which had two or more distinct vapour-densities—cases which were becoming added to daily. He expressed his entire concurrence with the views of the President of the Section as to the non existence of two distinct forms of combination, atomic and molecular, and strongly opposed the notion that various metallic elements possessed a definite capacity of saturation. He brought forward various illustrations to show that the saturation capacity of both metallic and non-metallic elements was indefinite. He contended that the doctrine of atomicity furnished a very inadequate idea of the most important facts of chemical combination, and that the representation of atomicity notions by graphic formulæ was highly misleading.

On Peppermint Camphor (Menthol) and some of its Derivatives, by R. W. Atkinson, B.Sc. (Lond.), and H. Yoshida.—This paper contains an account of the determinations of the physical properties of menthol ($C_{10}H_{20}O$), menthone ($C_{10}H_{18}O$), menthene ($C_{10}H_{18}$), and those of a hydrocarbon, $C_{10}H_{16}$, which latter compound is obtained by the action of hydric iodide upon menthol and subsequent treatment with caustic soda and metallic sodium. The authors also discuss the constitution of the above compounds.

Note on the Occurrence of Selenium and Tellurium in Japan, by E. Divers, M.D.—The author draws attention to the fact that the presence of these two elements has been observed in Japanese sulphuric acid, and considers it probable that these substances occur in material quantities in Japan.

Note on the Sodium Alum of Japan, by E. Divers, M.D.—An analysis is given of a specimen of this substance; it occurs as an efflorescence on decomposing sodium albite, which contains pyrites scattered through it. It is found in the province of Idzuno, in the prefecture of Shiniané, near the coast. It is said to occur in considerable quantities. It occurs in two forms, one massive, finely fibrous, greyish white and translucent, and the second in friable opaque tears, slightly coloured by iron oxide.

Brewing in Japan, by R. W. Atkinson, B.Sc. (Lond.).—The Japanese brewing process is divided into two parts comparable with the malting and brewing processes of beer-making. The mode of preparation and the properties of the diastatic materials are different in the two cases. The Japanese equivalent of malt or "kōji" hydrates maltose in addition to cane-sugar, dextrin, and starch, and the ultimate products of its action on starch-paste are dextrose and dextrin, or perhaps dextrose alone. Kōji differs from malt in being rendered inactive by heat at a much lower temperature than malt. Kōji is prepared as follows: a mixture of steamed rice and water is allowed to remain in shallow tubs at a low temperature (0°–5° C.) until quite liquid; it is then heated, fermentation commences, and continues until nearly all the dextrine first formed is exhausted. This product is now used like yeast, and is added to fresh quantities of steamed rice and water, fermentation proceeding until the percentage of alcohol amounts to about 13 or 14 per cent. by weight. After the greater part of the rice added has been used up, the mash is filtered and clarified by standing. The "saké" so produced requires very careful watching, and when summer approaches, or it exhibits signs of putrefactive fermentation, it is then heated in iron vessels; this operation has frequently to be repeated. Analyses of various specimens, fresh and diseased, are given in the paper.

Observations on the Specific Refraction and Dispersion of Light by Liquids, by J. H. Gladstone, Ph.D., F.R.S.—The general conclusions arrived at from a large series of observations on different liquids are as follows:—

I. The confirmation of the statement made by the Rev. T. P. Dale and the author, viz., that the length of the spectrum (the difference between the refraction of the Fraunhofer lines A and H) decreases with elevation of temperature.

II. It would appear that the length of the spectrum divided by the density, *i.e.*, $\frac{\mu H - \mu A}{d}$, is approximately, but not exactly, constant for different temperatures.

III. The specific dispersion appears to decrease with increase of temperature.

IV. The specific dispersion is influenced by the chemical constitution of a compound. In the case of hydrocarbons the change of the refractive equivalent of the carbon from 5·0 to 6·1 or 8 influences the specific dispersion to a far greater extent than the specific refraction.

V. Finally, the specific dispersion of a compound does not appear to be the means of the specific dispersions of its constituents.

On the Production of Crystals by the Action of Metals on Carbon Disulphide in Sealed Tubes, by P. Braham, F.C.S.—The author described a series of experiments which consisted in sealing up fifteen different metals in carbon disulphide. Some of these tubes were sealed up in 1879, and in those which contained gold, antimony, and bismuth, microscopic crystals were found. The composition of these crystals has not as yet been determined. The author also exhibited a micro-goniometer.

On the Alleged Decomposition of the Elements, by Prof. Dewar, M.A., F.R.S.—In his remarks Prof. Dewar dealt chiefly with the spectroscopic work from which Mr. Norman Lockyer had drawn conclusions very different from those of Professors Liveing and Dewar, especially concerning the value of evidence on the subject. Prof. Dewar argued that Mr. Lockyer's views regarding the existence of carbon vapour in the corona of the sun would not bear scientific investigation, and that his views regarding the modifica

tion of the spectrum of magnesium were equally illusory, and gave no proof of the decomposition of elementary substances. Finally he discussed Mr. Lockyer's theory of "basic lines," and addressed himself to a refutation of the same. The results recorded, he said, strongly confirmed Young's observations, and left little doubt that the few as yet unresolved coincidences either would yield to a higher dispersion, or were merely accidental. It would indeed be strange if amongst all the variety of chemical elements and the still greater variety of vibrations which some of them were capable of taking up, there were no two which could take up vibrations of the same period. They certainly should have supposed that substances like iron and titanium, with such a large number of lines, must each consist of more than one kind of molecule, and that not single lines, but several lines of each, would be found repeated with the spectra of some other chemical elements. The fact that hardly a single coincidence could be established was a strong argument that the materials of iron and titanium, even if they be not homogeneous, were still different from those of other chemical elements. The supposition that the different elements might be resolved into simple constituents and even into a single substance had long been a favourite speculation with chemists; but however probable that hypothesis might appear *à priori*, it must be acknowledged, according to Prof. Dewar, that the facts derived from the most powerful method of analytical investigation yet devised, gave it but scant support.

On Manganese Nodules and their Occurrence on the Sea-Bottom, by J. Y. Buchanan.—The author exhibited specimens of the nodules obtained from the South Pacific, and also from Loch Fyne; an account of those obtained in Loch Fyne was given in NATURE, vol, xviii. p. 628. Some nodules containing cobalt were also exhibited; these the author had obtained from New Caledonia. An account is given of the author's method of dredging for mud. The nodules have been found to contain iron oxides, copper, cobalt, nickel, sand, &c. Further, in no case was the amount of oxygen found to be sufficient to form a peroxide with the manganese. The kernels of these nodules are usually richer in manganese oxides than the external portions. Concerning their mode of formation the author thinks that this takes place *in situ*, and that the nodules are not brought from a distance. Further, it would appear that living organisms assist in this formation, although indirectly, insomuch as the decomposing animal matter reduces the sulphates of the sea-water to sulphides, which in their turn react on the iron and manganese minerals (chiefly silicates) in the mud, and thus forming sulphides of these metals. When the organic matter is exhausted these sulphides are oxidised to oxides by the oxygen of the water, forming concretions or incrustations of the ochreous oxides, which naturally inclose the other and unaltered constituents of the mud.

On the Action of Zinc, Magnesium, and Iron on Acidified Solutions of Ferric Sulphate, by Prof. T. E. Thorpe, Ph.D., F.R.S.—The extent of the reduction of the ferric salt may vary with the strength of the solution, with its temperature, with the amount of free acid present, and lastly with the specific nature of the metal employed. The author has studied the conditions under which the hydrogen does work as a reducing agent. Experiments were made on dilute solutions of ferric sulphate, containing known quantities of free acid. The author finds (1) that the extent of reduction, produced by a given weight of zinc in dissolving, increases with the temperature; (2) that it is also affected, although to a less degree, by the initial surface of the metal exposed. Whilst the extent of reduction, as also the rapidity of solution, increase with the temperature, at a given temperature the extent of reduction increases, although at a gradually diminishing rate, with the time of solution. The rapidity of solution and extent of reduction produced by a given quantity of zinc, of a given area and in a solution of a given temperature, and containing a definite weight of free acid, increase with the amount of reducible iron present. Experiments made by placing zinc in contact with platinum showed that, although the time of solution of zinc in contact with platinum is considerably diminished, as compared with that of zinc alone, little difference in the reducing effect is observed. Similar results were obtained with magnesium, although the amount of reduction is from one-fourth to one-third of that produced by zinc under similar conditions. The diminution of the rate of solution with decrease in the amount of free acid present, is far greater in the case of magnesium than in that of zinc. The author concludes that his experiments strongly support the view that the reducing power of nascent hydrogen is connected with the existence of this body in the atomic condition, since all conditions tending to prolong the duration of this atomic condition augment the reducing power.

On the Reducing Action of Zinc and Magnesium on Vanadium Solutions, by Prof. Roscoe, LL.D., F.R.S.—From his original experiments on this subject the author had drawn the conclusion that, whilst the reduction in the case of zinc and sodium took place from V_2O_5 to V_2O_2, in the case of magnesium it only proceeded to V_2O_3. Later experiments have, however, shown that each of these reducing agents acts in the same manner, but that the reduction from V_2O_5 to V_2O_2 takes place very slowly when magnesium is used.

Note on a New Method of Measuring Certain Chemical Affinities, by A. Tribe.—The author points out that when a metal is immersed in an electrolytic field, *i.e.*, in an electrolyte in the act of electrolysis, and the electromotive force set up on any part of its surface is sufficient to decompose the medium, then the positive *ion* separates out on that part of the surface which has received negative electrification, and the negative *ion* on the portion which is positively electrified. If such a plate be of a rectangular form, and it be so placed that the lines of force are perpendicular to its surface, then the maximum electromotive force is set up in the central part of the plate, and at the edges it becomes so weak as to be unable to initiate any electro-chemical action. If the sheet be placed in the electrolytic field, so that the lines of force are parallel with its sides and with two of its edges, then the maximum electromotive force is at the end of the plate and is the weakest at the centre, where it is unable to bring about electro-chemical changes. That this is the case is shown by the boundaries of the deposits, which in many cases are very sharply defined. From the intimate connection between electromotive force and chemical affinity, the author supposed that if, in a series of trials, the chemical affinities were altered, other circumstances remaining the same, the magnitude of the intermedial space between the boundaries of the electro deposits would increase with the force required to overcome the affinities of the *ions* of the electrolytes. This supposition has been confirmed by several experiments, *e.g.*, with sheets or analysers of silver immersed in solutions of chloride, bromide, and iodide of zinc, it was found that the intermedial space was the greatest in the case of the chloride, and in the case of the bromide it was greater than in the case of the iodide.

On some Phenomena of the Nature of Chemico-Magnetic Action, by W. Thomson, F.R.S.E.—The author had observed that the colour from a piece of cloth dyed with Prussian blue was discharged in the neighbourhood of a piece of iron which had been lying upon it for some weeks. The ash of the portions of cloth from which the colour had been discharged was found to contain but a trace of iron. Experiments were made in which no iron was used, and the blue colour was bleached but slightly, showing that the action could not be attributed to light alone. Further experiments, in which small pieces of iron or magnets were used, showed not only that the colour was discharged, but that the colour so discharged appeared to be rearranged in semicircles on each side of the bar of iron. When magnets were used, the colour assumed more or less of circular forms, which were developed not only from the poles, but from all parts of the magnets. These phenomena the author does not regard as due to magnetic action, for when a piece of gutta percha tissue was placed between the wet cloth and the magnet, no action took place, even after several weeks. Similar observations have been made with cloth dyed with aniline colours, and with a like result.

On the Double Iodide of Copper and Mercury, by Prof. Silvanus P. Thompson, D.Sc.—After describing the preparation of this compound, which is cuproso-mercuric iodide, Cu_2HgI_4, the author draws attention to one property, viz., its change of colour by a comparatively small change of temperature. At the ordinary temperature this substance possesses a brilliant red colour, and when heated, it becomes black, changing back to red on cooling. In thin layers this substance transmits light, but becomes opaque on heating. Now according to the electro-magnetic theory of light, opaque bodies are the best conductors of electricity; therefore this double iodide of copper and mercury should conduct heat better at a high than at a low temperature. Experiment has shown this to be true to a certain extent only, as beyond a certain temperature its conductivity becomes less; this is probably due to its decomposition when heated beyond a given temperature. In conclusion, the author pointed out several ways in which this change of colour of this

compound could be used in lecture experiments. For instance, it may be used to show the conduction of heat along a copper rod; for this purpose the rod is coated with the red compound, which is gradually blackened as the heat travels along the rod. It may also be substituted for wax in Tyndall's experiment for showing how crystals conduct heat. There are also a variety of other ways in which it may be applied.

The Effect of the Spectrum on Silver Chloride, by Capt. Abney, R.E., F.R.S.

Alterations in the Properties of Nitric Ferments by Cultivation, by R. Warington.—The author, after giving an account of his experiments on the cultivation of these ferments, which consist of organisms resembling bacteria, states that these nitrifying ferments are capable of existing in three forms—(1) the nitric ferment of soil, which converts both ammonium salts and nitrites into nitrates; (2) the altered ferment, which converts ammonium salts into nitrites, but fails to convert nitrites into nitrates; (3) a surface organism which converts nitrites into nitrates.

On the Fluid Density of Certain Metals, by Prof. W. Chandler Roberts, F.R.S., and T. Wrightson, C.E.—This is an account of a continuation of experiments upon this subject, some of which were submitted to the Section at Swansea (*vide* NATURE, vol. xxii. p. 448). The authors also exhibited the *oncosimeter* described in the *Journal* of the Iron and Steel Institute (ii. 1879, p. 418), by the aid of which these results were obtained. The following table contains the results obtained:—

	Sp. gr. solid.	Sp. gr. liquid.	Percentage of change in volume.
Bismuth	9·82	10·055	Decrease 2·3
Copper	8·8	8·217	Increase 7·1
Lead	11·4	10·37	,, 9·93
Tin	7·5	7·025	,, 6·76
Zinc	7·2	6·48	,, 11·10
Silver	10·57	9·51	,, 11·2
Iron	6·95	6·88	,, 1·02

On Molecular Attraction, by F. D. Brown, B.Sc.—The author points out that, if we regard chemical affinity as neutralised by the union of two elements, we are then unable to account for the reactions taking place between molecules, and involving an interaction of the atoms composing different molecules. If, however, the act of combination be regarded as producing no change in the chemical forces, and it be supposed that the same attraction is exerted between any given pair of atoms without regard to the state of combination of one or both of the atoms, then a reasonable account can be given of chemical reactions, and the existence of molecular combinations does not appear very remarkable. Further, we are provided with a more or less effective explanation of the relative volatility of substances. Reasoning from this point of view, and considering the carbon compounds specially, the author concludes that intermolecular attraction should be greater in an acid than in a corresponding alcohol; greater in an alcohol of high molecular weight than in a homologue of which the molecule is less complex; greater in a primary alcohol than in the secondary or tertiary isomeride; and finally, greater in a chlorinated compound than in the corresponding substance containing hydrogen. If the volatility of a substance be a measure of the forces of attraction between the molecules, then it must be admitted that the boiling points of organic compounds show with some reason that the above expression represents the value of intermolecular attraction. From this point of view the study of the latent heat of many carbon compounds would materially aid us in the solution of the problem of chemical affinity.

On the Relative Atomic Weights of Silver, Manganese, and Oxygen, by Prof. Dewar, M.A., F.R.S., and A. Scott, B.A., B.Sc. The authors have determined the atomic weight of manganese, in relation to silver and oxygen, by a complete analysis of silver permanganate. Taking the atomic weight of silver to be 108, that of oxygen to be 16, the following values have been obtained for the atomic weight of manganese, 55·51, 54·04, 54·45. These numbers do not agree very well amongst themselves, nor with the numbers obtained from the analysis of pure manganese peroxide, made from manganese nitrate. By this latter method the following values were obtained, viz., Mn = 53·6 and 53·3.

Note on the Phosphates of Lime and Ammonia, by J. Alfred Wanklyn.—When ammonia is added to a soluble calcic phosphate a precipitate is obtained, which dissolves on heating, forming a viscid solution which solidifies on cooling. The soluble salt formed the author regards as having the composition $P_2O_5CaO(NH_4)_2OH_2O$. The author confirms Morfit's observation, that bone earth dissolved in acid is reprecipitated by alkalies in the form of a hydrated tribasic phosphate; a fact which he considers of great importance to agriculture.

On the Separation of Hydrocarbon Oil from Fat Oils, by A. H. Allen, F.C.S.—The author pointed out that the extensive production of hydrocarbon oils and their cheapness had led to their being employed for the purpose of adulterating animal and vegetable oils. Indications of their presence are afforded by the determination of the density of the oil, by the lowering of its flashing point and boiling point, further by its taste and the odour produced on heating. An oil so adulterated is not completely saponified, and the hydrocarbon oil may be removed from the product of saponification by extraction with ether. Fluorescence is also to some extent a useful indication of the presence of such a mineral oil; the fluorescence of some mineral oils may, however, be destroyed by chemical means, and as some mineral oils are not fluorescent the absence of fluorescence in an oil does not therefore indicate the absence of a mineral oil.

On Bowkett's New Thermograph, by W. Lant Carpenter, B.A., B.Sc., F.C.S.—This is an instrument for recording changes of temperature, which are measured by the action of heat upon a hollow circular metallic ring connected with a circular vessel, the whole being filled with fluid and hermetically sealed. One end of the ring is fixed, the other is free to move, and its motion is magnified by a series of levers, to the end of which is attached a recording pen. Increments of heat cause increments of pressure in the ring, which moves at its free end. The instrument has hitherto been used for clinical purposes only, but the author thinks it might with advantage be used in chemical and physical researches.

The Blowing Wells near Northallerton, by T. Fairley, F.R.S.E.—The author gave an account of a series of observations on these wells, of which there are three in the neighbourhood of Northallerton. The gas issuing from the fissures in these wells has been analysed, and is apparently nothing more than common air.

On the New Metal Actinium, by J. L. Phipson.—The author stated that he had been able to separate a new element from the pigment zinc-white. The oxide of the new element is said to be slightly soluble in caustic soda, and is soluble in ammonia and ammoniacal salts. Its colour is uninfluenced by exposure to light. The sulphide of actinium is described as a pale yellow canary-coloured substance; it is insoluble in ammonium sulphide, is soluble in acetic acid, and becomes darker on exposure to the air.

On some Vapour-Density Determinations, by Prof. Dewar, M.A., F.R.S., and A. Scott, B.A., B.Sc.—The authors described the apparatus they employed for the determination of the vapour-densities at high temperatures, and the means adopted for examining the vapours to ascertain whether or not decomposition had taken place. The vapour-densities of the halogen compounds of several metals have been determined, and it is interesting to note that the authors find, according to its vapour-density, the molecular formula for ferrous chloride is $FeCl_2$.

Some Remarks on Crystallogeny, by Prof. J. P. Cooke (Harvard University, U.S.A.).

On a New System of Blow-pipe Analysis, by Lieut.-Col. Ross.—The author described his system of blow-pipe analysis, and exhibited a compact form of blow-pipe and other necessary apparatus for use when travelling.

On Experiments with Manures on Barley Crops, Season 1881, by Ivison Macadam, F.C.S.—The author gave a detailed account of his experiments on two fields sown with barley in April of this year. The previous rotation of crops was as follows:—in 1878 potatoes with 20 tons of farmyard manure per acre, and 4 to 5 cwt. of dissolved manure; in 1879 wheat, no manure; in 1880 turnips, 20 tons per acre of town ashes and 5 to 6 cwt. of dissolved manure. The only difference between the two fields was that in one case the turnips were carted away, whilst in the other they were eaten on the ground by sheep. From the time when the barley appeared to the time of cutting, determinations of the following points were made every week: (1) the weight of the plant; (2) length of straw; (3) rate of storage of saline matter by the plant; (4) amount of water, nitrogen (given as ammonia), organic and vegetal matter; (5) the amount of the various saline ingredients present in the ash. The results appear to show that, in the case of the field where the turnips had been eaten on the ground, the barley crop grew more rapidly and was more healthy than that on the other field.

SECTION C—GEOLOGY

On the Laurentian Beds of Donegal and of other Parts of Ireland, by Prof. Edward Hull, LL.D., F.R.S., &c., Director of the Geological Survey of Ireland.—After a perusal of the writings of previous authors, and a personal examination made in the spring of 1881, in company with two of his colleagues of the Geological Survey, Mr. R. G. Symes, F.G.S., and Mr. S. B. Wilkinson, the author had arrived at the following conclusions:—1. That the Gneissose series of Donegal, sometimes called "Donegal granite," is unconformably overlaid by the metamorphosed quartzites, schists, and limestones which Prof. Harkness had shown to be the representatives of the Lower Silurian beds of Scotland (*Quart. Journ. Geol. Soc.*, vol. xvii. p. 256). This unconformity is especially noticeable in the district of Lough Salt near Glen. 2. That the Gneissose series is similar in character and identical in position and age with the "Fundamental Gneiss" (Murchison) of parts of Sutherlandshire and Ross-shire, and is therefore, like the latter, presumably of Laurentian age. That the formation is a metamorphosed series of sedimentary beds, has been shown by Dr. Haughton and Mr. R. H. Scott. 3. That the north-western boundary of the Donegal gneiss is a large fault between the Laurentian gneiss and the metamorphosed Lower Silurian beds, owing to which the older rocks have been elevated, and by denudation have been exposed at the surface. 4. That the Cambrian formation of Scotland is not represented in Donegal, and that the unconformity above referred to represents a double hiatus, and is of the same character as that which occurs in Sutherlandshire, in the district of Fornaven and Ben Arkle, where the Lower Silurian beds rest directly on the Laurentian gneiss. 5. That Laurentian rocks may be recognised in other parts of Ireland, as in the Slieve Gamph and Ox Mountains of Mayo and Sligo, at Belmullet, and in West Galway, north of Galway Bay, where the rocks consist of red gneiss, horneblende rock, and schist, &c., similar to those in Donegal; also possibly in Co. Tyrone, as suggested by Mr. Kinahan.

Laurentian Rocks in Ireland, by G. H. Kinahan, M.R.I.A., &c.—The writer first mentioned that Cainozoic and Mesozoic rocks only occurred in the province of Ulster, while in the rest of the island there was a nearly continuous sequence of Palæozoic rocks, proved by the work of Griffith, Jukes, and their subordinates, from the Coal-Measures down to the Cambrian. He then pointed out that a recent attempt had been made to try and disturb their natural order, but that the new theory was solely founded on assertions that would not bear investigation. He proceeded to observe that the geologists of the pre-Cambrian school appeared to lay more weight on lithological evidence than that to which it was entitled, and in continuation he gave the localities for the oldest rocks in Ireland, with the reasons for and against the rocks being Laurentian. The localities are *Carngore*, or South-East Wexford, while it was shown that although the rocks were lithologically similar to the Laurentians, yet they contained Cambrian fossils—*Galway, South-East Mayo, Sligo, and Leitrim*—rocks that, from their lithological characters, were said to be Laurentian by Murchison, who recanted his statement when Harkness showed that stratigraphically this was an impossibility. These rocks occur on two zones, those on the highest being now said to be Laurentian—*Erris, North-West Mayo*—very old rocks, about which nothing can be positively said, except that they are older than the associated metamorphic rocks, also of uncertain age. *Donegal, Londonderry, and Tyrone*—the Laurentian age of some of these, years ago, was suggested by Jukes, while now it is positively asserted, but solely on lithological characters. The author pointed out that, although lithologically very like Laurentians, they were more like Huronians, Logan's description of the latter being very suitable for those of Donegal. He also pointed out that it was unnecessary to make vague assertions, as the stratigraphical position of the rocks ought to be easily worked out, either by starting from the Pomeroy fossiliferous rocks, or from the fossiliferous rocks found in Donegal by Dr. King; but that, at the same time, the work must be much better and more correctly done than that in the neighbourhood of Pomeroy, where the unaltered fossiliferous beds are classed with those they lie on, although the latter were extensively metamorphosed, contorted, upturned, and denuded, prior to the fossiliferous rocks being deposited on them. *North-East Antrim* rocks, supposed to be of the same age as the older rocks near Pomeroy (*Upper Cambrians*).

On the Occurrence of Granite in situ about Twenty Miles South-West of Eddystone, by A. R. Hunt, M.A., F.G.S.—The author described and exhibited a fragment of granite brought up by a Brixham trawler twenty miles south-west of the Eddystone. He believed it to have been torn off a mass of granite *in situ*, and pointed out that in mineral composition it agreed with the gneisses of the Eddystone Reef and of the Shovel Reef in Plymouth Sound—all these rocks being composed of mica, quartz, and felspar, without hornblende or schorl. The author believed that the occurrence of gneiss in Plymouth Sound without altering the adjacent Devonian rocks was an indication that these Channel typical gneisses, and probably the typical granites too, were of pre-Devonian age.

Some Observations on the Causes of Volcanic Action, by J. Prestwich, M.A., F.R.S., &c., Professor of Geology in the University of Oxford.—The hypothesis generally accepted in this country as to the cause of volcanic action is that of the late Mr. Poulett Scrope, who considered that "the rise of lava in a volcanic vent is occasioned by the expansion of volumes of high-pressure steam, generated in a mass of liquefied and heated matter within or beneath the eruptive orifice," and that the expulsion of the lava is effected solely by high-pressure steam generated at great depths, but at what depths is not mentioned, nor is it explained how the water is introduced, whether from the surface, or whether from water in original combination with the basic magma. The objections to this hypothesis are—1. That during the most powerful explosions, *i.e.*, when the discharge of steam is at its maximum, the escape of lava is frequently at its minimum. 2. That streams of lava often flow with little disengagement of steam, and are generally greatest after the force of the first violent explosion is expended. 3. That it is not a mere boiling over, in which case, after the escape of the active agent—the water—and the expulsion of such portion of the obstructing medium, the lava, as became entangled with it, the remaining lava would subside in the vent to a depth corresponding to the quantity of lava ejected; but the level of the lava, *cæteris paribus*, remains the same during successive eruptions. Of the important part played by water in volcanic eruptions there can be no doubt, but instead of considering it as the primary, the author views it as secondary cause in volcanic eruptions. All agree in describing ordinary volcanic eruptions as generally accompanied or preceded by shocks or earthquakes of a minor or local character, to which succeed paroxysmal explosions, during which vast quantities of stones, scoriæ, and ashes, together with volumes of steam, are projected from the crater. The first paroxysms are the most violent, and they gradually decrease and then cease altogether. The flow of lava, on the other hand, which commences sooner or later after the first explosions, is continued and prolonged independently. Ultimately the volcano returns to a state of repose, which may last a few months or many years. Adopting the theory of an original igneous nucleus, the author considers a certain fluidity of the former, and mobility of the latter. The one and the other feebly represent conditions of which the phenomena of the rocks afford clearer and stronger evidence as we go back in geological time. Although thermometrical experiments, of the necessary accuracy and length of time, are yet wanting, it has been estimated that a small quantity of central heat still reaches the surface and is lost by radiation into space, and the escape of liquid lava and steam from volcanoes, and of hot springs from these and other sources, must bring, in however small a quantity, a certain increment of heat from the interior to the surface, where it is lost. This should lead to a certain contraction at depths, and of readjustment of the external crust, in consequence of which the fused masses of the interior will from time to time tend to be forced outwards, whenever tension became sufficient to overcome resistance. In this the author agrees with many other geologists. The further hypothesis respecting volcanic action, he now suggests, he has been mainly led to form by his researches on underground waters. A portion of the rain falling on the surface not only of permeable and fissured sedimentary strata, but also of fissured and creviced crystalline and other rocks, passes below ground, and is there transmitted as far down as the permeable rocks range, or as the fissures in the rocks extend, unless some counteracting causes intervene. Those causes are the occurrence of impermeable rocks, faults, and heat. The former two are exceptional, the latter constant. The increase of temperature with depth being 1° Fahr. for every 50 to 60 feet, the boiling point of water would be reached at a depth of about 10,000 feet, but owing to the pressure of the superincumbent rocks, it has been estimated that water will retain its liquidity and continue to

circulate freely to far greater depths. Unfortunately, very little is known of the substrata of volcanoes. Etna and Hecla apparently stand on permeable Tertiary strata, Vesuvius on Tertiary and Cretaceous strata, while in South America some of the volcanoes are seemingly situated amongst palæozoic and crystalline rocks. Under ordinary circumstances all the permeable strata and all fissured rocks become charged with water up to the level of the lowest point of escape on the surface, or if there should be an escape in the sea-bed, then to that level, *plus* a difference caused by friction. The extreme porosity of lavas is well known. All the water falling on the surface of Etna and Vesuvius (except where the rocks are decomposed and a surface soil formed) disappears at once, passing into the fissures and cavities formed by the contraction of the lava in cooling. Not only are these fissures filled, but the water lodges in the main duct itself, and occasionally rises to a height to fill the crater. Beneath the mass of fragmentary and cavernous volcanic materials forming the volcano, lies the original compact mass of sedimentary strata, &c. Owing to the fortunate circumstance of an Artesian well having been sunk at Naples, we know the underlying sedimentary strata there to consist of alternating strata of marl, sands, and sandstones, some water-bearing, others impermeable. The water from the lowest spring reached in this boring rose at first 8 feet above the surface, and 81 feet above the sea-level. Where the strata crop out in the sea-bed, the same pressure of the column of inland water forces the fresh water outwards, so as to form a freshwater spring in the sea, as at Spezzia and elsewhere on the Mediterranean coast. It is this fundamental hydrostatic principle which keeps wells in islands, and in shores adjacent to the sea, free from salt water, as in the Isle of Thanet. Where, however, the head of inland waters is small or impeded, sea-water will enter the permeable strata, and spoil the springs, as in the case of the Lower Tertiary sands at Ostend, and the Lower Greensand at Calais and in the Somme, in which latter department the underground spring was found affected to a distance of about one mile from the sea, but pure at a distance of nine miles. Further, if where the head of inland water is sufficient to force back the sea-water under ordinary conditions, those ordinary conditions are disturbed by pumping to an extent that lowers the line of water-level to below that of the sea-level, then the sea-water will flow inwards until an equilibrium is established. The flow of water under a volcanic mountain may be also influenced by the quaquaversal dip, which there is some evidence that the underlying strata there take, owing probably to the removal of matter from below, and the weight of the mountain. If we are to assume that the volcanic ashes and tufas below Naples are subaerial, the original land-surface has sunk not less than 665 feet, and a dip of the underlying strata from the seaward, as well as from inland, has in all probability been caused. This Artesian well was carried to the depth of 1524 feet, and passed through three water-bearing beds—one in the volcanic ashes, the second in the sub-Apennine beds, and the third in the Cretaceous strata at the bottom. No eruption of lava can then take place without coming in contact with these underground waters. The first to be affected will be the water in the cavities of the mountain in and around the crater. As the pressure of the ascending column of lava splits the crust formed subsequently to the preceding eruption, the water finds its way to the heated surface, and leads to explosions more or less violent. When the fluid lava breaks more completely through the old crust, and the mountain is fissured by the force and pressure of the ascending column, the whole body of water stored in the mountain successively flows in upon the heated lava, and is at once flushed off into steam. Then take place those more violent detonations and explosions—those deluges of rain arising from the condensed steam—with which the great eruptions usually commence. In conclusion, the author conceives that the first cause of volcanic action is the welling up of the lava in consequence of pressure due to slight contraction of a portion of the earth's crust. Secondly, the fluid lava coming into contact with water stored in the crevices of the masses of lava and ashes forming the volcano, the water is at once flushed into steam, giving rise to powerful detonations and explosions. Thirdly follows an influx of water from the underlying sedimentary or other strata lying at greater depths into the ducts of the volcano; and, lastly, as these subterranean bodies of water are thus converted into steam and expelled, the exhausted strata then serve as a channel to an influx of sea-water into the volcano. A point is finally reached when, owing to the cessation of the powerful shocks and vibrations, and the excessive drainage of the strata, the flow of the lava is effected quietly, and so continues until another equilibrium is established and the lava ceases to escape.

The Connection between the Intrusion of Volcanic Action, by Prof. W. J. Sollas, M.A.—In a volcanic eruption there are concerned first the elevation of the lava column in the axial pipe of the volcano, and next the explosion by which the lava is ejected into the air. The author attempts to find a *vera causa* for the latter. Sorby's researches on included cavities prove that steam at a high tension must have been everywhere present throughout plutonic rocks when these were in a state of fusion, and the presence of steam in ejected lava is well known. He considers it probable that the axial pipe of a volcano is occupied by fused rocks permeated by steam, which is probably in a liquid state, and the tension of which will depend on the hydrostatic pressure due to the lava column above it. Any sudden diminution of this pressure will tend to a sudden expansion of the steam, and tend to produce a volcanic explosion. The mere elevation of the lava in the volcanic pipe cannot directly produce a diminution of pressure, though an overflow at the surface of the ground would, but this infers that the overflow of lava should precede an eruption, which is not the case; hence the author concludes that an overflow of lava from the sides of the pipe and other places underground, and the pressure on the lava column being reduced beneath the point of overflow, an eruption follows. The ascendant pressure of intruded sheets and dykes of igneous rock known to occur beneath volcanic cones thus stands in close connection with the production of volcanic explosions.

A Restoration of the Skeleton of Archæopteryx, with some Remarks on Differences between the Berlin and London Specimens.—Prof. H. G. Seeley, F.R.S., traced the forms of the bones from a photograph, and arranged the skeleton so as to represent a bird which stood about ten inches high. The head has a post-occipital process in the cormorants; the neck is curved forward; the tail reached almost to the ground; and the limbs were exactly as in birds.

On Simosaurus pusillus (Fraas), *a Step in the Evolution of the Plesiosauria.*—Prof. H. G. Seeley gave a detailed description of the skeleton of Simosaurus recently discovered in the Trias near Stuttgart, and briefly noticed and figured by Dr. Oscar Fraas. He then drew special attention to the difference from Plesiosaurus, especially in the form of the pectoral arch and in the characters of the fore and hind limbs. The hind limb was discussed, to show how it might assume like character with the fore limb. Prof. Seeley concluded that the Plesiosaurus were originally land animals, and that their ancestors and affinities must be sought in Simosaurus, Nottrosaurus, and allied types of amphibious Triassic reptiles.

Influence of Barometric Pressure on the Discharge of Water from Springs, by Baldwin Latham, M.Inst.C.E.—The author of this paper mentioned that it was alleged, by some of the long-established millers on the chalk streams, that they were able to foretell the appearance of rainfall from a sensible increase in the volume of water flowing down the stream before the period of rainfall. He had, therefore, undertaken a series of observations to investigate the phenomena, and he found, in setting up gauges in the Bourne flow in the Caterham Valley, near Croydon, in the spring of this year (1881), and selecting periods when there was no rain to vitiate the results, that whenever there was a rapid fall in the barometer, there was a corresponding increase in the volume of water flowing, and with a rise of the barometer, there was a diminution in the flow. The gaugings of deep wells also confirmed these observations; for where there was a large amount of water held by capillarity in the strata above the water-line, at that period of the year when the wells became sensitive and the flow from the strata was sluggish, that a fall in the barometer coincided with a rise in the water-line, and that under conditions of high barometric pressure the water-line was lowered. Percolating gauges also gave similar evidence, for after percolation had ceased and the filter was apparently dry, a rapid fall of the barometer occurring, a small quantity of water passed from the percolating gauges. The conclusion arrived at was, that atmospheric pressure exercises a marked influence upon the escape of water from springs.

On Evaporation and Excentricity as Co-factors in the Causes of Glacial Epoch, by the Rev. E. Hill, M.A.

On some Points in the Morphology of the Rhabdophora, by John Hopkinson.—The author, after reviewing the characteristics of the group, concludes from his investigation into the morphology of this group that they are the Palæozoic representatives of the recent Hydroida.

The Glacial Deposits of West Cumberland, by J. D. Kendall, C.E., F.G.S.—The extent, form, and inner nature of these deposits is first described; a number of new and important facts being brought forward on the distribution of boulders both in the boulder clays and in other glacial deposits. The conclusions arrived at from the facts are (1) that the boulder-clays were formed in the sea, partly by glacier action and partly by icebergs. The occurrence of boulders from distant localities, often in very different directions, in a matrix partaking of the character of the underlying rocks, is explained in an entirely new way. 2. That the middle sands and gravels are the result of marine and river action combined. 3. That the mounds of sands and gravels occurring in the mouths of valleys were accumulated by floating ice from pre-existing deposits. A somewhat novel explanation is given of the occurrence of boulders on higher levels than the rocks from which they were derived.

On "Flots," by J. R. Dakyns, M.A., Geological Survey of England and Wales.—The word "flot" is a miner's term for ore lying between the beds, or at certain definite horizons in the strata. In text-books flots are generally called "flats" or "flattings." They are of two kinds: (1) those connected with "cross-veins"; (2) those connected with courses of dun limestone. Firstly, cross-veins are veins (generally mere spar veins on Greenhow Hill) which cross and intersect or shift the metal veins, but which often bear ore at their intersection with the metal veins. Where these cross-veins cut the flot planes, ore is found. Secondly, similarly with courses of dun limestone. Dun limestone, so-called from its colour, is a dolomitised form of ordinary limestone. The dun lime occurs in beds or irregular masses, or more frequently in dyke-like courses, running north-north-west and south-south east. These courses are often several yards or even fathoms wide, and where the dun course crosses the flot plane ore is developed along the joints between the dun and the white limestones. Ore is not found along the flot plane except at its intersection with the cross-veins or with the courses of dun limestone.

On the Lower Cambrian of Anglesea, by J. McK. Hughes, Woodwardian Professor, Cambridge.—In this paper the author gives the results of further examination of the basement beds of the Cambrian, which he has now traced all along the north-west flank of the Archæan axis of Llanfaelog. The sequence he found almost invariably was in ascending order:—(A) Quartz conglomerate passing up into (B) grit, which in turn becomes finer, and passed into (C) sandstones weathering brown, which got split up in their upper part by thin slabby shales; (D) black shales with subordinate beds of black (D 2) breccia, and occasionally sandstone in the lower part.

On the Gnarled Series of Amloch and Holyhead in Anglesea, by T. McK. Hughes, Woodwardian Professor, Cambridge.—The author offers the results of his inquiries into the age of certain schists which form the main mass of the rocks of northern and Western Anglesea, leaving for the present the consideration of the masses of somewhat similar rock which occur south of the Llanfaelog gneissic axis in the central and south-east part of the island. The author believes these felspathic gnarled rocks must be either the marine equivalents of the Bala volcanic series, or the result of a later (probably Silurian) denudation of those beds. As Lower May Hill (= Birkhill) fossils only occur in the slates immediately south of the area in question, the latter supposition is the only one tenable in the present state of the evidence.

Notes on the Subsidences above the Permian Limestone between Hartlepool and Ripon, by A. G. Cameron, Geological Survey of England and Wales.—In this paper attention is drawn to the numerous forms of shrinkages of the land-surface, often extending to considerable depths into the rocks beneath, observable over the top of the Permian rocks betwixt Hartlepool and Ripon. As a general explanation of their origin, it is suggested that where the underground water, flowing over the limestone surface, reaches the margin of the sandstone, it receives a check whereby it accumulates, forming a chain of dams or pools along the line of junction of these rocks. As denudation proceeds, hollows form above, until ultimately the phenomena of the pits appear. This being so, "the water bubbling and frothing all over" is explained without calling in the aid of river-action. Allusion is made to the Home Farm Colliery accident at Hamilton, N.B., in February, 1877, through a subsidence in the gravelly alluvium of the Clyde; also to the recent subsidences at Blackheath, near London, and to the extensive caverns in the hematite districts of Furness.

The Great Plain of Northern India not an old Sea-Basin, by W. T. Blanford, F.R.S.—The author described the distribution of land in the Indian Peninsula and the intervention of a vast plain traversed by the Indus, Ganges, and Brahmaputra. This plain has constantly been considered, both by geological and lithological writers, as the basin of a great sea; but on examining the evidence, there does not appear to be a single fact in favour of the sea having at any geological period occupied the Gangetic or eastern position of the plain. The tract is evidently an area of depression filled up to above sea-level, through a long period of geological range of time.

The Gold-Fields and the Quartz Outcrops of Southern India, by William King, Deputy Superintendent (for Madras), Geological Survey of India.—The paper is a *résumé* of the knowledge ascertained through the author's original survey of the Wainád gold-field in 1874 and by the later surveys and examinations of others; also in his examination of the Travancore and other areas in the beginning of the year. The geographical distribution of the gold areas is briefly treated of as being at Manyapet, on the Godávari River, near Dúmbal, in the South Mahratta country, near Kolár in Mysore, at Salem, in part of the Travancore State, and in the Nilgiri and Malabar country; and these are reduced to the more important fields of Malabar (including Wainád, and the Nilgiris) and Mysore. The reefs of Wainád are developed to a remarkable extent over a very large area of country; but their gold-bearing quality is only displaced over a portion of this, chiefly in the south east of Wainád and in the adjacent low country of Malabar, in a generally east and west belt, the reefs outside of this being fewer and only very locally auriferous. The "leaders" or offshoots of the reefs in this belt are strongly and numerously developed, and they and the "easing" are rich in gold. The author expects the gold-yield to be seven pennyweights to the ton. He does not think that a paying return can be obtained on less than three pennyweights of gold to the ton.

Geology of the Island of Cyprus, by R. Russell, C.E.—The author described the physical features of the island as consisting of two great mountain chains, the axes of which are mainly parallel to each other, distinct from each other in structure and in physical matter. The southern range, rounded in outline, rises to 6340 feet; the northern range rises up from hummocky ground, on both sides, as it were, in one great continuous wall-like cliff. The central area consists of flat-topped irregular hills rising abruptly from the low ground, and therefore show more prominently than they would otherwise. The rocks which occur may be classified as follows:—

Post-Tertiary.	Blown sand.	
	Alluvium (vent).	
	Kavara (solidified surface).	
	Raised beach.	
	Sand and gravel (old river deposits).	
	Calcareous tuff and travertine.	
Tertiary.	Pliocene	Kerynia rock.
		Nicosia beds.
	Miocene	Idalian beds.
Secondary	Upper Cretaceous.	Konnos.
	Jurassic	Mount Hilarion limestone.
	Igneous rocks.	

The last upheaval of the island took place in a comparatively recent period, and was not more than fifteen or twenty feet in vertical height.

On some Sections in the Lower Palæozoic Rocks of the Craven District, by J. E. Marr, B.A., F.G.S.—The author showed by means of a thin band containing *Phacops elegans*, Bœck and Sars, that a series of beds consisting of pale green shales, underlain by black shales, passing below into a conglomerate which rested unconformably upon the Bala beds (the whole exposed in Austisich Beck, near Settle), were the equivalents of the Stockdale shales of the Lake District, and of the May Hill beds of the Continent. The beds are lithologically similar to those of the Lake District, and, like them, are surmounted by blue flags containing *Monograptus priodon* and *M. vomerinus*.

Life in Irish and other Laurentian Rocks, by C. Moore, F.G.S.—The author drew attention to certain forms found by a microscopic examination of specimens of certain Laurentian and other Palæozoic limestone prepared by trituration, solution in acid, and washing. These forms were clearly those of organic structures, some apparently hairs and other feather barbs. The author considered that he had taken precautions to eliminate

sources of error, through admixture of foreign materials, and he was led to think that the organisms belonged to the rocks.

The Subject-matter of Geology and its Classification.—Prof. W. J. Sollas, M.A., stated his object was to remove certain prevailing misconceptions as to the aim and scope of geology. The accepted definition of geology as "the history of the earth's crust and the fossils it contains," was considered to be both too wide and too narrow; the former since it includes palæontology, which, so far as it is a study of forms of life, belongs to biology; and too narrow, since the science of the whole, necessarily embraces much more than a study of its crust. Geology is one of the group of concrete sciences which include astronomy, geology, and biology. The scope of geology, or the science of the earth, is so wide, that a fresh classification of its subject-matter is required, and the author proposes *Morphological Geology*:—embracing geography, petrology, lithology, and mineralogy corresponding to anatomy and histology in biology; minerals, rocks, rock masses, constituting the earth's crust as cells, tissues, organs constituting living organisms, while palæontology is a study of successive morphological states, corresponding to embryology or development. *Physiological Geology*, considering the movement of the earth as a whole, and of all activities produced upon it, by extrinsic and intrinsic forces, acting singly or in combination; it rightly includes meteorology, hydro-geology, as well as the physiology of the earth's crust. *Distributional Geology* seeks to determine the distribution of the earth in time and space, and *Otiological Geology* corresponds roughly to what is known as cosmogony.

Exploration of a Fissure in the Mountain Limestone at Raygill, by James W. Davis, F.G.S.—Attention was first called to this fissure by Mr. Tiddeman about eight years ago. It occurs in a quarry in Lothersdale, about five miles from Skipton. The mouth of the cavern is blocked with glacial drift; under this occurs a finely laminated clay, beneath which is a brown sandy clay with well-worn boulders. The fissure, when excavated, proved to be forty feet in length, horizontal, with a second branch, both of which are abraded and smoothened by the action of running water. Contains bones of *Elephas*, teeth of *Hippopotamus*, *Rhinoceros leptorhinus*, remains of the roebuck and hyæna, and one or two teeth of lion, and a single tooth of bear.

On the Zoological Position of the Genus Petalo-rhynchus, Ag., *a Fossil Fish from the Mountain Limestone*, by J. W. Davis.—The species described resemble genera *Janassa*, Munst., and with it appear to occupy an intermediate position between the genera *Myliobatis* and *Cestraciontes*.

On Diodontopsodus, Davis, *a New Genus of Fossil Fishes from the Mountain Limestone at Richmond in Yorkshire*, by James W. Davis, F.G.S.—These teeth resemble those of the modern fish Diodon.

Preliminary Remarks on the Microscopic Structure of Coal from East Scotland and South Wales, by Prof. Williamson, F.R.S., Owens College.—This subject will not be worked out until ten years, but he described layers of vascular tissue which can be separated layer by layer, while in other cases the charcoal layer on the surface of the coal and the organic structure is not capable of separation, and he stated that charcoal contains a tubular structure, like tissues of ordinary bark. The association of tissues resembles that of Cycadian plants; and referred to the genus *Cordaites* having been proved to belong to this group by M. Renault; the author has made nearly a thousand distinct observations on the structure of coal. Separates ordinary coal with large quantities of mineral charcoal, with macrospores of Lepidendrold plants filled up with myriads of microspores which were certainly not floated to the spots, from the *paraffine coals* which do not contain these large macrospores. He divides coals into "Iso-sporous" coals and "Heterosporous" coals; both abound in *Cordaites*, which form the mineral charcoal.

On an International Scale of Colours for Geological Maps, by W. Topley, Geological Survey of England.—The author described the objects of the International Geological Congress which is to meet at Bologna this month. Three main subjects are there to be discussed, (*a*) colours and signs for geological maps, (*b*) nomenclature of rocks and formations, (*c*) nomenclature of species. This paper is concerned only with the first of these questions, and especially with the resolutions passed by the English Map Committee, of which Prof. Ramsay is president, and the author secretary. At present all countries and many map-makers in each country have different systems of colouring maps, and it is necessary carefully to study the index, or scale of colours used, before the map can be at all understood. The Congress proposes to frame some scheme of colouring which can be used and readily understood by all nations. It may not be possible, at least for some time to come, to obtain any alteration in national surveys in progress. But it is to be hoped that in new small-scale maps the scheme to be decided on will be adopted. One important point which the Congress proposes is the preparation and publication of a general map or atlas of Europe, compiled under the authority of the Congress, from the various national surveys and the work, of independent observers. The scheme of colouring proposed is one based on the order of colours in the solar spectrum, violet denoting the older rocks. Bright reds are reserved for igneous rocks; metamorphic rocks will be shown by dark bands of colour over the colour denoting the age; to these will be added bands of colour showing the period at which metamorphism has taken place, when such fact is clearly established: thus, Silurian rocks metamorphosed in Cretaceous time would be shown by violet striped with alternate lines of dark violet and green. The sub-divisions of a formation will be shown by shades of the body colour, the darkest shade denoting the oldest subdivision. The letter denoting the formation will be the capital initial letter of the name of the formation; with very small arrangements one system of lettering can be made to apply to all countries. It has been found impossible to adhere strictly to the order of colours of the spectrum, and an interpolation has been made of browns and greys for the series of beds between the Silurian and the Lias. Examples of maps and tables of strata coloured according to the plan adopted were exhibited, as were also a series of Indexes of Colours issued at various dates by the Geological Survey, commencing with one in MS. by Sir H. de la Bêche in the year 1832. The author also drew attention to a proposal made by Mr. J. W. Salter before this Association in 1847, and again at the International Exhibition in 1862, to colour geological maps in the order of colours of the solar spectrum. The plan recommended by the English committee differs considerably in detail from that of Mr. Salter.

On the Rhætics of Notts, by E. Wilson, F.G.S.—The author gave a summarised account of the Rhætic series in Nottinghamshire. The Rhætic sections of this district already known to geologists comprise those at Gainsboro', Newark, and Elton. The author described several additional new sections in the Rhætics of the county—viz. at Cotham and Kilvington between Newark and Bottesford; at Barnstone, between Bingham and Stahern; the boring for coal at Owthorpe, near Colston Bassett; and the section at Stanton-on-the-Wolds, between Nottingham and Melton Mowbray. A list of the Rhætic fossils of Notts was given, and the presence of bone-beds noticed. The author could not agree with certain geologists that the green marls which are found beneath the Paper shales in Notts (nor probably also the "Tea green marls" of the West of England) belong to the Rhætic series, but took them to be Upper Keuper marls, once red in colour, which had become discoloured by some deoxidising agent, probably carbonic acid evolved during the decomposition of the organic matters of the fossils of the Paper shales. For, in lithological character the green marls agreed with underlying beds in the Keuper, but differed markedly from the overlying Rhætics; then there was every appearance of a passage between the green marls and the underlying red and green marls of the Keuper; and, lastly, the green marls, like the rest of the Keuper marls, were practically unfossiliferous, while with the commencement of the Paper shales we get the remains of an abundant and distinctly marine fauna, in part Liassic.

Notes on the Cheshire Salt-Field, by C. E. De Rance, F.G.S., of H.M.'s Geological Survey.—The author described the brine-springs of the Keuper marls in Cheshire and part of Shropshire as having been derived from rainfall absorbed at the line of the original outcrop of the beds of thick rock-salt, which is represented by a porous bed. These waters flow out by pressure in various natural springs, and are bored into by the wells or artesian shafts of the brine-pumpers. The natural solution of the rock-salt has caused the characteristic subsidences that occur in the district. Northwich subsidences, however, have been chiefly caused by bad mining.

On the Strata between the Chillesford Beds and the Lower Boulder Clay. "The Mundesley and Westleton Beds," by J. Prestwich, M.A., F.R.S., Professor of Geology in the University of Oxford.—The beds between the Chillesford Clay and the Lower

Boulder Clay present such a series. Its exhibition on the coast of Norfolk, although very limited, is accompanied by special palæontological features that have caused it to be divided into the number of local beds which have been described by Trimmer, Green, Gunn, Wood, and Harmer, the author, Reid, Blake, and others. It includes the "Laminated Clays" of Gunn, the "Bure Valley Crag" of Searles-Wood, the "Westleton Shingle" of the author, and the "Rootlet-bed" and "Norwich Series" of Blake. Without reverting at present to the exact correlation of the several beds in the Norfolk area, respecting which there is still some difference of opinion, the author suggests that they should be included under a general term founded on the localities where, on the one hand, their varied palæontological characters are exhibited, and on the other where their peculiar petrological characters are well marked—characters which the author proposes to show, in another paper, have a very wide range, and serve to mark an important geological horizon in some interesting questions of local physical geology. The Mundesley beds were described by the author in 1860, and consist of alternating beds of clay, sands, and shingle, some containing freshwater and others marine mollusca, with a forest-growth and mammalian remains at their base; and again in 1871, including them in his Westleton group (No. 5 in the author's sections), which he showed to consist entirely of great masses of well-rounded shingle, with intercalated seams containing traces only of marine shells. Seeing the inconvenience of attaching the same term to the two very distinct series of beds, and that it may conflict with other local terms, the author now proposes to group this series under the term of "The Mundesley and Westleton Beds," indicative of their stratigraphical position in Norfolk, and of characters in Suffolk which serve to trace them in their range westward and inland to considerable distances beyond the Crag area, to which alone these beds have hitherto been restricted. At the same time it may be convenient, for brevity, to use one term only in speaking of typical cases.

On the Upper Bagshot Sands of Hordwell Cliff, Hampshire, by E. B. Tawney, M.A., F.G.S.—The descriptions of former writers having been cited, it was found that there were two main views regarding the affinities of these sands, which occur in the cliff between Long Mead End and Beacon Bunny. The view formulated by the distinguished foreign geologists, D'Archiac, Dumont, Prof. Hebert, and Prof. C. Mayer, is that they are parallel to the upper sands of the Beauchamp (= Barton) period, and allied, therefore, to the marine Barton beds. This view is much the same as that of E. Forbes, and the Geological Survey, who called them the Upper Bagshot Sands. Latterly Prof. Judd has sought to revive the term Headon-Hill Sands for them, presuming them to be most nearly connected with the Headon series, and extending the bounds of that series to receive them. The author now gives a list of twenty-eight species obtained from the bed at Long Mead End; of these 35 per cent. are common to the sand and the Barton beds, but do not occur in the Headon series; while only 21·4 per cent. are common to the sand and Headon series, but do not occur in Barton beds. It is shown that this sand belongs to the zone of *Cerithium pleurotomoides*, Lam., and is exactly parallel to the sands of Mortefontaine, which belong to the same horizon, constituting the upper portion of the Beauchamp deposits. This is altogether below the *C. concavum* zone. From these sands being intimately connected with the Barton beds in both areas, it is held that the term Upper Bagshot is the most fitting designation that has been proposed for them.

NOTES

THE Emperor of Germany has, by Imperial Decree dated June 1, 1881, awarded the Gold Medal of Merit for Agriculture to Mr. Lawes and Dr. Gilbert jointly, in recognition of their services for the development of scientific and practical agriculture.

THE death is announced, at the age of sixty-two years, of Mr. Frederick Currey, F.R.S., F.L.S. Mr. Currey was well known as a botanist, and was secretary to the Linnean Society from 1860 to 1880. It is stated that Mr. Currey has left his valuable collections of fungi to Kew.

THE honour of knighthood has been conferred upon Dr. G. C. M. Birdwood, C.S.I., of the India Office; and also upon Dr. John Kirk, H.M. Political Agent and Consul-General at Zanzibar, well known as the friend of Livingstone, and naturalist to his second exploring expedition, and as having done so much to promote African exploration.

THE Sedgwick Memorial Fund (Cambridge) now amounts from subscriptions and interest to more than 14,000*l*., but this sum is not sufficient to build the new geological museum which it has been decided to erect in honour of the late professor. As, however, the present museum was built partly by subscriptions collected mainly through the exertions of Prof. Sedgwick, with a view to the erection of a geological museum, as well as of the library and other University buildings, the value of the portion occupied by the present museum should be taken into account in estimating the sum available for the new memorial building. An architect has been consulted as to the possibility of erecting a new geological museum and a chemical laboratory on the vacant space in front of the new museums and lecture-rooms facing Pembroke Street, but after examination of his plans and report it was found that the proposal could not be carried out, and it has consequently been decided to await the result of further negotiations for the purchase of the contiguous property. The recent acquisition by the University of some adjoining land will, it is hoped, diminish the difficulties now existing in the way of finding a suitable site for the erection of the new geological museum.

A LONG and interesting article in the *Daily News* of Tuesday describes the progress which has been made in carrying out the scheme of Mr. Holloway for the erection of a college for the education of young ladies. Mr. Holloway's endowment is of the amplest liberality; the building is all that could be desired, and is in a fair way of being completed; there is no danger of the institution becoming one for the benefit of the teachers and not of the students; the programme of education is meant to place science on a footing of absolute equality with learning. "The governing body will consist of twenty-one persons, to be appointed partly by the University of London, and partly by the Corporation of London, and it is stipulated that a certain portion shall always be women. Religious opinions are not in any way to affect the qualification for a governor. It is the founder's desire that power by Act of Parliament, Royal charter, or otherwise, should be eventually sought to enable the college to confer degrees after due examination; and that until such power is obtained the students shall qualify themselves to pass the Women's Examination of the London University, or any examination of a similar or higher character which may be open to women at any of the existing universities of the United Kingdom. The curriculum will not be restricted to subjects enjoined by any existing university. Instead of being regulated by the traditions and methods of former ages, the system of education will be mainly founded on studies and sciences which the experience of modern times has shown to be most valuable, and as best adapted for the intellectual and social requirements of students. The governors will therefore be empowered to provide instruction in any subject or branch of knowledge which shall appear to them, from time to time, most suitable for the education of women; and the curriculum of the college will not discourage students who may desire a liberal education apart from the Latin and Greek languages." All this is admirable, and we trust the spirit of the founder's wishes will be faithfully carried out. This building and the Sanatorium are not far from Virginia Water, and the total cost, with endowments, will probably amount to close upon a million.

IN connection with the Smoke Abatement Committee, an International Exhibition and trials of smoke-preventing appliances will be held in the East and West Arcades, and in buildings adjoining the Royal Albert Hall, at South Kensingto

from October 24 to November 26. Gold, silver, and bronze medals and certificates of merit will be awarded upon the report of a special committee. Regulations and forms of application for space may be had on application (by letter) addressed to Mr. Gilbert R. Redgrave, Superintendent of the Exhibition, Exhibition Buildings, Queen's Gate, South Kensington; or to Mr. W. R. E. Coles, Hon. Secretary to the Smoke Abatement Committee, 44, Berners Street, W.

THE Parkes Museum is closed until the end of September. In October it will again be opened free to the public on Tuesdays, Thursdays, and Saturdays, and during the winter lectures on sanitary science will be given in the Museum. The lectures will be illustrated with the sanitary appliances deposited in the Museum, which now include many new contributions sent from the recent Medical and Sanitary Exhibition at South Kensington. We believe it is intended to distribute the awards to the exhibitors at the Exhibition, at the second public annual meeting of the subscribers to the museum in October or November.

A NEW College of Practical Engineering has been opened at Muswell Hill, near London, under the auspices of a number of eminent practical engineers, among whom we may mention Sir John Anderson, late chief engineer at Woolwich, Sir Henry Bessemer, Sir R. M. Stephenson, Sir Joseph Whitworth, Bart., and Mr. Charles Manby, honorary secretary of the Institution of Civil Engineers. The principal of the College is Mr. John Bourne, C.E., author of several works on the Steam Engine and other kindred subjects. The instruction it is stated will combine the best theory with the best practice.

As a special number of the *Journal* of the Society of Telegraph Engineers, a valuable Guide-Book to the British Section at the Paris Electrical Exhibition has been issued, edited by Prof. W. E. Ayrton, F.R.S.

THE success of the Siemens electrical railway in Paris is very great, and the mode of locomotion very highly prized by Parisians. It is certain that steps will be taken after the Exhibition for rendering it a permanent feature of the French capital.

DURING the recent meeting of the British Association a conference of delegates from scientific societies was held, and the chair was occupied by Mr. W. Whitaker, F.G.S., Norwich Geological Society. The following resolution was adopted:—"That a committee be appointed, consisting of Sir Walter Elliot, F.R.S., Mr. H. George Fordham, Mr. John Hopkinson, Mr. G. J. Symons, F.R.S., and Mr. W. Whitaker, to arrange for a conference of delegates from scientific societies to be held at the annual meetings of the British Association, with a view to promote the interests of the societies represented by inducing them to undertake definite systematic work on a uniform plan; that Mr. Fordham be the secretary, and that the sum of 5*l*. be placed at their disposal for the purpose." An interesting conversation followed as to the best methods of stimulating the local societies to more active work. Mr. John Hopkinson, F.L.S., F.G.S., gave a most interesting account of the operations of the Hertfordshire Natural History Society. He had induced several members to take up the registration of the rainfall, and they had now twenty-eight observers of rainfall in their small county. Every one did not care about such observations, but there were plenty of other matters needing attention. Other members had been induced to take up the recording of the migration of birds, the flowering of plants, the appearance of insects, and other periodical phenomena; and the club furnished about one-third of the entire phenological observers of the Meteorological Society. They were also preparing lists of the fauna and the flora of the county, and one ornithologist was collecting a record of all the birds that are, or have been, observed in Hertfordshire. A resolution was unanimously adopted appointing Sir Walter Elliot and Messrs. Fordham, Hopkinson, Symons, and Whitaker a committee to arrange for the next Conference, and to send out a circular to the local scientific societies pointing out the work of the various committees of the British Association to which they might render aid, and other scientific work of a systematic character that they might usefully undertake.

AT a recent meeting of the Banburyshire Natural History Society Mr. E. A. Walford read a note "On the Occurrence of a Fire-ball at Watergall" on August 23 In answer to Mr. Walford's queries, Mr. Fessey, jun., had sent an account as follows, dated "Watergall, Leamington, August 30:—As regards the fire-ball, I was about 200 yards from it, in a waggon hovel. I saw it directly it left the sky, as I was looking in that direction at the time. When I first saw it, it looked like a ball of fire, about as large as a dinner-plate. It slowly descended, and I have no doubt I could have run twenty yards from the time I first saw it until it struck the ground; but when about fifteen to eighteen feet from the ground, it exploded with a loud crash, quite as loud as a cannon, distinctly before the thunder, which was very loud also. The explosion shook the whole buildings. I certainly thought the slates were falling in, but when it exploded one part struck the hedge, making a hole in the ground about a foot deep, and laying all the roots bare, but not damaging them. For some time the place looked all on fire, and there was a considerable quantity of smoke when it hit the ground, lasting for a second or two. It was seen by myself and four men. They also agree with me that this is as near as possible a correct explanation of it. We dug the hole out yesterday, but found nothing. The soil was blackened for several inches deep."

DR. C. S. MINOT, in a paper read at the Cincinnati meeting of the American Association, recommended the following method of mounting chick embryos whole. The blastoderm is removed and cleaned in the usual manner, and then floated out on a glass slide, where it remains permanently. It is carefully spread out and allowed to dry until the edges become glued to the slide. It is then treated with a 0·5 per cent. osmic acid solution, until a slight browning occurs. Stain with picrocarmine. The next step is particularly important, because it prevents the further darkening by the osmium, which otherwise injures or ruins the specimen. Pour Müller's fluid, or 0·5 per cent. chromic acid solution, on the slide, and leave it over night. The next morning the blastoderm is ready for dehydration by alcohol, and mounting in the usual manner in balsam or Dammar lac. Embryos prepared in this manner make particularly beautiful specimens.

THE winter session of the Charterhouse Science (the largest in the United Kingdom) and Art School and Literary Institute will, under the presidency of the Rev. Henry Swann, M.A., commence on September 24. During the late session about 700 students attended this institution; and of this number nearly 500 presented themselves for examination, and were successful in obtaining no less than 100 Queen's prizes. At a nominal fee instruction of a practical character is given in most of the sciences. Chemical students have the opportunity of working in a well-fitted laboratory capable of holding sixty students. During the session Mr. W. B. Carpenter, F.R.S., will deliver a course of lectures on physiology, to which teachers will be admitted free. This will prove a great boon to the teachers of the metropolis. Dr. Gladstone, F.R.S., Prof. Farrar, Mr. Sydney B. J. Skertchley, F.G.S., and others will lecture during the session.

THE Berlin Museum is now the fortunate possessor of archæological treasures which are perfectly unique. They are the long-expected sculptures from the Central American field of ruins at

Santa Lucia de Cosumalgapan, Guatemala, purchased for the Museum by Prof. Bastian when upon his American journey.

A SEVERE earthquake was felt three weeks ago in the southern part of the North Island, New Zealand. No lives were lost, but in some of the townships in the Manawater district scarcely a chimney was left standing. In Foxton, for instance, no less than 250 were thrown down. Fissures extending for many miles are reported to have been made, and the railway line was rendered unsafe in that neighbourhood, owing to the undulations of the earth alternately raising and depressing the rails. Since the large shock a good many of a slight nature have occurred. Two shocks of earthquake, each lasting from four to five seconds, were felt at noon on September 2 at Spalato in Dalmatia. The earthquake, which was accompanied by a subterranean rumbling, passed from the south-west to the north-east. It also made itself felt in the neighbouring islands of Brazza and Mascarsa, and in the town of Sebenico. A shock of earthquake was distinctly felt by several individuals at Courtown House, Gorey, Ireland, on August 27, at a quarter to five o'clock. Many heard a rumbling noise as of thunder, some noticed the rattling of doors and windows, and one experienced what he called a "shiver." Lord Courtown noticed a rumbling noise, coming apparently from the north, passing under the house, and so away to the south; the door of the room in which he was sitting rattled. A slight shock of earthquake was felt at Naples at eight o'clock on Saturday morning. At about the same hour severer shocks took place at Popoli, Pescara, and Orsogna, in the Abruzzi. The seismographic instruments on Mount Vesuvius show great activity. In the Abruzzi the earthquake shock has damaged several houses at Chieti and Castelfrentano, where some people have been wounded. At Lanciano two people were killed. At Orsogna one was killed and several were wounded. At Atessa the church of St. Giustina was seriously damaged. There is a great panic everywhere amongst the population. A shock of earthquake occurred at Sanpietro Brazza (Dalmatia) on August 29, at 9 p.m. It lasted four seconds. On September 2, at 10.48 a.m., two strong oscillations were felt at Sign, Spalato, and Brazza (in Dalmatia). Direction east-west. Over forty shocks of earthquake have been felt at Khoi, Persia, between the 28th ult. and September 11. Some houses were destroyed, but no lives have been lost. Most of the inhabitants have left the town, and are encamped outside. The direction of the earthquakes was from north to south. The shocks were accompanied by rumbling noises.

A TERRIBLE disaster has occurred at Elm, a village in the Canton of Glarus. The place has been almost destroyed by a fall of rock. It is believed that at least 200 persons are buried beneath the ruins.

THE German Ornithological Society held its annual meeting early this month at Hamburg. Prof. Landois (Münster) spoke on birds' nests and on the origin of egg-shells; Dr. Reichenow (Berlin) on the classification of ducks.

ACCORDING to the last report of the director of the Central Sanitary Bureau of Japan, the Central Government has granted the necessary funds for the establishment of a hospital in the capital for the special treatment of *kakke*, a disease which has been spreading more and more in the country, and one of the usual symptoms of which is œdema of the legs. It is intended to investigate carefully the causes and proper treatment of the disease at this institution. The average mortality, from all causes, is given at 10·43 in every 1000 of the population, and is stated to be lower than that of places in Europe and America under similar conditions. The director observes, however, that the registration of deaths is not efficiently carried out; but measures are being taken to render this as complete and accurate as possible. It is noticeable that deaths from diseases of the digestive organs and nervous disorders greatly preponderate over all others. In the former the proportion is 24·1 per cent., and in the latter 23·1. Small-pox was the most destructive epidemic of the year, but the number of annual vaccinations is largely increasing. During the year covered by the report it was 1,659,298.

THE second part of Dr. Lang's "Butterflies of Europe" is before us, and quite justifies our mostly commendatory remarks in a recent number. When the work more nearly approaches completion we may probably again find occasion to notice it.

FROM Surgeon-Major Bidie's Report on the Government Central Museum at Madras, we see the number of visitors during 1880-81 was less than in the previous years, due, however, to trivial and temporary causes. The total number of visitors was 173,898, of whom 39·36 were women and girls. Many of course go simply for curiosity, but a very considerable number visit the museum for the express purpose of obtaining information, and there seems no doubt that, under Mr. Bidie's energetic and intelligent management, the institution is doing much good. Very considerable additions have been made during the year, and the whole is in a fair way of being catalogued.

THE additions to the Zoological Society's Gardens during the past week include two Greater Black-backed Gulls (*Larus marinus*), British, presented by Mr. A. Allen; a Blue-shouldered Tanager (*Tanagra cyanoptera*) from South America, presented by Mr. Ernest L. Marshall; a —— Tanager (*Tanagra*, sp. inc.) from Brazil, presented by Dr. Arthur Stradling; a Green Lizard (*Lacerta viridis*), South European, presented by the Misses Parry; two Pantherine Toads (*Bufo pantherinus*) from North Africa, presented by Mr. R. E. Holding; six Common Lizards (*Lacerta vivipara*), two Smooth Snakes (*Coronella lævis*), two Sand Lizards (*Lacerta agilis*), British, presented by Mr. J. T. Mann; a Grey Parrot (*Psittacus erithacus*) from West Africa, four Passerine Parrakeets (*Psittacula passerina*), two Lineated Finches (*Spermophila lineata*) from South America, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, deposited; six Common Chamæleons (*Chamæleo vulgaris*) from North Africa, purchased. The additions to the Insectarium include larvæ of the Tussch Silk Moth (*Attacus mylitta*); several larvæ of the Poplar Hawk Moth (*Smerinthus populi*), presented; an imago of the Death's-Head Moth (*Acherontia atropos*), presented by Mr. M. H. Temple, Warwick, and two specimens of *Ceratocampa ixion*, bred from pupæ received a short time since from South America, also many species of aquatic Coleoptera from Askham Bog, near York, presented by Mr. W. A. Forbes, including *Haliplus elevatus*, *Hyphydrus ovatus*, *Hydroporus rufifrons* and *lineatus*, *Colymbetes exoletus* and *grapei*, *Ilybius ater* and *uliginosus*, *Agabus dispar* and *abbreviatus*, *Noterus sparsus*, *Helophorus aquaticus*, *Hydrobius fuscipes*, *Philhydrus melanocephalus*.

OUR ASTRONOMICAL COLUMN

THE DEARBORN OBSERVATORY, CHICAGO.—The annual report from Prof. Hough to the Board of Directors of the Chicago Astronomical Society, dated May last, has been issued. The planet Jupiter has been made a special object of study with the great equatorial, the first observation having been secured on May 6, 1880, and the last on January 30, 1881. The observations made at the Dearborn Observatory do not support the idea that the surface of the planet is "subject to sudden and rapid changes, which may be accomplished in a few days or even a few hours." On the contrary, the observations in question show that all minor changes in the markings or spots have been slow and gradual. "In fact the principal features have been permanent, no material change being detected by micrometer measurement." With regard to the rotation of Jupiter, the discussion of the measures on the great red spot made from September 25, 1879, to January 27, 1881, or over a period of 490 days, gave

for the mean value 9h. 55m. 35·2s., but when the individual observations are compared with it, a well-marked maximum displacement of the centre of the spot, to the amount of 1″·4, is exhibited, apparently indicating that it gradually oscillated to this extent in longitude, which on the surface of Jupiter corresponds to about 3200 miles. The observations however may be well represented by making the period of rotation a function of the time; thus the period 9h. 55m. 33·2s. + 0·18s. $\sqrt{t}$ is found to satisfy all the measures with a mean maximum error of 0″·5: the zero-epoch being September 25, 1879, and t the number of days after that date. The mean-rotation period derived from observations of polar spots is 9h. 55m. 35·1s., that deduced from the small spots indicating an average displacement during two months of 2″, or about 4600 miles. The rotation resulting from the observations of equatorial spots is 9h. 50m. 9·8s. with uniform motion. Prof. Hough states that the actual size of the great red spot, as seen with the Chicago telescope (18½ inches aperture) is—length, 29,600 miles; breadth, 8300 miles; and he remarks that smaller telescopes make the approximate length considerably less than the real value.

The nebula near Merope in the Pleiades, of which so much has been written, was not seen with the Chicago refractor in 1879, but as so many observers have described it, Prof. Hough, in conjunction with Mr. S. W. Burnham, made a thorough examination of the locality, with the result that they satisfied themselves that "the nebula did not exist, but that the appearance described by different astronomers was wholly an optical illusion, due to the glow from *Merope* and neighbouring stars." This opinion will probably be disputed in many quarters.

THE WASHBURN OBSERVATORY, WISCONSIN.—No. 1 of "Contributions from the Washburn Observatory, of the University of Wisconsin," has been received. The establishment is under the direction of Prof. Edward S. Holden, late of the Naval Observatory, Washington. Work was commenced in the latter part of April in the present year, with the Clark refractor of 15·56 inches aperture, and Prof. Holden has had the good fortune to secure the co-operation of that eminent observer, Mr. S. W. Burnham, who left Chicago at the beginning of April to accept a post in Washburn Observatory, and although the publication to which we refer is dated May 31, some five weeks after the commencement of operations, thirty-four new double-stars had been detected and measured by Mr. Burnham, and a number of other doubles, discovered in the course of zone-observations in which Prof. Holden took part, were also measured. In addition we have a list of new nebulæ detected in the zone-observations, several of which appear to deserve special attention. On May 2, in R.A. 18h. 8m. N.P.D., 108° 20′, a void space was remarked in the Milky Way; it is thus described: "This is a black circular hole (10′) in the Milky Way. The stars around it are excessively crowded, and inside there are but two stars, one 10 mag., the other very small."

The number of newly-discovered objects—double stars and nebulæ—of which we have the particulars in this first "Contribution" from the Washburn Observatory, is quite extraordinary, considering the few weeks over which observations have extended. We wish continued success to the Observatory of the University of Wisconsin.

SCHAEBERLE'S COMET.—The following meridian observations S.P. of Comet *c* 1881, made with the transit-circle at the Radcliffe Observatory, Oxford, have been communicated by Mr. E. J. Stone, the Radcliffe observer. The N.P.D. is uncorrected for parallax.

	G.M.T.		R.A.		N.P.D.
	h. m. s.		h. m. s.		° ′ ″
July 31,	9 54 47·5	...	6 28 21·6	...	44 3 46·1
(*a*) Aug. 2,	9 56 28·0	...	6 37 55·6	...	42 57 (36)
4,	9 59 56·5	...	6 49 17·7	...	41 47 13·3
6,	10 6 1·3	...	7 3 16·6	...	40 35 47·9
10,	10 29 50·1	...	7 42 55·6	...	38 21 31·6
(*b*) 19,	12 35 2·4	...	10 23 57·4	...	40 44 50·7

(*a*) Comet very faint. Only an approximate observation.
(*b*) Much brighter. Observation good.

ENCKE'S COMET.—The early observations of this body point to a negative correction of the mean anomaly to the extent of 3′, which corresponds to a retardation in the time of perihelion passage of about 0d·169 The perturbations from the action of Jupiter during the last revolution have been much greater than between 1875 and 1878, in which latter year the necessary correction to the mean anomaly given by the calculations of the late Dr. von Asten, was about one-third as great, but in the same direction. The work of his successor, Dr. O. Backlund of Pulkowa, has been executed with a most thorough determination of the planetary perturbations, which is extended to the preparation of the ephemeris.

The first glimpse of the comet, so far as we know at present, was obtained by Dr. Hartwig and Prof. Winnecke with the six-inch comet-seeker at the Observatory of Strassburg on August 20. Five days later it was clearly seen in the same instrument as a nebulosity 4′ in diameter.

ELONGATIONS OF MIMAS.—The following Greenwich times of apparent preceding elongations of this difficult object depend upon the same elements as previously used in this column:—

	h. m.		h. m.		h. m.
Sept. 19 at	15 36	Sept. 21 at	12 50	Sept. 23 at	10 4
20 at	14 13	22 at	11 27	24 at	8 41

GEOGRAPHICAL NOTES

THE International Polar Conference, which was held last year at Berne, and the previous year at Hamburg, met last month at St. Petersburg. The object of this Conference is the organisation of a series of stations around the Polar area for the continuous prosecution of scientific observations. Since its last meeting it has lost Lieut. Weyprecht, who was the originator of the idea of such a scheme. Delegates were present from all the leading European States except England, and from the United States of America. The first subject discussed was the time at which observations should be taken, and their frequency. Observations will begin for all the expeditions in the Polar regions, as also for observations in the temperate zones, as soon as possible after August 1, 1882, and will finish as close as possible to September 1, 1883. All the meteorological and magnetical phenomena will be observed hourly during all this time; and, besides, there will be taken on the 1st and 15th of each month magnetic observations every five minutes for twenty-four hours, and every twenty seconds during an hour of the day fixed on in advance, and that everywhere after the mean time of Göttingen. These latter observations have for their special end to obtain a perfect knowledge of perturbations or magnetic storms, and their connection with the aurora borealis. On the basis of a programme of observations to be made, already elaborated by the Hamburg Conference, the obligatory meteorological observations were discussed—*i.e.*, observations which all the stations must make in order to insure the scientific success of the enterprise. The result of the discussion was the fixing of the principles, and in part also of the methods and instruments of observation, to insure the accuracy and comparability of the meteorological observations to be made. Happily the Conference numbers among its members several distinguished men of science, who have acquired in former expeditions in the Polar regions very great experience of the difficulties to be met with in taking observations, who were able to give advice useful in obviating beforehand those obstacles, by the arrangement of the instruments, and by the method of taking observations. One day was devoted by the Conference to visiting the celebrated meteorological and magnetic observatory of Pavlovsk, and discussing there the choice of the best apparatus. The members visited in detail the provisional installations which have been made at the observatory for inspecting the magnetic instruments intended for the Russian expedition to the mouth of the Lena. At the third sitting of the Conference, the magnetic observations were discussed: these also meet with difficulties unknown in temperate zones. It is not only the great cold, but also the feebleness of the horizontal intensity of terrestrial magnetism, as also the frequency and greatness of the perturbations, which render observations very difficult and delicate. At the fourth meeting the Conference was occupied with observations on the aurora borealis, and with the question of facultative observations, those which are recommended to the expeditions, without being considered indispensable—as observations on the temperature of the soil, evaporation, terrestrial galvanic currents, atmospheric electricity, &c. The conference, among other things, decided to apply to different institutes to assure their co-operation, and to request magnetic observatories in the temperate zones, especially those in the southern hemisphere, to participate in the simultaneous observations, as also to ask the directors of the telegraphs of different countries to study more accurately terrestrial

galvanic currents in the telegraphic wires when aurora borealis or magnetic perturbations appear. Finally the assembly unanimously approved three proposals by Count Wilczek:—1. To found, if possible, a special publication to convey more quickly to the knowledge of the scientific world, as well as to the leaders of the expeditions, the proposals and reports concerning the expeditions, as also their first results. 2. To leave, if possible, on the spot the buildings and other arrangements likely to be useful to future expeditions of the same kind, and to recommend them in each country to the care of navigators or of the inhabitants. 3. To ask railway and steamboat companies to grant a reduction in the fares for the staff and effects of the various international Polar expeditions. The stations proposed, we may state, are two on the north coast of Siberia, one in Novaya Zemlya, one in Spitzbergen, one on Jan Mayen Island, one on the west coast of Greenland, one at Lady Franklin Bay, one in the Behring's Strait region, and the participating countries are Russia, Sweden, Denmark, Germany, Austria, and the United States.

ON the 3rd of next month the members of the Italian scientific expedition for the exploration of the Arctic Seas will embark at Genoa in one of Lovarello's steamers. The zoology will be under the care of Dr. Vinciguerra; the botany will be confided to Dr. Lorenzo, at present residing at Buenos Ayres; mineralogy and geology to Prof. Lovisato, of the University of Sassari; and to Lieut. Roneagli the artistic department is given, for which purpose he will take photographic apparatus, &c. At Buenos Ayres the Commission will embark on a vessel belonging to the Argentine Republic. Lieut. Bove, who will take the command of the expedition, has already left for Buenos Ayres.

THE U.S. Government have been officially advised of the arrival of Lieut. Greeley's Polar Expedition at Lady Franklin Bay, six days after leaving Upernivik. The expedition entered Discovery Harbour on August 11, where a station was formed. The party were all well and plentifully provided.

ADVICES from Copenhagen state that the news received from the Dutch Polar Expedition on board the schooner *Willem Barents* is very unfavourable. Owing to the continuous ice barrier, which extends nearly to Norway, Spitzbergen could not be reached, nor yet even the Bear Islands; and after one more attempt to force through northward, the expedition will return home, as the captain is convinced that this year Novaya Zemlya is completely inclosed in a barrier of ice.

THE Russian Geographical Society has prepared short notices on the progress of different branches of geographical science from 1875 to 1881, *i.e.* from the second to the third Geographical Congress. Three of them are printed: (1) "Aperçu des Travaux Hydrographiques"; (2) M. Bogdanow: "Aperçu des Recherches Zoo-géographiques en Russie"; (3) P. Matvelew and A. Stichinsky: "Aperçu des Études sur le Droit coutumier en Russie." Besides there are in preparation notices on botanical geography by M. Bataline, on geology by M. Alénitzin, and on Count Uvarow's work on the Stone Age in Russia, by L. Maikof. A. W. Grigorief and Dr. A. Woeikof will be the Russian official delegates to the third Geographical Congress. The absence of the celebrated Russian cartographers is much to be regretted; one of them, General Stubendorff, hoped to attend the Congress, but now it is known he will not be present.

THE new number of the Geographical Society's *Proceedings* is remarkable for the excellent map of Khorasan and the neighbouring countries, in illustration of Col. Stewart's account of his journey and investigations in the Tekke Turkoman country and the region of the Tejend and Murghab Rivers. The map goes beyond Merv and Herat on the east and takes in the south-east part of the Caspian on the west. There is also an article on the recent journey of two Baptist missionaries from Vivi, by the north bank of the Congo, to Stanley Pool. Dr. Matteucci's great geographical achievement in North Central Africa and subsequent death in London are sympathetically referred to in the Geographical Notes. The Society's telegram of condolence to the Geographical Society at Rome appears to have been much appreciated there, as it has been reproduced in the Italian papers. One of the most interesting items in the present number is a letter from Mr. W. H. Dall, of the United States Coast Survey, on "The Chukches and their Neighbours in the North-Eastern Extremity of Siberia." The letter is written in reply to some strictures which Lieut. Nordqvist, of the *Vega*, addressed to the St. Petersburg Geographical Society, and which were noticed in the *Proceedings* for June.

THE Berlin African Society has received further news from several German explorers in Western Africa. Dr. Pogge and Lieut. Wissmann were at Malange at the end of May, hoping to start early in June, and to reach Kimbundo at the end of that month. From Robert Flegel news are to hand up to June 4. The members of the station at Kokoma are occupied with scientific collections and the exploration of the environs. Dr. Stecker is trying to reach the Central African lakes from Abyssinia.

A NEW volume of travels by Mr. E. A. Floyer, F.R.G.S., &c., entitled "Unexplored Balūchistan, a Survey of a Route through Western Balūchistan, Mekran, Bashakird, Persia, Kurdistan, and Turkey," will be published during the autumn by Messrs. Griffith and Farran. Mr. Floyer was the first to explore the wild district of Bashakird; he contributed a paper on that little-known country to the Plymouth meeting of the British Association. Besides the narrative, which is full of interesting personal incident and adventure, the work will contain original illustrations, a map, vocabularies of dialects, lists of plants collected and tabulated, and observations, astronomical and meteorological.

PROF. SIMONY has published a list of the greatest depths of various Alpine lakes, which may interest our readers: Gmunden Lake, 191, Hallstadt Lake 125, Attersee 171, Mondsee 67, Wolfgang Lake 114, Achensee 132, Königssee 188, Lake of Constance 276, Chiemsee 89, Starnberg Lake 131, Lake Leman 309, Neufchâtel Lake 144 metres. The last-named four measures 92, 57, 589, and 240 square kilometres surface. The greatest depth of the northern part of the Adriatic is only 243 metres.

SCIENTIFIC SERIALS

The Journal of the Royal Microscopical Society, August, 1881, contains:—On some remarkable enlargements of the axial canals of sponge spicules and their causes, by Prof. P. Martin Duncan (plates 7 and 8).—On a blue and scarlet double stain, suitable for nerve and other animal tissues, by Dr. B. Wells Richardson. With the summary of recent researches, zoology, and botany, pp. 575 to 651; Microscopy, pp. 651-711.—Proceedings of the Society for June.

The American Naturalist for August, 1881, contains: The great crested fly-catcher, by Mrs. Mary Treat.—On the reasoning faculty of animals, by Joseph F. James.—On the progress of anthropology in America during 1880, by O. T. Mason.—On the manuscript Troana, by Cyrus Thomas.—The Editor's Table.—Some recent literature.—General notes and scientific news.

Proceedings of the Academy of Natural Sciences of Philadelphia, Part 1, January to May, 1881, contains: Dr. Jos. Leidy, Rhizopods as food for young fishes.—Thomas Meehan, note on treeless prairies; motility in plants; sexual characters in *Fritillaria atropupurea*, Nutt.—R. Arango, descriptions of new species of terrestrial mollusca of Cuba.—Rev. H. C. McCook, on the honey-ants of the Garden of the Gods. (This detailed memoir on the structure and habits of *Myrmecocystes melliger* is illustrated with ten plates.)—John A. Ryder, on the structure, affinities, and species of Scolopendrella. *S. gratiæ* is figured and described. An American specimen of what is presumed to be *S. notacantha* is also figured. The author places these strange insects in an order Symphyla, indicating that it has affinities to Thysanura; trachea are present. Henry Hemphell, on the variations of *Acmæa pelta*.—R. E. C. Stearns, observations on Planorbis (with many woodcuts).

American Journal of Science, August.—Method of obtaining and measuring very high vacua with a modified form of Sprengel pump, by O. N. Rood.—Geological relations of the limestone belts of Westchester county, New York; origin of the rocks of the Cortlandt series, by J. D. Dana.—New meteoric iron of unknown locality, in the Smithsonian Museum, by C. U. Shepard.—The relative motion of the earth and the luminiferous ether, by A. A. Michelson.—Observations on the light of telescopes used as night-glasses, by E. S. Holden.—Nature of dictyophyton, by C. P. Whitfield.—Observations on the comet, by H. Draper, C. A. Young, W. Harkness, L. Boss, and A. W. Wright.

Journal of the Franklin Institute, August.—Boiler explosion in Philadelphia in June, by W. B. Le Van.—Auchincloss's averaging machine.—Red o-dynamics II., by P. E. Chase.—The properties of air relating to ventilation and heating, by R. Briggs.

Annalen der Physik und Chemie, No. 8.—Experimental investigation of the tones which arise in passage of gases through slits, by W. Kohlrausch.—On the observation of air-vibrations in organ-pipes, by R. König.—On the conductivity of metals for heat and electricity (continued), by L. Lorenz.—On the application of photometry to the study of the phenomena of diffusion in liquids, by S. v. Wroblewski.—Experimental contribution to the theory of influence-machines, by W. Holtz.—On the development of polar electricity in hemimorphous crystals by variation of pressure in the direction of the unsymmetrically formed axes, by W. Hankel.—On the decomposition of water on platinum electrodes by discharge of Leyden jars, by F. Streintz.—On the resistance of polarised cells, by E. Cohn.—On the phenomena in Geissler tubes under external action, by E. Reitlinger and H. v. Urbanitzky.—Note on the maximum of temporary magnetism in soft iron, by C. Fromme.

La Nature, August.—The air-barometer, by Prof. Ferrini. The electro-photometer of Dr. Nachs.—On the electric phenomena of Canton's jar, by Prof. Righi.—On the origin of electricity in storm-clouds and atmospheric air, and on electricity in general, by Dr. Nachs.—On the direction of sounds and the object of double hearing, by Prof. Pinto.

Journal de Physique, August.—Researches on the capacity of polarisation (continued), by R. Blondlot.—Discharge of a condenser, and energy of telephonic currents, by H. Pellat.—On a new interrupter for induction-coils, by M. Deprez.—Note on the registering instruments of MM. Richard frères.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 6.—Note on a new dolphin of New Zealand, by M. van Beneden.—A word on some new infusoria parasitic on Cephalopoda, by M. Fœttinger.—Study on the hypophysis of Ascidians and the neighbouring organs (second paper), by M. Julin.—Note on the fossiliferous Porphyroids met with in Brabant, by M. Poussin.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xiv. fasc. xii., xiii.—Researches on the phenomena of sense, motion, circulation, and respiration in hypnotism, and on their modification by æsthesiogenic agents, by Prof. Tamburini and Dr. Sepilli.—On some products of transformation of chinoline, by Prof. Körner.—Theorem on linear systems in projective measurements, by Prof. D'Ovidio.—Consequences of pachymeningitis and hematoma of cerebral membranes, by Prof. Sangalli.—Fasc. xiv.—On the small volcano of Querzola in the province of Reggio, by S. Taramelli.—On the resistance to passage of the voltaic current in an iron wire at different temperatures, by Dr. Poloni.

Rivista Scientifico-Industriale, July 1 and 15.—Determination of vapour density, by Dr. Valente.—Palæontological peregrinations in the Pliocene of Mount Falcone Apennino, in the province of Fermo (Marche), by Prof. Spada.—On determination of the electromotive force of the Voltaic couple by Fuchs' method, by Dr. Guglielmo.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 5.—M. Wurtz in the chair.—The following papers were read:—The direct-vision spectroscope applied to physical astronomy, by M. Zenger. One may (as before shown) compound refringent media whose index for the red ray A is less than that of crown glass or quartz, while the index for the violet ray H is much greater. The spectrum so produced is fan-shaped, and, with a single dispersion parallelepiped (two similar prisms with their refringent angles opposite), may be made of considerable length (25° and more). With one arrangement all the rays, except blue or red, may be eliminated, and the sun, *e.g.*, viewed in monochromatic light. M. Zenger specifies various combinations of quartz or crown glass with anethol, benzene, alcohol, &c. He obtains effects equal to those of the most powerful spectroscopes hitherto made.—Influence of nutrition on poisoning with strychnine, by M. Delaunay. Strychnine affects more quickly and intensely strong frogs than weak ones; frogs well fed than those which have been fasting; frogs that have been in vigorous exercise than those at rest; frogs that are exercised immediately after injection than those which are not; a frog hung by the leg than one hung by the head; an intact frog than one which has been bled; the right side of frog than the left, &c.—Observations of Cruls' comet (*b* 1881) at Marseilles Observatory, with an equatorial of 0·26 m. aperture, by MM. Borelly and Coggia.—Observations of Schäberle's comet (*c* 1881) in the same way and place, by M. Coggia.—Observations of Encke's comet, by M. Tempel. He observed it on the 21st ult. A letter from M. Loewy stated that M. Struve found it on the 24th (MM. Winnecke and Hartwig at Strasburg about the same time). The comet (according to M. Tempel) was large, but very diffuse, without nucleus or condensation towards the centre, and so, very difficult to observe.—On the light of comets, by M. Respighi. He considers we are not yet in a position to say that comets have a light of their own, due to incandescence of cometary matter. The discontinuity of the spectrum, and the bright lines and bands, may arise from reflected light as affected in traversing the gases and vapours of the comet; the same cause as affects the spectrum of the sun when near the horizon. Only the phenomenon is exaggerated in comets by reason of the enormous thickness of the absorbent layers, their richness of chemical composition, and the weakness of the light they reflect to us.—On observations of meteors from July 25 to 30, 1881, by M. Cruls (Rio). More than 90 per cent. of the meteors seemed to radiate from near Fomalhaut. The horary average increased rapidly between the evening and morning hours, and there was a remarkable recrudescence shortly before sunrise. It would thus seem that the stream of meteors moves in opposite direction to the earth. This is corroborated by the fact that the morning meteors, especially after 5 a.m., all moved with great velocity, and were very brilliant. They were all sensibly displaced in the plane of the ecliptic; their direction is probably very little inclined to this plane.—On ferruginous carbonated waters, by M. Ville. Neutral alkaline carbonates precipitate such water immediately; neutral alkaline earthy carbonates also have this effect, but more slowly. Alkaline and alkaline-earthy bicarbonates do not alter ferruginous water. Chlorides and sulphates sensibly retard the decomposition of ferruginous water in air. The disturbing influence of neutral alkaline carbonates may explain the relation between the richness of ferruginous carbonated waters and the presence of these saline compounds. The action of neutral carbonate of calcium explains the existence of considerable beds of limonite in calcareous strata.—On absorption by the vesical mucus, by MM. Cazeneuve and Lépine. The sound bladder absorbs the normal elements of urine. Certain toxical or medicamentary substances (*e.g.*, sulphate of strychnine) are not absorbed.—On experimental tuberculosis, by M. Brunet.

CONTENTS

THURSDAY, SEPTEMBER 22, 1881

EGYPTIAN EXCAVATIONS AND MUMMIES

THE recent excavations in Egypt have been productive of great results to archæology and the history of Egypt. One site, which has yielded unexpected additions to the early period of the country, has been excavated on scientific principles under the direction of M. Maspero, the present superintendent or director of the Archæological Department. It is his intention to open the whole group of unexplored pyramids, in order to find the sequence of monarchs of whom they were the sepulchres, and to discover any inscriptions with which they may have been decorated. An examination of the whole group of pyramids indeed was formerly made by J. Shay Perring, C.E., at the expense of Col. Howard Vyse, who spent a fortune in pyramidal researches; but the excavations of Perring were chiefly devoted to the examination of three great pyramids of Gizeh, those of Cheops, Chephren, and Mycerinus, and although he examined the whole group in the scientific manner of an engineer, by some fatality he appears not to have penetrated into the interior of the smaller ones, which are now in process of examination by M. Maspero. The conviction which that *savant* has arrived at is that these pyramids are arranged in symmetrical groups, each group holding the remains of the monarchs who followed each other in chronological succession. The group just discovered consists of three pyramids at Sakkara, of small dimensions, lying to the N. and E. of the step-shaped pyramid, and on the road to the Serapeum, the sepulchres of three monarchs of the sixth dynasty, Ra-meri or Mira, whose name was Pepi or Phiops, a king who is said to have reigned 100 years all but an hour; his successor, Merienra, named Har-em-saf, or Ta-em-saf, and a king called Una. They seem all to have been constructed on the same principle, having inclined entrances leading to sepulchral chambers with pointed roofs, the walls of the passages and chambers covered with hieroglyphs coloured green, the ceilings of the sepulchral chambers with pointed roofs on which were stars in white upon a black ground, indicative of the hours of the night. The inscriptions of these chambers are of interest purely mythological, no historical fact or allusion being mentioned in them, but their contents consisting of prayers similar to those in the Book of the Dead, or Ritual, and chiefly referring to the myth of Osiris and Hades, especially the identification of the kings with Osiris as the son of Nut and Seb, and his following the course of the constellation Orion, rising and setting with that constellation, allied with the star *Sebt*, or Sothis, and the progress of the king to the *Aahlu* or Egyptian Elysium, and in the account of the Island of the Fields of Ho-tep, or Peace, recalling to mind Eden, mention is made of a tree of life. In the Pyramid of Pepi, the Phiops of the sixth dynasty, who is said by the history of Manetho to have reigned 100 years all but an hour, and who must consequently have ascended the throne quite a boy, was found the remains of a sarcophagus of black and white granite of unfinished work, which had been broken, and another in the south-east corner of the chamber of the same material, which had been let into the masonry. In the vicinity of this sarcophagus on the west side between this and the wall was found amidst a heap of rubbish remains of dresses and mummy bandages varying from yellow to dark brown of extreme fineness; of the mummy itself an embalmed hand in good condition was only found, and even this may be considered remarkable, as the bodies of the earlier period were only dried, and not embalmed, and generally fall to pieces when exposed to air. The pyramid was indeed small, considering the long reign of Pepi. The Pyramid of Merienra, or Har-em-saf, which resembled in general character that of Pepi or Phiops, had two sarcophagi of red granite close to one another, the cover of one removed and hidden under blocks of stone. The other held a body mummied, which was that of the king; it had been anciently plundered of its ornaments, but embalmed with the greatest care, the skin well preserved, the traits of the countenance distinct, the eyes closed, the end of the nose fallen in, the stature of medium height, and the limbs youthful. This king was the successor of Phiops. The third pyramid of the group was of Noferkara or Nephercheres, but no details of the inscriptions have as yet been published, although they probably refer to the Osiris myth, like the others. The details of the size of coffins and mummies of this pyramid are still wanting. Each pyramid had a special name: that of Pepi was called Mennefer, that of Ha-rem-saf was Shanefer, that of Noferkara also Mennefer. Compared with the great Pyramids of Gizeh, they are far inferior, but the inscriptions in them offer an interest greater than that of the plain Gizeh Pyramids. The only question is whether the mummies found in them are contemporaneous with the sixth dynasty, which appears most probable, or subsequent usurpations, of which there is no monumental or inscribed evidence.

The next remarkable discovery is that of the thirty-nine mummies, several of kings, in a subterraneous well or pit not very far from the edifice of the Deir-el-Bahari. This remarkable structure, consisting of a temple on a platform with chambers let into the solid rock, had been published by Marriette Pasha, and had been suspected by Brugsch Bey to be the site of the sepulchres of the early monarchs of the eighteenth dynasty. The temple itself had been commenced by the queen Hatasu or Hasheps, daughter of Thothmes I. and wife of Thothmes II., and its sculptures commemorated the expedition made by that queen to Punt or Somali, the treasures brought from thence in gold, silver, frankincense, besides trees of that material, besides giraffes, cynocephali, large dogs. Besides which they give representations of the inhabitants and of the Egyptian fleet which descended the Red Sea on the voyage of amity or discovery promoted by the Egyptian queen.

In the well or pit of the Deir-el-Bahari, which was formed of bricks of conical shape stamped with inscriptions, on which could be traced the titles of the high priest of Amen-ra thus used by the monarchs of the twenty-first dynasty, were found the coffins, mummies, and other objects which appear to have been there deposited in the reign of Herhor, first monarch of the twenty-first dynasty, and of another king, Panetem or Pinotem, of the same dynasty. The cause of the removal of the mummies deposited in the Theban sepulchres, such as the El Assasif and the Biban-el-Molook, is stated on some of the wraps of the mummies to have been the apprehension

of a foreign invasion, and that possibly of the Assyrians, whose arms had made great progress in Central Asia. According to Brugsch Bey the twenty-second dynasty was Assyrian, and he identifies the name of the monarch with that race; but at all events they were never Assyrian monarchs, such names as Shashanq or Shishak, Namrutha or Nimrod, not having been found in the Assyrian annals, although Uasarkan or Sargon, and Takelloth or Diglath may correspond with Assyrian kings.

From the El Assasif had been removed the mummy of Taakan, also known as Skanenra, which was formerly deposited at the Drah Abu-el-Neggah with its three inscribed coffins, and which was intact at the time of Rameses IX. about B.C. 1150. It was in his reign that the quarrel of the Egyptian kings with the Shepherd Kings commenced, and he is mentioned in the celebrated Sallier Papyrus. The mummy of Aahmes or Amosis I. in three plain cases was also found amongst the coffins, but it is not known where this king was buried; as he succeeded Skanenra, his tomb was probably somewhere in the vicinity. The mummy of Aahmes Nefertari was also found, it is said, in three cartonages with paintings on a white ground. Another queen, Aahhotep, daughter of the King Aahmes, was also found, and it will be recollected that this was the name of the queen whose mummy and coffins, and gold and silver jewellery, and arms were discovered by fellaheen at the Biban-el-Molook, a few feet below the surface. She was wife of Kames and mother of Aahmes, while the queen of the Deir-el-Bahari was the wife of Amenophis I. The mummy of Amenhotep I. or Amenophis was found in a wonderful state of preservation, painted and varnished, and with wreaths of flowers so exquisitely preserved that they retain all their colour like recent flowers kept and pressed between the leaves of books. These flowers, it will be remembered, are above 3000 years old, and their preservation is probably due to their having been buried in hot sand, a mode still in use in Palestine, by which means botanical specimens retain their colour for a long time unchanged, a process perhaps known to the ancient Egyptians, although wreaths and flowers, even of the Roman times, from Egypt are brown and semicarbonised. The tomb of Amenophis I. is mentioned as at the Drah Abu-el-Neggah in the Abbot Papyrus, and the body transported thence of Thothmes I., his son; the mummy case, considerably mutilated, was only found, and this had been appropriated by Pinotem. The mummy of Thothmes II., in three mummy cases, was likewise discovered. That of Thothmes III., the great and warlike monarch of the eighteenth dynasty, was found in a single coffin much mutilated, his body broken into three pieces and rifled in ancient times, but with an inscribed ritualistic linen roll said to prove the identity of the mummy. Of the other personages of the eighteenth dynasty were the mummies and coffins of the Prince Saamen, the Princess Satamen, a princess and king's sister, but unmarried, named Hanta-em-hu; and a similar royal sister and queen named Me-han-ta-emhu, child of Hanta-ena-hu, had been removed at the time of the twenty-first dynasty; another unmarried queen-sister named Miramen, and Nebseni, a priest or flamen of a Pharaoh. All these coffins of the eighteenth dynasty have a certain similarity with each other. Those of the nineteenth are Rameses I., whose tomb and sarcophagus are at the Biban-el-Molook. There is some uncertainty in the different accounts which have come to hand whether there are three coffins or one, and if the mummy was deposited at the Deir-el-Bahari. The mummy of Seti I., whose tomb is in the Biban-el-Molook and alabaster sarcophagus in the Soane Museum of London, is well preserved in one wooden coffin; the mummy of one of the Ramessids, apparently the twelfth, not the second, as reported, in a plain coffin, the features not aquiline, but the shroud, covered with lotus flowers, looking remarkably fresh; this also came from the Biban-el-Molook. These mummies, it is stated, were removed under apprehension of a foreign invasion. Then follow the cases and mummies of the twenty-first dynasty. The queen, Notem, mother or wife of Herhor, of whom there is a papyrus in the British Museum, exhibited by H.R.H. the Prince of Wales, in a badly-preserved but inlaid coffin; Panotem or Pinotem, high-priest of Amen, in three coffins of the style called *richi* by Marriette, and gilded faces; he was, besides high-priest, a *saten sa Kush*, Prince of Cush or Æthiopia, according to the inscription in Lepsius' *Königsbuch*; the queen, Ramaka or Makarra, who assumed the same prenominal title as Hatasu of the eighteenth dynasty, who is in three coffins with the youthful queen, called the "lady of the two countries," or absolute queen-heiress, embalmed in a sitting posture, either having died in a fit or at her birth, and named Mutemhat; the king, Pinotem II., hastily deposited in the coffins of Thothnes I., the mummy has been partially unwrapped and the features exposed, which have a singular resemblance to those of Voltaire, with a sarcastic or satiric smile or grin, a peculiarity also found on a hieratic papyrus ritual in the British Museum, probably of the same period; the queen-mother, Hantau, whose ritual had found its way to the Boolak Museum prior to the discovery; in three cases, the prince Masaharuta, son of Pinotem II. in the same; the queen Asemkheb or Hesemkheb, in as many cases, who appears to have been the wife of Menkheperra; another princess called Nasikhonsu; Tet-ptahaufankha in an appropriate coffin, and four other priests and functionaries. Several other objects were found in the pit: a leather tent embroidered with names, boxes with royal names, boards with inscriptions, and five rituals of the monarchs of the later dynasty; but the whole of the details—amulets, inscriptions, and style of art—cannot be known until the mummies are unrolled and all peculiarities carefully examined, for this remarkable find will afford invaluable data for Egyptian archæology, especially the sepulchral division.

TWO SPIDER BOOKS

The Spiders of Dorset, with an Appendix containing Descriptions of those British Species not yet found in Dorsetshire. By the Rev. Octavius Pickard-Cambridge, M.A., C.M.Z.S., &c. From the *Proceedings* of the Dorset Natural History and Antiquarian Field Club, edited by Prof. James Buckman. (Sherborne: L. H. Ruegg, pp. 1-625, with 6 plates, 1879-1881, 8vo.)

Studi sui Ragni malesi e papuani. Per T. Thorell. III. Ragni dell' Austro-Malesia e del Capo York, conservati nel Museo Civico di Storia Naturali di Genoa. Pp. 1-720. 8vo. (Genoa, 1881.)

IF we take down part 2 of vol. i. of the twelfth edition of Linné's "Systema Naturæ" (1767) and refer to that marvellously incongruous order *Aptera*, in which the

old naturalist contrived to group together nearly all the Arthropods known to him and which agreed almost solely in the one point of the non-possession of wings, we find under the genus "*Aranea*" only 47 species indicated, and of these only 9 are from outside Europe. In the second edition of the "Fauna Suecica" (1761) we find 33 species indicated for Scandinavia. Thus six years later all the spiders known to Linné from outside his native country amounted to 14 species! At the present time 518 species are recorded as British, and a still almost unexplored region of the Eastern Archipelago has contributed nearly as many from the researches of one or two naturalist-travellers, with whom spider-collecting was certainly not considered of first importance. And yet, notwithstanding the vast and rapid strides that arachnology has made within the twenty years past, the number of workers is still small. The subject is not always an attractive one to naturalists, and is often repugnant to non-naturalists, with whom a passion for collecting or studying spiders is seldom associated with respect for the naturalist thus smitten. But all this is rapidly changing, and no two men have done more to bring this about than the authors of the books noticed below.

In vol. xxi. p. 273, we noticed vol. i. of Mr. Pickard-Cambridge's work; vol. ii., completing it, is now before us. The whole is dedicated to John Blackwall, and the second volume must have appeared about the time of the decease of that venerable naturalist. A postscript notices some species new for the county or for Britain, and there are additional remarks on senses and economy, in which "sight," "touch and hearing," "power to utter sounds," "venom," "modes of forming snares," &c., are severally alluded to. With regard to "venom," the author expresses his firm belief that the bite of the common garden geometric spider (*Epeira diademata*) is attended by the emission of a poisonous fluid, sufficiently strong to cause visible effects on the skin of his young son, but without effect upon his own. He now agrees with the conclusion that currents of air play a great part in enabling spiders to carry their lines across from one object to another, although previously he was of opinion that the lines were carried across by the spiders themselves. As we remarked when noticing vol. i., it is a pity the author did not intercalate the descriptions of those British species not yet found in Dorsetshire amongst the others, instead of placing them in appendices at the end. This would have vastly increased the usefulness of what is still a most useful work, and while not destroying its local intentions (as indicated by the title) would have rendered it more distinctly a *Manual of British Spiders*, for such it really is. With it and Blackwall's magnificently illustrated Ray Society monograph before him, no student of our spider-fauna should be at a loss to determine, with approximate certainty, any species he may come across. The six plain plates are excellent, engraved from the author's own drawings, and representing many of the principal genera, with copious details. The index is full. The author recognises 518 species of spiders as inhabiting the British Isles, of which 373 have been found in Dorsetshire. The distribution of these amongst the several families is strikingly unequal. Thus we find three families represented by only one species each; another by only three species. On the other hand the *Therideides* claim 267 species, the *Drassides* 56, the *Epeirides* 32, and so on. Possibly this is the first time that any thoroughly local society has undertaken to bring out a manual of a large group of British animals; so much the more to the credit of the Dorset Society for initiating so laudable a scheme. Their undertaking, so well concluded, is not of local (or even British) interest only, but will have to be considered by every European student of *Arachnida*.

In Dr. Thorell's bulky memoir (which forms vol. xxii. of the *Annali del Museo Civico di Genoa*) the author continues his studies on the Spiders of the Eastern Archipelago. The descriptions are worked out with his well-known detail and accuracy. Most of the materials result from the exploring voyages of D'Albertis and Beccari, and the flourishing society under whose auspices the volume is published deserves the highest credit for the promptness with which it is making known to the scientific world the riches acquired during the voyages of these renowned travellers. The descriptive portion is preceded by a bibliographical sketch of what was previously known from the regions, with an analytical and comparative examination of the arachnid fauna generally, still further subdivided in a series of tables at the end; 317 species are noticed as in the collection (of which 173 appeared to be new to science), viz. 252 from Austro-Malesia and 82 from Cape York, but 505 are recorded for the whole of that part of the globe, divided as follows:—*Orbitelariæ*, 162 species; *Retitelariæ*, 38; *Tubitelariæ*, 31; *Territelariæ*, 10; *Laterigradæ*, 84; *Citigradæ*, 29; and *Saltigradæ*, 151. Some idea of the riches of the fauna in this particular respect may be gathered from the fact that no less than eighteen species of the extraordinary genus *Gasteracantha* are described. We cannot resist a few words of admiration at the manner in which the publications of this Italian society are got up, the more so as the printing is done at the Deaf and Dumb Institute of Genoa (Istituto Sordo-Muti). Paper, typography, and editing alike leave nothing to be desired.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Oldest-known Insects

I MUST ask your permission to correct the errors into which your correspondent, Dr. Hagen, has fallen respecting the Erian (Devonian) beds near St. John, New Brunswick, holding certain fossil insects described by Dr. Scudder.

The Dadoxylon sandstone and Cordaite shale of the vicinity of St. John have been studied not only by myself, but by so good geologists as Prof. Hartt, Prof. Bailey, and Mr. Matthew, and by the officers of the Geological Survey of Canada; and their stratigraphical relations have been illustrated by maps and sections, not only in my "Acadian Geology," but in the *Reports* of the Geological Survey, more especially those for 1871 and 1875. They have, besides, been thoroughly exposed and ransacked for fossils by expensive quarrying operations undertaken by the Natural History Society of St. John, and their plants have been described and compared in detail with those of the neighbouring Carboniferous formations in my Report "On the Devo-

nian Plants of Canada," and subsequent Reports on the plants of the Lower Carboniferous and Millstone Grit formations (Geol. Survey of Canada, 1871 and 1873). In these circumstances it seems strange that the received conclusions as to their age should be termed "simple negation not supported by facts," and regarded as of no scientific value in comparison with the mere assertion of a gentleman who has no knowledge whatever of the stratigraphy of the region, and with the "authority" of Dr. Heer, who is no doubt an excellent authority on certain departments of European palæobotany, but who has not seen the beds in question, nor, so far as I am aware, studied their fossils.

The beds referred to, like the Devonian generally in Eastern Canada, underlie unconformably the lowest Carboniferous beds, a circumstance due apparently to the extensive igneous action which closed the Devonian period in this region, giving origin to masses and dykes of intrusive granite, and disturbing and partially altering the strata of Devonian and greater age, the materials of which have contributed to the Lower Carboniferous conglomerates. There is thus no question here as to any transition between Devonian and Carboniferous, and the beds holding the plants and insects are stratigraphically pre-Carboniferous.

The Lower Carboniferous beds, succeeding to the Devonian formation, and developed to the eastward of St. John, hold the characteristic flora of the Horton series, or Lowest Carboniferous, equivalent to the Calciferous or Tweedian formation of Scotland. In succession to this we have the flora of the Millstone grit, of the true Coal-Measures, and of the Permo-Carboniferous or Lower Permian. All of these have been explored and their plants catalogued and described in my own memoirs or in the reports of the Geological Survey, and it has been fully established that the flora of the Devonian beds is characteristic and distinct from any of these sub-floras of the Carboniferous.

The plants of the Cordaite shales are not only distinguishable from those of the Carboniferous found in their vicinity, but the assemblage includes forms like *Psilophyton* and *Archæopteris*, which are characteristic of the Devonian, and are not found in the Carboniferous elsewhere in America. In the Devonian of Northern New Brunswick some of these plants are associated with fishes of the genera *Cephalaspis*, *Pterichthys*, &c., well-known Devonian types.

For additional information as to the geological relations of the St. John plant beds and notices of new species, I may refer to my paper on "New Erian Plants," in the *Journal* of the Geological Society of London, vol. xxxvii., May, 1881. This paper Dr. Hagen had probably not seen at the time when his letter was written.

The particular fern in question, *Pecapteris serrulata* of Hartt, has been fully described, first by Prof. Hartt, and subsequently by myself, and its distinctness from *P. plumosa* pointed out.[1] The criticism of Dr. Hagen, as to its not appearing in the sectional lists, and still being called by me a common fern, is based on a mere accident, which I could easily have explained to him. The plants referred to as found in each layer in the detailed section are those originally described by me from these beds. Some species, subsequently recognised and described by Hartt, were not included in the sectional lists, and were referred to only in a note, because I had received no information from Prof. Hartt as to the particular layers in which they were found, though I knew that some of them were by no means uncommon, from the number of specimen obtained. Dr. Hagen criticises my figure of the species, but that does not affect the question, as I have compared the specimen on the slab with *Platephemera* with the original specimens in my collection. My figures, however, show fairly the general form of the frond; and there is also a magnified figure of a pinnule, showing the venation, which should enable any one to recognise the species, and with the aid of the description to distinguish it from *P. plumosa*.

With regard to the "Ursa stage" of my respected friend Dr. Heer, founded on a little known and apparently exceptional locality, I have always objected to its being made a standard of comparison for the thoroughly worked and widely distributed Devonian or Erian rocks of North America. I gave some reasons for this in a paper sent to the Geological Society of London shortly after the appearance of Dr. Heer's memoir, an abstract of which appears in the *Proceedings* of the Society. It will be sufficient to say here that the grounds on which Dr. Heer refers the Devonian of New Brunswick to the Ursa stage would apply to the Chenning, and even to the Hamilton formations of the New York series.

[1] "Acadian Geology; Report on Devonian Plants." *Canadian Naturalist*, 1881.

The great richness of the Devonian of North America in fossil plants is a very remarkable geological fact, which I regret to say has hitherto far exceeded the means available for its adequate illustration. I hope, however, to remedy this to some extent on the occasion of the meeting of the American Association in Motreal in 1882, when my whole collection of Erian plants, together with those illustrating the several stages of the Carboniferous, will be exhibited in the new Peter Redpath Museum, and will show more fully than has been hitherto possible the progress of the American flora from the Silurian to the Permian. It will be a great pleasure to me if any palæobotanists who are sceptical as to the magnitude of the Devonian flora will avail themselves of this opportunity to judge for themselves and to form their own opinions as to the affinities and relations of the species.

McGill College, Montreal, September 2 J. W. DAWSON

Sound-producing Ants

I AM glad to see my statement in NATURE, vol. xxii. p. 583, verified by Mr. H. O. Forbes, from Sumatra, and now inclose a few ants of another kind that make this peculiar tapping or scratching sound, though not in the same system of taps as those before noted, and that was thus—

. . . ——— . . . ——— . . . ——— . . . ———

i.e. three taps in unison and a pause of about a second, the taps of equal duration. The ants inclosed make a series — — — — — — ... that dies off and does not seem repeated unless the exciting cause again acts, when they again start in beautiful unison. How they so correctly start together in all cases of . . . ——— or the — — — — — — — ... and keep each tap in time, is really wonderful. White ants make similar tappings, but not in rhythm as far as I know, and they use it to call or warn. These little black ants cannot be heard by us unless the material they are on is sensitively sonorous. Happening to place a tumbler on a sideboard lately in the dark, I was startled to hear this — — — — — . . . . arise from a sugar-bowl. I repeated it, and the noise at once convinced me that it was ants. On getting a light I found a sheet of writing paper had been laid on the bowl, and was covered by them; the glass alone was useless, I found. S. E. PEAL

Asam, July 6

[The ants sent are apparently "workers" of a species not larger than a small British *Myrmica*.—ED.]

Wasps

A COUPLE of weeks ago I found on my window-pane a large black wasp holding in its mandibles a plump spider of about an eighth of an inch in diameter. I placed the wasp under a bell-glass and set it on my desk, where I could readily watch for further developments. Finding itself in captivity, the wasp dropped its booty and spent some time in trying to find a way of escape. Coming at length to a state of rest, it espied the spider and sprang upon it with tiger-like fierceness. Seizing it and raising itself up to its full height, the wasp brought its posterior under and forward with a quick motion, and gave the spider, two or three thrusts with its sting. Assured that the spider was dead, the wasp proceeded to roll it over and over, rapidly working it up into a globular mass. This done it started to fly away; but, foiled in the attempt, it dropped the spider, which was for some time apparently forgotten. This whole operation I saw several times repeated during the two days of my observations. Being called away from home for a few days, I was curious on my return to ascertain the results of my experiment. I had taken the precaution at the first to place under the bell-glass a small dish of clean water, to which the wasp had helped itself freely. I found the wasp dead; but not the least morsel of the spider had it eaten. My conclusions are: (1) that the wasp died of starvation; (2) that the spider was intended, not for its own food, but for that of its young in their larval state. In confirmation of this I have broken open several of the finished cells of these wasps, and found them filled with pellets made of portions of spiders, flies, and worms. Only yesterday a fine opportunity was afforded me for further observations in this direction. One of my flowering vines is infested with a green worm—the larva of the yellow butterfly. I discovered a bronze-and-yellow wasp standing on the edge of a leaf of this vine, holding fast to one of these worms of twice its own size. The worm was dangling in mid-air, and the wasp endeavoured laboriously for a long time to pull it up on the surface of the leaf.

Failing in this, with a dexterity worthy of the Knight of the Shears it cut the worm in two, letting about three-fourths of it fall to the ground. The remainder was then easily dragged to the surface of the leaf, where the wasp spent some fifteen minutes in cutting down, trimming, and reducing it to a globular mass of about an eighth of an inch in diameter. Then resting for a few minutes, and taking a fresh hold of its booty, it flew briskly away. J. T. BROWNELL

Lyons, N.Y., August 13

Treatment of Hay Fever

SOME years ago Prof. Helmholtz, in a letter to you, gave an account of a remedy he had found for "hay fever." This was simply to treat the part of the nose, which seems to be the seat of the trouble, with sulphate of quinine solution by pouring it into the nose with a pipette, while lying on a sofa with the head turned upside down. Having had the most enjoyable part of summer destroyed by hay fever ever since I can remember, I have tried every remedy I have heard of, including internal doses of arsenic, and I have found them all to fail. Prof. Helmholtz's method only gives me relief for ten minutes or so, and cold water does the same. I have tried solutions of sulphate of zinc and tannin, and many other astringents, but all to no purpose. As many others knew that I was experimenting upon myself in this matter, I have had several patients trying all the remedies that I have tried, and I can therefore say with certainty that no remedy yet published will cure hay fever. I have however succeeded in finding a method which is a really effectual cure, and as I know that many are rendered miserable during the most enjoyable part of the year, I hasten to give them the benefit of the result of my inquiries. One thing which misled me was that my eyes were often very much inflamed and pained during an attack, and I often tried remedies for my eyes (which have sometimes gone wrong when I had no hay fever) when they were only affected in sympathy with my nose. I found that the only thing required was to prevent the entrance of the pollen grains into the nose. When there are not many in the air, as during or after rain, it is simply necessary to stop the nose with a spring clip. I have used a piece of brass or steel ribbon bent double, and having only sufficient spring to close the nostrils without undue pressure. This causes the patient to breathe by the mouth, but one soon gets accustomed to the inconvenience. I found that to stop the nostrils with cotton wool was far too irritating, especially as those afflicted with hay fever are so owing to the tenderness of the internal coating of the nose. When going amongst hay a further precaution must be taken, viz., plugging the ducts from the eyes. I used for this purpose dumb-bell shaped pieces of glass, which are easily slipped into the ducts, and can be removed when wanted. Thus protected, any one who is troubled by hay fever can go into the camp of the enemy and stir up hay in a field with as much impunity as one not troubled with this "sixth sense." The season for hay fever is nearly passed now, but I hope that the publication of this note will be the cause of relief to many during next summer, and on that plea I ask its publication in your valuable journal, and I hope that medical men in the South of England, where hay fever is common, will give it a trial and report upon it next summer. In Scotland hay fever is practically unknown. J. B. HANNAY

Cove Castle, Loch Long, N.B.

Red Rainbows

PROF. S. P. THOMPSON's letter (p. 459) makes me recall that when on September 2 last year I crossed Wales westwardly from Hereford, on a fine sunny day, the train ran into a misty shower after 6 p.m. at Machynlleth, and out of it as we neared the viaduct at Barmouth. The sun had been obscured for some time, when it suddenly shone out through a chink between sea and cloud, causing in the east a very beautiful red rainbow. Like Prof. Thompson, I was under the impression that the phenomenon was of no uncommon occurrence, so did nothing more than note it in my diary. HENRY MUIRHEAD

Cambuslang, September 16

Infusorial Parasites on Stickleback

ONE day in June, when examining a very small stickleback under the microscope, I was surprised to find it infested by numbers of infusoria, evidently parasitic upon it. This led me to examine others from the same water, viz., a pond very rich in infusorial life generally, as also specimens from the river close at hand. Every specimen from the pond was similarly attacked, while none of those from the river were so. The parasite is apparently *Trichodina pediculus*, which is stated to be parasitic upon *Hydra vulgaris*. Want of literature on the subject has prevented me from following the matter up, but it seems that I have found, if not a new species, at least a new host for a known species. I shall be glad if any of the readers of NATURE can give me any information on the subject. N. H. POOLE

Charterhouse, Godalming

Photographing Diffraction Rings—Optical Phenomenon

THE peculiar character of the photographs of an opening to the sky in the dark Cyclopean gallery at Tiryns, to which Mr. W. J. Stillman calls attention (NATURE, vol. xxiv. p. 260), finds an obvious explanation in the well-known optical phenomenon of diffraction rings, produced when a beam of light is transmitted through a small circular aperture, and viewed by means of a lens. Had your "Cecropian" correspondent examined the image of the illuminated opening by the assistance of a lens, the phenomenon of concentric coloured rings would, doubtless, have been recognisable to the eye. Hence the only point of interest in the phenomenon observed by Mr. Stillman is the significant fact that in securing the fleeting images of the rings on the gelatine plate—the actinic rays being alone effective—alternate dark and bright concentric rings are produced, as in the case of homogeneous or monochromatic light, instead of the coloured rings seen by interposing a lens between the aperture and the eye. In other terms, the impressions on the gelatine plate being due to the action of the monochromatic actinic rays, the theory of diffraction shows that the concentric rings should be alternately dark and bright. This is an important circumstance in the applications of photography to such investigations. JOHN LE CONTE

Berkeley, California, August 16

A Primitive Diving-Bell

IN NATURE, vol. xxiv. p. 201, it is stated that Herr Bulde has found a description of a primitive diving-bell in a work of Bartolini, 1674. The inventor appears to have been Franciscus Kesler, 1616. This description of Kesler's diving machine will also be found, together with representations of the same, in Schwenter's "Deliciæ Physico-Mathematicæ," 1636, a very rare and curious volume; so rare indeed that it is stated in *Cosmos*, January 27, 1860, "it is not to be found in the Imperial Library, nor in any of the public libraries of Paris." J. van Lennep, in *Notes and Queries*, December 15, 1859, p. 503, says "there is a Dutch translation of Schwenter, 1672; of this rare volume I fortunately possess a copy." N. S. HEINEKEN

Sidmouth, September 11

ITTAVIO LAUDI.—Messrs. Trübner, publishers, London, might be able to help you to get copies of the Chinese translations mentioned in Mr. Fryer's articles on "Science in China."

FREDERICK CURREY, M.A., F.R.S.

THE late Frederick Currey, whose death was announced in last week's NATURE, p. 475, was born at Eltham on August 4, 1819, educated at Eton and Trinity College, Cambridge, there obtaining a scholarship, and attaining his B.A. in 1841; three years later he proceeded to M.A., and was called to the Bar, afterwards practising as conveyancer and equity draughtsman.

His first public performance as a scientific writer was a translation of Schach's "Das Mikroskop" in 1853, a second edition of which was called for within two years. In the *Microscopical Journal* for 1854 he published some observations on two new fungi, and by the same channel he afterwards communicated several papers, chiefly on the obscure points in the reproduction of the lower cryptogams. The Greenwich Natural History Club was established in 1852, Mr. Currey being one of the earliest members, and the next year he read a paper on the

"Fungi of the Neighbourhood of Greenwich," which was printed in the fifth volume of the *Phytologist*. In 1857 a committee of that Club was appointed to draw up a report on the flora of the district; Mr. Currey was chosen its chairman, and drafted the report, which enumerated 395 fungi. In that year he contributed a paper to the Royal Society, which was printed in the *Proceedings*, On the Occurrence of Amorphous Starch in a Tuberaceous Fungus, a point of much interest, as starch is rarely found in fungi. He was elected Fellow of the Linnean Society in 1856, and in 1858 Fellow of the Royal Society also; in 1860 he was appointed secretary of the former society in succession to Mr. J. J. Bennett, a post which he held until 1880, when he relinquished that office and acceded to the less arduous duties of treasurer, retaining that appointment until his death. In 1862 the Ray Society issued a translation, with considerable additions, from Hofmeister, entitled "On the Germination, Development, and Fructification of the Higher Cryptogamia," a task which Mr. Currey undertook in 1859; the following year he edited a second edition of Dr. Badham's "Esculent Funguses of England," in which he made as few alterations as possible. Several papers on mycological subjects appear in the *Transactions* of the Linnean Society, which will be found in the "Catalogue of Scientific Papers," under his name, his last contribution being, "On a Collection of Fungi made by Mr. Sulpiz Kurz," in 1876.

By his death the Linnean Society has lost an experienced officer, and its members a valued friend whose place it will be hard to fill.

THE AMERICAN ASSOCIATION

THE thirtieth meeting of the American Association for the Advancement of Science commenced at Cincinnati on August 16. Always a hot place in summer, the temperature at Cincinnati during the past two months has been at intervals unusually high, running above 100° for several days together. This and the fear of its recurrence no doubt induced many to stay away who would otherwise have been present. However, a goodly number—over 400—of new members were enrolled, and many papers of interest were read. Some of the visitors from the Eastern States were amazed to find that Cincinnati was not a little backwoods town, but a city deserving in many ways the title she claims, "The Queen City of the West." The ignorance of many of the inhabitants of the Eastern States in regard to the West is only equalled by the ignorance of Europeans in regard to America generally.

The citizens of Cincinnati exerted themselves to welcome their guests, and did it well. Their bodily wants were amply supplied, and this, to those coming from a distance and being strangers, was a matter of no small moment. Visitors known or bearing introductions were in very many cases entertained privately, and a free lunch was provided by subscription for all members daily from 1 to 2.30 p.m. The Western Union Telegraph Company, whose wires extend over almost the whole Union, gave the members the use of their lines for communication with their families, free of charge, at whatever distance they might be situated. Connection was made by telephone between the hall of meeting and the Central Telegraph Office, and between the various rooms in the building, so that it was possible to send a message without leaving the building—to ask, for example, what paper was being read in any other section. The City and Suburban Telegraph and Telephone Companies also extended the same privileges to members of the Association. The various Express Companies offered to convey light parcels containing specimens for them, free of charge, between their homes and the place of meeting, and heavy ones at low rates. Of course there was a post-office and a parcels office in the hall, with convenience and materials for writing. There was however a little deficiency in finger placards to direct strangers over the great building in which the meetings were held—the new music hall.

Besides the above, an excursion was arranged for Saturday afternoon to the Zoological Garden, one of the best in the country, and capable, if well supported, of becoming *the* best. Free transportation through the streets by the horse cars was provided, and a sumptuous repast prepared for the visitors before leaving. In the same way the Anthropological Section and others were taken out to Madisonville, about ten miles from the city, on Monday to see a prehistoric cemetery recently discovered, from which several hundred skeletons, uncounted arrowpoints, animal bones, horns, and teeth, and pieces of pottery have been exhumed during the past two years. The train stopped at the place to set them down, and again on its return to take them up. After the adjournment of the meeting an excursion was arranged to the Mammoth Cave, about 150 miles distant, and another to Chattanooga, about 300 miles over the new Southern Railway, both free so far as the travelling was concerned.

It will be evident that Cincinnati did her best to entertain her scientific visitors, and the latter carried away pleasant and lasting impressions of the city where the fifth and the thirtieth meetings of the Association were held, the latter having been one of the most successful in its history. E. W. CLAYPOLE

Another correspondent writes:—

The Cincinnati meeting proved to be one of the best ever held by the Association. In attendance of members it was much surpassed by the Boston meeting last year, which reached the phenomenal number of 997; the number at Cincinnati was about 550, being more than double that at any previous meeting in the West. Over 400 persons joined the Association. Nearly 200 papers were presented; it is not derogatory to any author to say that these papers were of average interest only, and comprised no startling announcements. The general management was very successful. The chief feature of the meeting was the adoption of the important changes in the constitution which are expected to simplify business in the future. If any complaint can be made, it is of the rather generous disposition on the part of the Sectional Committees towards authors whose productions are of doubtful novelty and uncertain value. There is a tendency also to overrun the allotted time; papers for which twenty-five minutes are asked consume seventy minutes, and those of ten minutes extend to thirty minutes. The remedy of this evil lies with the chairmen of the sections, and it is hoped that they will hereafter exert their powers more frequently. The social features of the meeting were very enjoyable; the reception at Highland House, and the daily lunches served in the Exhibition building brought together citizens and members in pleasant intercourse.

Prof. Wm. B. Rogers of Boston, the first presiding officer of the Association, was elected an Honorary Fellow, on the unanimous recommendation of both Sections A and B. Prof. Rogers is the first Honorary Fellow chosen by the Association.

The amendments to the constitution proposed at the Boston meeting were adopted almost unanimously. They provide for the formation of nine sections, as follows:—A, Mathematics and Astronomy; B, Physics; C, Chemistry; D, Mechanical Science; E, Geology and Geography; F, Biology; G, Histology and Microscopy; H, Anthropology; I, Economic Science and Statistics.

Each of the above Sections is to have its own chairman, who is also Vice-President of the Society, and its own Secretary. The amended constitution also creates the office of Assistant-General Secretary, makes certain changes in the composition of the Standing Committee,

and of the nominating committee. These alterations make the organisation more like that of the British Association for the Advancement of Science, and became necessary on account of the recent rapid growth of the Association. The membership has doubled within two years, being now about 1800.

The recommendation of the Standing Committee to meet in 1882 at Montreal was adopted with acclamation. The invitation from Minneapolis for 1883 was referred to the Standing Committee.

The Association elected Dr. J. W. Dawson of Montreal President for the ensuing year. The time of meeting was fixed for the fourth Wednesday in August, 1882. Nearly seventy Fellows were elected by ballot, and the following officers for 1882, in accordance with the recommendation of the Nominating Committee, were unanimously elected:—

Officers for 1882. Vice-presidents: Section A, Mathematics and Astronomy, Prof. William Harkness, U.S.N.; Section B, Physics, Prof. T. C. Mendenhall of Columbus; Section C, Chemistry, Prof. H. Carrington Bolton, Ph.D., of Hartford, Conn.; Section D, Mechanical Science, Prof. W. P. Trowbridge, Ph.D., of Columbia College, New York; Section E, Geology, Prof. E. T. Cox of San Francisco; Section F, Biology, Capt. W. H. Dall of Washington, D.C.; Section G, Histology and Microscopy, Prof. A. H. Tuttle of Columbus, Ohio; Section H, Anthropology, Prof. Daniel Wilson of Toronto; Section I, Economic Science and Statistics, E. B. Elliot of Washington, D.C.

We have already (vol. xxiv. p. 455) referred to the action taken by the Association in reference to science degrees.

The following are some of the principal papers read at the meeting:—

In Section A: Magnetic survey of Missouri, by Prof. F. E. Nipher; on the methods of determining the solar parallax with special reference to the coming transit of Venus, by Prof. William Harkness; on the wave-lengths of the principal lines of the solar spectrum, by Prof. T. C. Mendenhall; experiments to determine the comparative strength of globes and cylinders of the same diameter and thickness of sides, by Samuel Marsden; historic notes on cosmic physiology, by Dr. T. Sterry Hunt; upon the use of the induction balance as a means of determining the location of leaden bullets imbedded in the human body, by Prof. Alex. Graham Bell; upon a new form of electric probe, by the same; on a new method of applying water-power of small head to effect the direct compression of air to any required high pressure, by Prof. H. T. Eddy; the needle telephone, a new instrument by Dr. Goodman of Louisville, Ky., by Dr. J. Lawrence Smith; an improved sonometer, by W. Le Conte Stevens; on the great outburst in comet *b*, 1881, observed at the Cincinnati Observatory, by Prof. Ormond Stone; method of determining the value of the solar parallax from meridian observations of Mars, by Prof. J. R. Eastman; numbers of cometary orbits relative to perihelion distance, by Prof. H. A. Newton; numerical elements of the orbits of the seven electrical vortices to whose motions atmospheric storms are principally due, with the processes by which they have been derived, and examples given of the application of the formula by which their positions on the surface of the earth can be computed for any given time, by Thomas Bassnett; a preliminary investigation of the two causes of lateral deviation of spherical projectiles, based on the kinetic theory of gases, by Prof. H. T. Eddy; note on the theory of the flight of elongated projectiles, by Prof. H. T. Eddy; on the mechanical principles involved in the flight of the boomerang, by Prof. H. T. Eddy; the electrophore and electric lighting, by Mr. E. B. Elliott; nodular concretions in meteoric iron, bearing on the origin of same, by Dr. J. Lawrence Smith; an anomalous magnetic property of a specimen of iron, by Dr. J. Lawrence Smith; on the errors to which self-registering clinical thermometers are liable, by Dr. Leonard Waldo; a new radiometer, by Dr. H. Carmichael; a new differential thermometer, by Dr. H. Carmichael; note on an experimental determination of the value of π, by Prof. T. C. Mendenhall; remarks upon, and an exhibition of, Japanese magic mirrors, by the same; on standard time, by E. B. Elliott; note on a comparison of Newcomb's tables of Uranus and Neptune, with those of the same planets by Leverrier, by D. P. Todd; universal energy of light, by Pliny Earle Chase; electricity, magnetism, gravitation—their phenomena considered as the manifestations of one force, by S. S. Parsons.

In Section B: On the influence of the structure of the nerve-fibres upon the production and conduction of nerve-force, by H. D. Schmidt; on the action of pilocarpin in changing the colour of the human hair, by Dr. D. W. Prentiss; the unification of geological nomenclature, by Dr. R. Owen; the life-unit in plants, by Prof. B. D. Halsted; recent discoveries, measurements, and temperature observations made in Mammoth Cave, Ky., by Rev. H. C. Hovey; influence of forests upon streams, by David D. Thompson; note on the segmentation of the vertebrate body, by Charles Sedgwick Minot; phenomena of growth in plants, by D. P. Penhallow; the recurrence of faunas in the Devonian rocks of New York, by H. S. Williams; a contribution to Croll's theory of secular climatal changes, by W. J. McGee; the evidence from the drift of Ohio in regard to the origin of Lake Erie, by E. W. Claypole; on some relations of birds and insects, by S. A. Forbes; Niagara River, its cañon, depth, and wear, by Wm. Hosea Ballou; evolution and its place in geology, by Edward S. Edmunds.

In Section C: Is the law of repetition the dynamic law underlying the science of chemistry? by Miss V. K. Bowers; evidences of atomic motion within molecules in liquids, as based upon the speed of chemical action, by Prof. R. B. Warder; the constitution of the "atom" of science, by Mrs. A. B. Blackwell; the sources of nitrogen in plants, by Prof. W. O. Atwater; notes in experimental chemistry, by Prof. A. B. Prescott; determination of phosphorus in iron, by Dr. J. Lawrence Smith; the liquefaction of glass in contact with water at 250° C., by Prof. H. Carmichael; the chemistry of fish and invertebrates, by Prof. W. O. Atwater; notes in experimental chemistry, by Prof. Albert B. Prescott; the quantitative estimation of nitrogen, by Prof. W. O. Atwater; the quantitative estimation of chlorine, by Prof. W. O. Atwater; the nitrogenous constituents of grasses, by Clifford Richardson.

In Section D (Anthropology): Animal myths of the Iroquois, by Mrs. Erminnie A. Smith; antiquity of man in America, by W. de Haas; progress of archæological research, by W. de Haas; the mound builders: an inquiry into their assumed southern origin, by W. de Haas.

In Section E (Microscopy): On a convenient method of expressing micrometrically the relation between English and metric units of length on the same scale, by William A. Rogers and George F. Ballou.

In Section F (Entomology); On the length of life of butterflies, by Prof. W. H. Edwards; on the life duration of the Heterocera (moths), by Prof. J. A. Lintner; how does the bee extend its tongue? by A. J. Cook; the egg-case of *Hydrophilus triangularis*, by Dr. C. V. Riley; on the oviposition of *Prodoxus decipiens*, by the same; the cocoon of Gyrinus, by the same; suggestions of co-operation in furthering the study of entomology, by Prof. B. P. Mann.

THE BRITISH ASSOCIATION

REPORTS

Report of the Committee on Tidal Observations in the English Channel and the North Sea, by J. N. Shoolbred.—In the report it was stated that no official reply had been received by the Committee as to having an international datum for observations, or as to maritime governments giving facilities for detailed observations. The Committee urged the desirability of carrying out a series of observations on the Azores. The Portuguese Government had established a station for registering tides, as had also our own Government at Dover. The Committee hoped before long to have a series of observations giving most important results.

Report of the Committee for Underground Temperature, by Prof. Everett.—In the report it was stated that the temperature varied from one degree for 30 feet to one degree for 100 feet in going down beneath the surface of the earth in different places. During the past year observations have been made in the East Manchester coal field, the Talavgoch lead mine, Flintshire, and at the Radstock collieries, Bath. With regard to the observations in the East Manchester coal-field, these were respectively

taken at Ashton Moss, Bredbury, and Nook Pit: the temperatures were as follows:—

Place.	Depth. Feet.	Temperature.
Ashton Moss	2790	85°·3
Bredbury Colliery	1020	62·0
Nook Pit	1050	62·3

The increments of temperature would be as follows:—

Place.	Depth. Feet.	Increase of temperature.
Ashton Moss	2790	36°·3
Bredbury Colliery	1020	13
Nook Pit	1050	13·3

This gave for each degree of increase, Ashton Moss 76·9 feet, Bredbury 78·5, and Nook Pit, 79 feet. In Flintshire the observations showed great irregularity last year, and the observations taken this year increased the irregularity. The observations were taken at a place in the lead mine at a depth of 660 feet; at this depth the temperature was 62°, 48° being assumed to be the surface temperature which gave an increase of 14° in 660 feet, or 1° in 47 feet. At the Radstock colliery observations were made at three places. The Wells May Pit, 560 feet deep, the Ludlow Pit, 1000 feet, and a third station in the same pit 810 feet deep, the surface temperature being assumed 50°, the rate of increase was found to be 11°·7 in 560 feet, and 13° for both 810 feet and 1000 feet.

Mr. W. M. Hicks read his report *On Recent Progress in Hydrodynamics*, which he related to the investigations undertaken since 1846, the date of Prof. Stokes's well-known report.

A Report of the Committee on Fundamental Invariants, by Prof. Sylvester, was read.

Report of Committee on Mathematical Tables, by James Glaisher, F.R.S.—The author stated that the factor table for the sixth million was now completed and stereotyped, and he had the pleasure to exhibit to the section proofs of the whole of this million taken from the stereotyped plates. The factor tables for the fourth and fifth millions had been already published, so that now the gap of three millions between the tables of Burckhardt (1817) and those of Dase (1861) was completely filled up. The introduction to the sixth million would shortly be completed, when this volume would appear.

Report on the Tertiary Flora of the Basalt of the North of Ireland, drawn up and illustrated by W. H. Bailey, Geological Survey of Ireland.

Mr. Sclater, in the absence of Mr. Thiselton Dyer, presented the *Report of the Committee appointed for the Purpose of Investigating the Natural History of Timor-laut*.—In a letter addressed to Sir Joseph Hooker, director of the Royal Gardens, Kew, Mr. H. O. Forbes had written from Sumatra, offering, if some assistance could be forwarded to him, to attempt an expedition to Timor-laut, for the purpose of investigating its natural history —"an object," as Mr. Forbes stated, "the accomplishment of which is desired both by botanists and zoologists." An application on Mr. Forbes's behalf was accordingly made to the British Association, and a sum of 50*l*. was voted by the General Committee at the Swansea Meeting to be placed at the disposal of the Committee, to whom the conduct of the matter was intrusted. The action taken by the Association was communicated to Mr. Forbes, and a letter was received in reply. This was the most recent information which the Committee possessed as to his plans. It was somewhat doubtful whether, owing to insufficiency of funds, he was able to start. At any rate, the grant made at Swansea remained in the hands of the Committee. The expedition was obviously attended with some difficulty, if not danger. Its success must be largely dependent on fortunate accident. The Committee, however, thought that there was a reasonable chance of the work being done, and therefore recommended their reappointment, and that a further sum of 100*l*. be placed at their disposal.

The *Report of the Committee on the Natural History of Socotra* gave an account of the progress made in working out Prof. Bayley Balfour's collections, and recommended its reappointment with a somewhat extended sphere, so as to embrace the adjoining highlands of Arabia and Somali Land.

A Report by Mr. R. J. Ussher *On Caves and Kitchen-Middens at Cappagh, County Waterford*, described an extensive series of kitchen-middens which had been excavated with the aid of the Association grant. One of these filled a cave of considerable extent, the more ancient parts of which had not been yet explored. The excavated parts yielded large quantities of bones and implements which did not furnish very striking results.

The *Report of the Committee for the Investigation of the Influence of Bodily Exercise on the Elimination of Nitrogen*, presented by Mr. North, detailed the delays in commencing the actual investigation, owing to the necessary devising and construction of instruments. It was hoped however that valuable results might be obtained before the meeting at Southampton.

The *Report of the Committee on the Zoological Station at Naples* was read by Mr. Percy Sladen. He stated that the laboratory had added micro-spectroscopic and polariscopic apparatus, a new Du Bois-Reymond's section apparatus, and a valuable series of chemico-physiological apparatus; and the breeding and aerating apparatus have been successfully worked. Two of the monographs of the Fauna and Flora of the Gulf of Naples have been published in the last year, viz., the Ctenophora by Dr. Carl Chun, and the species of the genus Fierasfer by Dr. Carlo Emery. The monographs on the Pantopoda, by Dr. Dohrn; the Corallines, by Solms-Laubach; and on Balanoglossus, by Dr. J. W. Spengel, are in a forward state. The *Zoologische Jahresberichte* for 1880 is in the press, and will be issued in four parts. The novel method of investigating the sea-bottom by means of diving apparatus has been successfully used in many clear portions of the bed near Naples. Many details of improvement have been made, and many fissures and cavities may be explored which are inaccessible to the trawl or the dredge, and sponges, hydroids, actiniæ, bryozoa and planarians, nudibranchs and algæ may be obtained *in situ*. During the year Mr. Allen Harker has studied at the British Association table, chiefly on the circulation and respiration in the polychætous annelids; also Mr. F. G. Penrose, who investigated the circulation in *Solen legumen*. Mr. P. Geddes desires to prosecute special researches during the coming year, and will be accompanied by an assistant. Thirty-four naturalists have worked at the station during the year, many memoirs have been published, large quantities of specimens have been sent to foreign museums and naturalists, as well as microscopical preparations.

Report of the Committee, consisting of Mr. James Heywood, F.R.S., Mr. William Shaen, Mr. Stephen Bourne, Mr. Robert Wilkinson, the Rev. W. Delany, Mr. N. Story Maskelyne, M.P., F.R.S., Dr. Silvanus P. Thompson, Miss Lydia E. Becker, Sir John Lubbock, Bart., M.P., F.R.S., Prof. A. W. Williamson F.R.S., Mrs. Augusta Webster, and Dr. J. H. Gladstone, F.R.S. (Secretary), on the manner in which Rudimentary Science should be taught, and how Examinations should be held therein, in Elementary Schools.—Rudimentary science is taught in public elementary schools in the form of—I. Object lessons; II. Class subjects under article 19, c. 1, of the New Code; III. Specific subjects under Schedule IV. of the same Code; IV. Science subjects preparatory to entering classes in connection with science schools.

I. Object lessons are attempted in a large number of infant schools, and in some instances are very effective in developing the perceptive powers and intelligence of the children; but in other cases they are too formal, and left too much to the junior teachers. In boys' and girls' schools they frequently appear upon the time table, especially where, as in the schools of the London Board, they are looked upon as a necessary part of the instruction: but they are generally given in an unsystematic, and often in an unsatisfactory manner.

II. The teaching of science as a class subject under the Code only commenced last October, and thus no examinations have yet been held under it. Natural history, physical geography, natural philosophy, &c., are mentioned in article 19, c. 1, and it is stated that the instruction should be given "through reading lessons, illustrated, if necessary, by maps, diagrams, specimens, &c."; but the teachers are limited to two subjects, and the old subjects, grammar, history, geography, and needlework naturally retain their place in the great majority of the schools. Suitable reading-books for these rudimentary science subjects have scarcely yet came into existence.

III. The specific subjects of the fourth schedule include mechanics, animal physiology, physical geography, botany, and domestic economy, but only two subjects may be taken (or three if the child has passed Standard VI.); and the schedule also includes English literature, mathematics, Latin, French, and German. Literature is a general favourite; and domestic

economy is obligatory in girls' schools if any specific subject is taken at all; so that the chance of any of the others being introduced is very much diminished. It must also be remembered that these subjects are only allowed to be taught to children in the Fourth Standard and upwards; while only about one-fifth of the children in the boys' and girls' schools are to be found at present in these standards. According to the Report of the Committee of Council for Education recently issued, there were 476,761 children presented for examination in these standards, of whom the following numbers only were examined in the science subjects:—

Mechanics	2,109
Animal physiology	24,725
Physical geography	34,288
Botany	1,853
Domestic economy	50,797

Out of 489 boys' and girls' departments under the London School Board, the specific science subjects were taken up, as follows, during the year 1880:—

Mechanics in	4	departments
Animal physiology in	123	,,
Physical geography in	112	,,
Botany in	9	,,
Domestic economy	172	,,

Mr. Hance of the Liverpool School Board has favoured us with an account of the systematic scientific instruction which is given in the Board schools of that town by a special science staff. The subject selected for the boys is mechanics as defined in the New Code, with a considerable development in the direction of elementary physics. It has been in operation since 1877, and the results for the year 1880–81 are given in the following table:—

Year 1880–81.	Number presented.	Number passed.	Percentage of passes.
Stage I.	797	442	55·46
,, II.	398	261	65·59
,, III.... ...	122	82	67·21
Total... ...	1317	785	59·6

Domestic economy is also taught to the girls in a similar manner. In Birmingham 1200 scholars are receiving scientific instruction in the schools of the Board, and it is stated that the teachers uniformly find that "it added interest to the work of the school, that the children were eager to be present, and that the lessons were enjoyed, and were in fact giving new life to the schools." The Board have found the results so satisfactory that they are now furnishing their newest school with a laboratory and lecture room.

IV. As to science-teaching which does not fall under the provisions of the New Code it is not probable that any large amount is attempted. In Manchester, however, the Board gives instruction to 404 children, all of whom have passed Standard VI., the highest ordinary standard, in the following subjects: physiology; acoustics, light, and heat; magnetism and electricity; chemistry; practical chemistry; botany. This teaching is illustrated by means of good apparatus, &c., and has had a very beneficial effect upon the science and art classes of the town. When it is considered that the provisions of the Code naturally form, in almost all cases, the extreme limit of what will be attempted in the schools, it is important that they should be placed as high as possible. This will be a great advantage to the stronger schools, and no disadvantage to the weaker ones, as the higher branches of science teaching will of course be optional. Your committee have, therefore, arrived at the following conclusions:—

I. *As to object lessons.* That it is very desirable that Her Majesty's Inspectors should take object lessons into account in estimating the teaching given in an infant school; and that they should examine the classes in the graded schools wherever object lessons are given.

II. *As to class subjects.* That the teaching of such subjects as natural history, physical geography, natural philosophy, &c., should not necessarily be "through reading lessons," as oral lessons, "illustrated by maps, diagrams, specimens, &c.," are undoubtedly better when given by a teacher duly qualified to handle these subjects. They are of opinion, also, that it will be desirable to allow a larger number of class subjects to be taken up in any particular school, and to give in such case a proportionately increased grant.

III. *As to specific science subjects.* That a knowledge of the facts of nature is an essential part of the education of every child, and that it should be given continuously during the whole of school life from the baby class to the highest standard. Of course in early years this teaching will be very rudimentary; but by developing the child's powers of perception and comparison it will prepare it for a gradual extension of such knowledge. They consider also that the early teaching must be very general, while the later may be more specific; they think, however, that the science subjects as given in Schedule IV. are fairly open to objection, as being somewhat too ambitious in their nomenclature and in their scope, and that they ought not to be attempted unless the child has had a previous training in natural knowledge before entering the fourth standard. Thus the specific scientific subjects ought not to be distinct, as they practically are at present, from the previous teaching; greater latitude of choice might be allowed in them; and while they should not afford technical instruction they should prepare the way for any technical classes or schools into which the children may subsequently enter. In regard to domestic economy they are of opinion that most of the points embraced in the schedule would be useful to boys as well as to girls.

IV. *As to examinations.* That in the appointment of Her Majesty's Inspectors some knowledge of natural science should be considered as absolutely requisite; that in examining the children they should direct their inquiries so as to elicit not so much their knowledge of special facts as their intelligent acquaintance with the world of nature around them; and that this may be much better done by oral examination than by paper work.

SECTION A—MATHEMATICAL AND PHYSICAL

On the Economy of Metal in Conductors of Electricity, by Sir W. Thomson.—The most economical size of the copper conductor for the electric transmission of energy, whether for the electric light or for the performance of mechanical work, would be found by comparing the annual interest of the money value of the copper with the money value of the energy lost in it annually in the heat generated in it by the electric current. The money value of a stated amount of energy had not yet begun to appear in the City price lists. If 10*l*. were taken as the par value of a horse-power night and day for a year, and allowing for the actual value being greater or less (it might be very much greater or very much less) according to circumstances, it was easy to estimate the right quantity of metal to be put into the conductor to convey a current of any stated strength, such as the ordinary strength of current for the powerful arc light, or the ten-fold strength current (of 240 webers) which he (Sir William Thomson) had referred to in his address as practically suitable for delivering 21,000 horse-power of Niagara at 300 miles from the fall. He remarked that (contrary to a very prevalent impression and belief) the gauge to be chosen for the conductor does not depend on the length of it through which the energy is to be transmitted. It depends solely on the strength of the current to be used, supposing the cost of the metal and of a unit of energy to be determined. Let A be the sectional area of the conductor; s the specific resistance (according to bulk) of the metal; and c the strength of the current to be used. The energy converted into heat and so lost, per second per centimetre, is sc^2/A ergs. Let p be the proportion of the whole time during which, in the course of a year, this current is kept flowing. There being $31\frac{1}{2}$ million seconds in a year, the loss of energy per annum is

$$31 \cdot 5 \times 10^6 psc^2/A \text{ ergs} \quad . \quad . \quad . \quad (1)$$

The cost of this, if E be the cost of an erg, is

$$31 \cdot 5 \times 10^6 psc^2 E/A \quad . \quad . \quad . \quad (2)$$

Let V be the money value of the metal per cubic centimetre. The cost of possessing it, per centimetre of length of the wire, at 5 per cent. per annum, is

$$VA/20 \quad . \quad . \quad . \quad . \quad (3)$$

Hence the whole annual cost, by interest on the value of the metal, and by loss of energy in it, is

$$\frac{1}{20} VA + \frac{31 \cdot 5 \times 10^6 psc^2 E}{A} \quad . \quad . \quad . \quad (4)$$

The amount of A to make this a minimum (which is also that which makes the two constituents of the loss equal) is as follows:—

$$A = \sqrt{\left(31 \cdot 5 \, . \, 10^6 psc^2 E / \frac{V}{20}\right)}$$
$$= c\sqrt{(63 \, . \, 10^7 psE/V)} \quad . \quad . \quad (5)$$

Taking 70*l.* per ton as the price of copper of high conductivity (known as "conductivity copper" in the metal market), we have ·00007*l.* as the price of a gramme. Multiplying this by 8·9 (the specific gravity of copper), we find, as the price of a cubic centimetre,

$$V = \cdot 00062 l. \quad . \quad . \quad . \quad (6)$$

and the assumption of 10*l.* as the par value of one horse-power day and night for 365 days gives, as the price of an erg,

$$10l./(31\tfrac{1}{2}\times 10^6 \times 74 \times 10^8) = \frac{1}{23\times 10^{14}} \text{ of } 1l. \qquad (7)$$

Supposing the actual price to be at the rate of $e \times 10l.$ for the horse-power year, we have

$$E = \frac{e}{23\times 10^{14}} \text{ of } 1l. \quad . \quad . \quad . \quad (8)$$

Lastly, for the specific resistance of copper we have

$$s = 1640 \quad . \quad . \quad . \quad . \quad (9)$$

Using (8) and (9) in (5) we find,

$$A = c\sqrt{\frac{63\times 10^7 \times 1640 \times pe}{23\times 10^{15}\times \cdot 00062}} = c\sqrt{\frac{pe}{1\cdot 38}} \quad . \quad (10)$$

Suppose, for example, $p = \cdot 5$ (that is, electric work through the conductor for twelve hours of every day of the year to be provided for), and $e = 1$. These suppositions correspond fairly well to ordinary electric transmission of energy in towns for light, according to present arrangements. We have—

$$A = c\sqrt{\frac{1}{27\cdot 6}} = \frac{c}{5\cdot 25} \doteq \cdot 19.c.$$

That is to say, the sectional area of the wire in centimetres ought to be about a fiftieth of the strength of the current in webers. Thus, for a powerful arc-light current of 21 webers, the sectional area of the leading wire should be ·4 of a square centimetre, and therefore its diameter (if it is a solid round wire) should be ·71 of a centimetre. If we take $e = \frac{1}{27\cdot 8}$, which corresponds to 1900*l.* a year as the cost of 5250 horse-power (see Presidential Address, Section A), and if we take $p = 1$, that is, reckon for continued night and day electric work through the conductor, we have—

$$A = \frac{c}{\sqrt{381}} \doteq \frac{c}{19\cdot 5};$$

and if $c = 24$, $A = 1\cdot 24$, which makes the diameter 1·26 centimetres, or half an inch (as stated in the Presidential Address). But even at Niagara it is not probable that the cost of an erg can be as small as $\frac{1}{360}$ of what we have taken as the par value for England; and probably therefore a larger diameter for the wire than ½ inch will be better economy if so large a current as 240 webers is to be conducted by it.

Illuminating Powers of Incandescent Vacuum Lamps with Measured Potentials and Measured Currents, by Sir William Thomson and James T. Bottomley.—The electromotive force used in these experiments was derived from Faure secondary batteries, kindly supplied for the purpose by the Société la Force et la Lumière in their London office. Two galvanometers were used simultaneously, one (called the *potential galvanometer*) for measuring the difference of potentials between the two terminals of the lamp, the other (called the current galvanometer) for measuring the whole strength of the current through the lamp. The potential galvanometer had for its coil several thousand metres of No. 50 (B.W.G.) silk-covered wire (of which the copper weighs about one-twentieth gramme per metre, and therefore has resistance of about 3 ohms per metre). Its electrodes were applied direct on the platinum terminals of the lamp. The current galvanometer had for its coil a single circle, of about 10 centimetres diameter, of thick wire placed in the direct circuit of the lamp, by means of electrodes kept close together to a sufficient distance from the galvanometer to insure no sensible action on the needle except from the circle itself. The directive force on the needle which was produced by a large semicircular horseshoe magnet of small sectional area was about 2½ c.g.s., or fifteen times the earth's horizontal magnetic force in London. This arrangement would have been better for the potential galvanometer also than the plan actually used for it, which need not be described here. The scale of each galvanometer was graduated according to the natural tangent of the angle of deflection, so that the strength of the current was simply proportional to the number read on the scale in each case. Three lamps were used, Nos. II. and III. of a larger size than No. I. The experiment was continued with higher and higher potentials on each lamp till its carbon broke. The illuminating power was measured in the simplest and easiest way (which is also the most accurate and trustworthy), by letting the standard light and the lamp to be measured shed their lights nearly in the same direction on a white ground (a piece of white paper was used); and comparing the shadows of a suitable object (a pencil was used); and varying the distance of the standard light from the white ground till the illuminations of the two shadows were judged equal. The standard used was a regulation "standard candle," burning 120 grains of wax in the hour. The burning was not actually tested by weighing, but it was no doubt very nearly right; nearly enough for our purpose, which was an approximate determination of the illuminating powers of each lamp through a wide range of electric power applied to it. The following results were obtained:—

LAMP No. I.

No. of experiment.	Cells.	Volts.	Webers.	Volts × webers ÷ 10 ≐ kilogram-metres.	Horse-power.	Candles	Candles per horse-power.
1	26	56·9	1·21	6·88	·093	11·6	125
2	30	65·5	1·46	9·56	·129	25	194
3	32	70·2	1·64	11·51	·156	42	263
4	33	71·8	1·74	12·48	·170	38	224
5	34	74·1	1·81	13·42	·181	44	243
6	35	76·1	1·82	13·86	·187	55	294
7	36	78·0	1·99	15·52	·210	63	300
8	37	80·3	2·06	16·54	·224	66	295
9	38	81·9	2·06	16·88	·228	76	333
10	39	84·6	2·06	17·43	·235	82	349
11	40	87·0	2·10	18·27	·247	84	340
12	42	90·9	2·17	19·72	·267	102	382
13	44	92·0	2·17	19·96	·270	89	330
14	46	99·1	2·21	21·91	·296	114	385
		Carbon of lamp broke with same power, immediately after the measurement of the light was completed.					

LAMP No. II.

No. of experiment.	Cells.	Volts.	Webers.	Volts × webers ÷ 10 ≐ kilogram-metres.	Horse-power.	Candles	Candles per horse-power.
1	40	89·7	2·207	19·8	·27	49	181
2	42	93·3	2·296	22·42	·29	68	234
3	43	95·4	2·38	22·71	·31	76	245
4	44	98·8	2·49	24·60	·33	101	306
5	46	103·0	2·63	27·09	·37	117	316
6	50	106·9	2·74	29·29	·40	147	367
7	52	110·8	2·85	31·56	·43	189	440
8	54	117·0	2·95	34·53	·47	196	417
9	56	119·8	2·95	35·34	·47	186	388
10	58	121·8	2·98	36·29	·49	177	361
11	40	87·0	2·14	18·62	·25	35	140
12	42	89·7	2·24	20·09	·27	42	156
13	60	122·8	3·06	37·58	·51	186	365
14	62	126·0	3·13	39·44	·53	180	340
15	66	132·4	3·24	42·89	·57	222	383
	70	Carbon of lamp broke.					

LAMP No. III.

No. of experiment.	Cells.	Volts.	Webers.	Volts × webers ÷ 10 ≐ kilogram-metres.	Horse-power.	Candles	Candles per horse-power.
1	40	82·3	2·85	23·45	·31	68	219
2	50	101·8	3·90	39·70	·54	195	361
3	60	Carbon of lamp broke.					

Some of the irregularities of the results in the preceding tables are very interesting and important, as showing the effect of the

blackening of the glass by volatilisation of the carbon when too high electric power came to be applied. The durability of the lamp at any particular power must be tested by months' experience before the proper intensity for economy can be determined.

On some Uses of Faure's Accumulator in connection with Lighting by Electricity, by Sir W. Thomson.—The largest use of Faure's accumulator in electric lighting was to allow steam or other motive power and dynamos to work economically all day, or throughout the twenty-four hours where the circumstances were such as to render this economical, and storing up energy to be drawn upon when the light was required. There was also a very valuable use of the accumulator in its application as an adjunct to the dynamo, regulating the light-giving current and storing up an irregular surplus in such a manner that stoppage of the engine would not stop the light, but only reduce it slightly, and that there would always be a good residue of two or three hours' supply of full lighting power, or a supply for eight or ten hours of light for a diminished number of lamps. He showed an automatic instrument which he had designed and constructed to break and make the circuit between the Faure battery and the dynamo, so as automatically to fulfil the conditions described in the paper. This instrument also guarded the coils of the dynamo from damage, and the accumulator battery from loss, by the current flowing back, if at any moment the electro-motive force of the dynamo flagged so much as to be overpowered by the battery.

An Analysis of Relationships, by Dr. A. Macfarlane.—The paper contained a summary of the notation and elementary laws of an analytical method of dealing with such questions as, in the simplest cases, may be dealt with graphically by means of the genealogical tree. The subject is a special branch of the algebra of logic, and its development appears to the author to throw much light upon the fundamental principles of that science and to suggest important questions as to the relation of mathematical analysis to ordinary languages. The method has been applied to test the "systems of affinity and consanguinity" of Dr. Morgan of Rochester, New York.

On a Microscope with Arrangements for Illuminating the Sub-Stage, by E. Crossley.—The author stated that, using a bullseye condenser, the light from the lamp is thrown into the hollow horizontal axis of the microscope, and by means of a prism placed in the centre of this axis is reflected forwards in the direction of the axis on which the swinging sub-stage turns. The arm of a swinging sub-stage is made in the form of a box, and carries a second prism on the axis, on which it moves so as to intercept the rays of light coming from the first prism, and reflect them in the direction of the arm or box. At the end of the box is a third prism, which throws the rays of light forward on to the mirror, by means of which they are finally directed to the object on the stage. No change in the position of the microscope on its horizontal axis affects the direction of the light from the lamp, and whatever the position of the swinging sub-stage, whether above or below the stage, the illumination remains constant upon the object. The greatest facility is thus given for illuminating the object at any angle, and also seeing which is most suitable. The prisms used are one-inch, and give sufficient light for a one sixteenth-inch object-glass with a Ross B-eyepiece, a suitable condenser being used beneath the stage.

Observations of Atmospheric Electricity at Kew Observatory during 1880, by G. M. Whipple.—The author having spoken about the work already done, stated that he had devised a modification of Prof. Everett's method, and had constructed a glass scale by means of which curves could be tabulated with great facility. They had commenced tabulating and discussing the accumulated records, and he was able to state some of the facts derived from the curves for 1880. Having determined the atmospheric tension for every hour during the year when measurement of the trace was possible, the diurnal, monthly, and annual variations were computed. The months of maximum tension were January and March, and of minimum tension August and September. From the year's observations it was found that the laws vary in summer and winter; for the summer months the tension was greatest with an east wind and lowest with a north wind, whilst in winter the tension was greatest with north and north-west winds and least with south-east winds. From the results obtained it was found that light winds had a higher potential than strong winds. This, however, was not well marked in summer, but is almost entirely due to winter observations.

On Prof. Phillips' ainfall Observations made upon York Minster, by G. J. Symons, F.R.S.—The author, referring to the experiments established at York Minster, said that three gauges nearly identical in pattern were placed, one in the museum garden, one on the roof of the museum, and the third on a pole about 9 feet high placed on the centre tower of York Minster. These gauges were measured at various but identical times during the years 1832-1835, and the results were:—

		Total rain.	Ratio.
Museum garden 2 inches above ground	...	21.81 ...	100
Museum roof 44 feet ,,	... ...	17.39 ...	80
Minster tower 213 feet ,,	... ...	12.99 ...	60

Prof. Phillips stated the real amount of the diminution of rain at the upper stations depended upon the temperature of the seasons; the diminution did not vary uniformly as the square root of height, being in winter only as the cube root. Prof. Phillips' experiments soon became known, and Prof. Bache of Philadelphia set up four gauges at the angles of a square tower 162 feet high. His experiments were reported to the British Association in 1838. In 1861 Mr. Stanley Jevons made an important theoretical contribution to this investigation; he pointed out the weakness of the different extant theories, and showed that the phenomena observed were all consistent with the theory that the fall of rain was practically identical at all elevations, and that the observed differences were due to the imperfect collection by the gauges; he also stated that towers, buildings, and even the gauge itself, were obstacles to the rain-bearing current of air, and he concluded that less rain would fall on the summit of the obstacle than elsewhere, the surplus being carried forward to the lee side. Similar observations have been made during the last fifteen years, which have also been supplemented by anemometric observations, and these have proved that the difference in the amount collected was always greatest when the wind was strongest. The subject of late has been investigated by Mr. Dines, who placed several gauges 50 feet from the ground on the tower of his house. In 1877 Mr. Dines read a paper, and said that there was no actual decrease at the higher level, but a diminished collection due to eddy; he added that he found a large gauge on the tower caught much more than a small one. Mr. Rogers Field now took the matter up, and setting down the values so as to form curves he showed:—1. That the ratio of the rainfall on the tower to the rainfall on the ground depends on the force and direction of the wind. 2. That when there is no wind the rainfall on the tower is about the same as the rainfall on the ground. 3. That when there is wind the amount of rain falling on the tower will vary on different portions of the tower, the portion nearest the point at which the wind strikes the tower receiving less rain than falls on the ground, and the portion farthest from the point at which the wind strikes the tower receiving the same or more rain than falls on the ground. 4. That the excess of rain falling on the portion of the tower farthest from where the wind strikes will, to a large extent, compensate the deficiency of rain on the portion nearest to where the wind strikes, but whether to a sufficient extent to make the average amount of rain falling on the tower equal to that falling on the ground cannot be determined from these experiments. From these conclusions it is clear that if the building be flat and large, the fall in the middle of the roof ought to be nearly the same as on the ground, and in two instances this is so, first at Messrs. Marshall's factory at Leeds, and secondly Mr. Dines on a roof of 5000 square feet of area. Thus finally experimental evidence has corroborated the views of Mr. Stanley Jevons, given above.

On some of Bell and Tainter's Recent Researches and their Consequences, by W. Lant Carpenter.—The author referred to the researches of Messrs. Graham Bell and Tainter upon the sonorousness of matter under the influence of a beam of intermittent light, and described the receivers employed, in which substances are placed for examination. Porous substances gave louder sounds than dense ones, and those of a dark colour louder than light when a rapidly intermittent beam fell on them. An apparatus had been contrived by Mr. Tainter for measuring the relative sonorous powers of bodies, which was described by the author. He also stated that it had occurred to him that a modification of this apparatus might be employed for audibly estimating the relative intensities of two lights when intermittent beams fell from them upon two precisely similar receivers. The author proposed to call this instrument an audible photometer, and said that some rough experiments had somewhat justified his expectations.

On Magnetic Disturbances, by Prof. W. G. Adams, F.R.S.—The author, in considering magnetic disturbances, stated that certain facts about them had long been known; from the observations of Gauss in 1834 the disturbing power was found to increase in northern latitudes; it was also found that the appearance of a disturbance occurred in several places at the same instant, but with great differences of results. The force seemed to originate at a certain point in the interior of the earth, and the direction of the disturbing force seemed constant, yet great differences were observable at places not remote from one another. Sabine found that these disturbances had daily and yearly variations from their mean values, and that they have an eleven-year period corresponding to the appearance of spots upon the sun. It has been shown by observations that magnetic disturbances and electric currents on the earth are related; these electric currents in the earth have commonly been attributed to changes of temperature. The month of March, 1879, was chosen for a comparison of the photographic records of magnetic disturbances, and records for the whole month were sent from Lisbon, Coimbra, Stonyhurst, Vienna, St. Petersburg, and Bombay in the northern hemisphere, and from Melbourne and the Mauritius in the southern hemisphere. Taking the disturbances on March 15-16, 1879, as an instance, we see that soon after 10 a.m. Greenwich time on the 15th, a disturbance-wave happens, which shows first a diminution and then an increase of horizontal force at St. Petersburg, Vienna, Kew, and Lisbon, and also at Melbourne in Australia. At 9.30 p.m. of the same day a magnetic storm begins, and continues for about an hour. It is felt in the northern and southern hemispheres. At all stations in Europe the horizontal force is increased in the first part of the storm, and then diminished. At Lisbon the vertical force is first increased and then diminished, and at St. Petersburg and Stonyhurst there is a diminution in the vertical force at the same time as at Lisbon. Regarding the declination needles, we find that at St. Petersburg, Melbourne, and Bombay the declination westward is first increased and then diminished, whereas at Kew and Lisbon the motions are in opposite directions. At Bombay and Mauritius, near to, but on opposite sides of, the equator, the declination needles are deflected opposite ways. If we assume that by magnetic induction the earth's magnetism is altered, the position of the magnet which would cause the disturbance must be such that its pole, which attracts the marked end of our needle, must lie at the beginning of the disturbance to the east of Kew and Lisbon, to the north of Vienna, and to the north-west of St. Petersburg; the Lisbon vertical force curve also shows it to be below the surface of the earth. Hence an inductive action equivalent to a change of position of the north magnetic pole towards the geographical pole would account for these changes. The strengthening and weakening of a magnet with its north pole to the north on the meridian of Vienna might account for magnetic changes observed between 9.30 and 10.30 at night, Greenwich time, on March 15, 1879. In attempting to explain this disturbance by currents of electricity or discharges of statical electricity in the air above the needles, we must imagine that at first there is a strong current from the south-west over St. Petersburg, from the west over Vienna, and from the north-west over Kew and Lisbon, the vertical force needle at Lisbon showing that the current from the north-west lies somewhat to the east of Lisbon; that at the Mauritius this current is from the north, and at Bombay from the south. Thus we must imagine that a current of electricity passes down from the north-west to the south-east, going on towards the east over Vienna, and towards the north-east over St. Petersburg. This must be kept up very much along the same line throughout the first part of the disturbance, and then the current must be altered in strength in the same manner at all stations. An examination of the principal disturbances at Kew and at St. Petersburg seems to show that (1) a diminution in the horizontal force is accompanied by greater easterly deflections of the declination needle at St. Petersburg than at Kew; (2) increase of horizontal force is accompanied by greater westerly deflections at St. Petersburg than at Kew, or is sometimes accompanied by a westerly deflection at St. Petersburg and an easterly deflection at Kew. Only moderate disturbances have already been considered, and the author now treats of a much larger magnetic storm which began at 10.20 a.m. Greenwich time on August 11. This storm may be divided into three storms: one lasting from 10.20 on the 11th to 1 a.m. on the 12th; a second from 11.30 a.m. on the 12th to 7.20 a.m. on the 13th; and the third from 11.50 a.m. on the 13th to 7 to 8 a.m. on the 14th of August. The first storm began on August 11, at the same instant at all stations. There is a decided similarity, especially in the horizontal force curves, throughout the first part of this storm, and certain points in it stand out prominently. The deflections are alike at Lisbon, Kew, Vienna, St. Petersburg, and after the first very sudden deflection at Toronto also. The greatest effect is produced at St. Petersburg; the similarity between the large disturbances at Vienna and Toronto, in Canada, places differing about six and a half hours in time, is remarkable. About 11.45 p.m. and 2.40 p.m. there are very remarkable points of agreement. From about 4.30 p.m. to 8 p.m. Greenwich time, *i.e.* from about 11 a.m to 2.30 p.m. Toronto time, the deflections are opposed at Toronto and at Vienna or Kew. This would point rather to solar action as the cause of the disturbance. At 9 p.m. the disturbances are all in the same direction, but about 11 p.m., whilst St. Petersburg agrees in direction with the others in a very violent phase of the storm, at Toronto the direction of the deflections is reversed, and this reversal of curves continues until about the end of the first of the three storms. The second storm, the most remarkable of the three, began about 11.30 a.m. on the 12th, and lasted until the next morning. At Toronto the line goes off the edge of the paper on which the photographic record is taken. At Vienna and Melbourne the motion is so rapid that the plate is not sensitive enough to receive the impressions. At 12.20 midday, the time of greatest disturbance at Lisbon and at Zi-ka-Wei near Shanghai in China, two places nine hours different and nearly in the same latitude, the vertical force is increased in precisely the same fashion. At St. Petersburg the change in the horizontal force was one thirty-fifth part of the whole horizontal force, and the total force was changed to about one-eightieth part of its full value. These magnetic changes are so large as to be quite comparable, as we see, with the earth's total force, so that any cause which is shown to be incompetent from the nature of things to produce the one can hardly be held to account for the other.

The number of mathematicians who attended the meeting was very remarkable, and among the foreigners present may be mentioned Messrs. Halphen, Chemin, Rudolf Sturm, Cyparissos Stephanos, and W. Woolsey Johnson (Annapolis, U.S.). A separate mathematical department was formed, which met on three days, and more than thirty papers on pure mathematical subjects were read, many of them being of great interest. Prof. Halphen made a communication on Steiner's theorem relative to the positions of the centres of conics passing through three given points, and gave an elegant extension of the theorem to distinguish the cases in which the three points lay on the same or opposite branches of the curve. He also made communications on the subject of linear differential equations and hypergeometrical series; and in a fourth paper he considered the number of aspects in which points in a plane may be viewed. He showed that two points may be thus viewed in six ways, that four points can be viewed in nine ways, and illustrated this by a diagram, and extended the theorem to five points Prof. Sturm communicated an elaborate memoir on curves of double curvature, relating to the researches of Cayley and Halphen, which was ordered to be printed *in extenso* among the reports. M. Stephanos read several papers, in one of which he showed that the different homographies which exist upon a straight line, and which are triply infinite in number, may be identified with the points of space. A simple and beautiful representation of the particulars of these systems was thus obtained.

The other papers included communications by Prof. Cayley, *On the Transformation of Elliptic Functions, and on Abel's Theorem*; by Prof. H. J. S. Smith, *On the Differential Equations satisfied by the Modular Equations, and on the Theory of the Multiplier in the Transformation of Elliptic Functions*; by Mr. J. W. L. Glaisher, *On the q-Series in Elliptic Functions*; by Dr. Hirst, *On Consequences of the Second Order and Second Class*; and by Prof. R. S. Ball, *On the Application of non-Euclidean Space to a Problem in Kinematics, and an Extension of the Theory of Screws to the Dynamics of any Material System.*

SECTION B—Chemical Science

The Present State of Chemical Nomenclature, by Prof. A. W. Williamson, Ph.D., F.R.S.—The author stated there were perhaps few departments of science in which such definite principles had been adopted, and to a great extent this applied to the

formation of names, as in their own science of chemistry. The practice of stating in a name as briefly as possible certain facts, and as a rule important facts, had been, as every chemist knew, the chief object of their nomenclature. But he thought he might be permitted to say that if one looked to the composition of any result like the present nomenclature of chemistry—which had been guided by intellectual principles—it was of immense importance to consider its purely intellectual principles, viz., the principles of convenience, and perhaps even of popular tastes, and, if he might be allowed to imagine such a thing, even the prejudices which occasionally arose among a great number of men who adopted any particular form of expression. He proposed to refer to the question from the different points of view. If they had occasion to consider, without knowing anything about it, what was the most important condition to which every name ought to conform, he fancied there would be no two opinions on the matter. The first and most important condition and requirement of every name of a thing that was important was that it should call to the minds of those who used it, without ambiguity, some one particular thing or one particular idea. He should be inclined to consider a code of laws by which their action would be rendered uniform with regard to names, and which would establish such fundamental principles that an absence of ambiguity would be secured. The more any name could be defined and shortened the better it would be for chemistry. In the modern progress of chemistry, especially in that department of which the growth had been enormously great—he meant the many carbon compounds—the purpose of obtaining clearness and avoiding ambiguity in the nomenclature had been, with few exceptions, satisfactorily attained. But he thought members would agree with him that in the names given to some compounds more complex than others the chief object of convenience had not been attained to an equal extent. They found names given which, when carefully considered by chemists, told a story, but a very long story, and in a manner which was really free from ambiguity, but only by aid of a great number of syllables, and a compound word of inconvenient length was this attained. On the other hand, amongst very common substances that systematic process had been, he thought, to a considerably less degree adopted. The elder names of commoner substances, such as salts, were to a great extent based upon facts which were true, but were by no means the only facts to be recalled. Of course every chemist knew the great number of names that were in common use, and how far they served to recall a particular process, but only one among many processes by which the substance could be formed. On the other hand, many names had grown up from bodies which were purely empirical—names which did not recall any particular properties, but served with great convenience and without ambiguity to indicate the body. If they looked to the circumstances which affected that one condition which he had submitted as essential to names being perfectly free from ambiguity, there was perhaps hardly one condition more practically important than this, that there should be in the names as little change as possible, and more especially was this the case when a name that had once been given had come to be used in relation to particular substances. It was within the memory of chemists that changes of name had taken place not only when a particular substance was recalled, but there were also a considerable number of cases showing that the name given at one time to one body was afterwards given to another. The circumstances attending such changes were in some instances of an exceedingly reasonable kind, and well worthy of consideration after it was found that there were grounds for believing that the names belonged more properly to other substances. If, however, changes introduced confusion, they were necessarily injurious to the progress of the science. When he looked back to the successive steps by which their knowledge had risen to its present position, and to the ideas that had succeeded one another, he felt that in order to really understand chemistry, and to be able to arrange the facts in a convenient order, they must see how they had grown up. If that was important in practical matters, it was even more important in what he might call the scientific work. He ventured to think, at all events he had always felt, that to use with safety any idea that they were accustomed to use, it was almost essential, and was certainly of importance, that they should endeavour to trace the origin and growth of that idea, so as to see what it really meant. His object in bringing the subject before the Section was to obtain from his colleagues and friends their views on the present state of matters, and to give them the opportunity of considering together—those who more especially felt it their duty to contribute by any means in their power to the advancement of science either in guiding the operations or growth of those names—whether there could not be greater concert among chemists as to what was being and what had been done, so that they might conform their doings to certain laws. He had frequently seen with regret some features in chemical nomenclature that had been springing up of late years. He had seen some habits gaining ground which appeared to be at variance with the best principles of nomenclature—he would assume such to be the case. But there were laws in the growth of those words, and he could not doubt for his own part that if chemists came to recognise those laws, or rid themselves of them, the future growth of words would gradually come to be a more systematic guide. It had sometimes been felt that to attempt to solve the problem would be useless, and that irregularities had become so prevalent that it would be hopeless to think they could ever remedy them. But he thought differently, and would urge that in the direction he had pointed out they were only now beginning to move. There was only one convenient division among names. That division, of course, was not absolute, because no such division could be absolute; but the great majority of names were used to denote things and ideas. Some names were of little use in relation to the particular ideas, and therefore it seemed to him that the best way to obtain a name was as the result of experiment. If founded on that principle there could be no ambiguity. At the present time, as their views had considerably changed, and as they had not attained finality in their operations, there was much to be learnt, and it was reasonable to suppose that if they adopted a particular name to indicate a particular thing it might perhaps turn out at some time hence an error upon which people would look back as historical. With regard to names, especially theories, there were some of them that had certainly served important purposes. It was then really essential to the arrangement of their ideas that they should for the time imagine something to exist, and that they should recall by some convenient name that which they assumed or imagined. Names, in his opinion, ought to express ideas; but there were many names introduced which he thought were used for no better purpose than to express the absence of ideas. It often happened that when exploring any particular part of a field, they got a rational clue which led them clearly and well for a certain way; and they failed to follow it further. The cases were numberless, but one of the most important was that of chemical combination itself. Complex bodies were far more numerous than the few simple bodies with which they had to do, and while in the habit of using the term chemical combination, they had concealed their ignorance of the state of combination. Others used the term molecular combination, and there again they concealed their ignorance of the bodies to which it was applied. Among the present anomalies in names there was one which he ventured to submit to the consideration of the Section, and which had grown up to some extent of late, and that was the replacing of empirical names of things by names, which, while he would call them rational, because they served to recall intelligibly and without ambiguity, served to recall the number of atoms. He mentioned such cases by way of illustrating the ractice which had seemed to him to be gaining ground of late years, for the purpose, as some said, of increasing the clearness of statement. He had no doubt that the words were framed for the purpose of conveying to the mind something useful to know, and as names formed on that principle had been found to be based on those superseded by others, he thought when they came to such names as indicated molecular composition it was better to avoid them, because, as he had said, they had not arrived at finality. The chemists of fifty years ago were as confident as chemists of the present day in the matter of nomenclature; and therefore the more they could obtain names without ambiguity and without liability to change in the future, the more probable was it that such names would stand and continue to be used. A crowd of material presented itself just then to his mind, but he did not think it would be well to trouble the Section with further remarks. He merely wished to throw out the ball for his colleagues to deal with.

Cellulose and Coal, by C. F. Cross, B.Sc., and E. J. Bevan.—This is a continuation of the authors' researches on bast fibre (Chem Soc. *Journ.*, 1880, abstr. 666). By the action of sulphuric acid (sp. gr. 1·65), at 70°, on jute fibre, an insoluble, black, spongy substance has been obtained; that the cellulose

of the fibre contributes to the formation of the substance, is shown by the formation of a similar compound from pure cellulose and dextrin. A chlorinated product ($C_{20}H_{16}Cl_4O_{10}$) has been obtained from this black substance, its properties are similar to those of the aromatic substance described in a previous paper (*loc. cit.*). The production of this spongy substance is usually a destructive one, and attended with an evolution of CO_2 and the production of acetic acid, &c. It is not, however, necessarily so, for when the action of the sulphuric acid is arrested before the evolution of carbonic dioxide, a reddish brown solution is obtained, from which when poured into water a copious flocculent precipitate is obtained, of a body very similar in chemical properties to the black substance described above. The chlorine substitution products are easily converted into astringent bodies, producing dark-coloured precipitates with iron salts and copious coagulation with gelatine. These facts, together with the following :—(*a*) Meissner and Sheppard's conclusion that the hippuric acid of herbivorous urine is derived from an aromatic body present in the fodder, apparently a form of cellulose, which the authors have identified as similar to the characteristic consitituent of bast fibre ; (*b*) the previous demonstration by the authors of the homogeneous nature of jute fibre, and that in its resolution the percentage yield of cellulose may be increased apparently at the expense of the aromatic constituent ; (*c*) that the process of liquification (or the formation of tannin-like substances) is said by microscopists to be due to an intrinsic modification of the substances of the cell-walls, *i.e.*, of the cellulose, and not to an infiltration of the substances present in the cell cavity ; (*d*) the numerous cases in which tannic acid is formed at the expense of plant structures of the nature of cellulose—lead the authors to conclude that, until the contrary is proved, lignin must be regarded as derived from cellulose by chemical modification. The spongy black substance, previously described, dries to a hard mass resembling cannel coal, with which the authors have compared it, and have obtained similar products of chlorination and nitration, and further support of the opinion that coal is not carbonaceous in any more special sense than alcohol, but is rather, as supposed by Balzer, composed of C, O, H, N bodies, which are genetically, if not homologously related. The authors suggest that cellulose, lignite, peat, lignin, and anthracite are terms of an infinite series differentiated under the conditions of their formation.

Hydration of Salts and Acids, by C. F. Cross, B.Sc.—The method adopted by the author for investigating the rate of hydration of a substance consisted in exposing about 1 gramme of the substance in a bell-jar of 2000 c.c. capacity, to an atmosphere saturated with aqueous vapour. After a critical investigation of the probable errors, the "Jolly" Federwaage was used to make the numerous weighings required, and thus the method of observation was rendered very expeditious. The paper contains diagrams representing the velocities of hydration for certain salts and oxides. The author has observed that, under these "artificial" conditions of exposure, all the soluble salts examined deliquesce. This takes place in some cases without previous hydration, *e.g.*, with potassium bichromate, and in such cases the water may be removed by pressure between blotting paper. In other cases, *e.g.*, with $CuSO_4$, the salt deliquesces after uniting with water of chemical hydration, and in a different manner. It would therefore appear that the continuity of the phenomena of hydration and solution, as regards the determining cause, is demonstrated by these observations.

On Colliery Explosions, by W. Galloway.—The author gave an account of his experiments made to show the influence of coal-dust in colliery explosions. In July, 1878, he made three sets of experiments with different kinds of apparatus. In the first set, in which coal-gas was used instead of fire-damp, and the gas and air were carefully measured, and then coal-dust added, it was shown that 2 per cent. of gas, mixed with air, was rendered inflammable when coal-dust was added ; 3 per cent. of gas made this mixture slightly explosive ; 4 per cent. made it still more explosive ; and 5 per cent. produced a violent explosion. The total quantity of gas and air mixture was little more than a cubic foot. In the second set it was shown that the return air of a mine containing 2 per cent. of fire-damp became inflammable when coal-dust was added to it. In the third set the explosion of a mixture of air and fire-damp was made to raise and ignite coal-dust scattered along the floor of an artificial gallery 70 or 80 feet long, and 14 inches square inside. The flame of the fire-damp explosion alone was found to be 7 feet or 8 feet long ; the flame of coal-dust in pure air was 35 feet or 40 feet long ; and the flame of coal-dust in the return air employed in the first set of experiments was 80 or 90 feet long. The publication of these results called further attention to the subject, and after the Seaham explosion the Home Secretary requested Dr. Abel to inquire, amongst other things, into the influence of coal-dust in promoting that disaster. Prof. Abel made experiments near Wigan, and obtained results similar in kind to the author's, but different in some respects. In July of the present year the author made experiments with apparatus of the following description : A sheet-iron cylinder 6 feet long by 2 feet in diameter, closed at one end and open at the other, had its open end bolted to a wooden gallery 126 feet long by 2 feet square inside. One end of the wooden gallery was thus closed by the sheet iron cylinder, an explosion chamber, and the other end was open. Six sheets of newspaper were placed between this open end of the explosion chamber and this gallery, and a tight joint was ensured by means of screws. Rather less than 2 cubic feet of fire-damp was carefully measured and introduced into the explosion chamber. The wooden gallery contained only pure air. The air and fire-damp contained in the explosion chamber was thoroughly mixed by means of an appropriate mechanical arrangement, and the mixture was exploded. The explosion burst the sheets of paper, and the resulting flame travelled about 12 feet or 14 feet along the gallery, and as suddenly disappeared. The gallery was then strewed with a layer of the coal-dust from ¼ inch to ½ inch thick along its floor, and some was placed on shelves which stood in sets of three, one above the other, at distances of 10 feet from each other, along the gallery. The same arrangement as before was then made in regard to preparing for a fire-damp explosion, exactly the same quantity of fire-damp being measured, mixed, and exploded. By this explosion of the fire-damp mixture the coal-dust was raised in a cloud throughout the whole length of the gallery, part of it was projected out into the air to a distance of 20 feet or 30 feet beyond the end, and, after the lapse of an appreciable interval of time, the flame found its way to the end of the gallery and flashed out through the cloud of dust to a greater or less distance according to circumstances. The greatest length of flame thus obtained with coal-dust and pure air was 147 feet on one occasion, and from 100 feet to 140 feet very often. He considered that these results proved in the most convincing manner that coal-dust formed an inflammable mixture with pure air, and they settled once for all the question as to how an explosion in one district of a dry and dusty mine could penetrate to the most distant parts of every other district of the workings in the same mine. In conclusion the author spoke of the necessity of keeping the floors of mines damp, and thus lessening the dangerous influence of coal-dust.

SECTION C—GEOLOGY

A preliminary Account of the Working of Dowkerbottom Cave in Craven during August, 1881, by E. B. Poulton, M.A., F.G.S.—Dowkerbottom Cave is 1250 feet above the sea, between Arncliffe and Kilnsey. Its mouth is merely a fall in the roof of the cave, which stretches from either end of the fissure thus formed. The original mouth is not now visible, but is probably to be found at the foot of a slope to the south. During most of its course the chambers and passages of the cave are not separated by any great thickness of rock from the ground above, and thus other falls must be expected to occur. The eastern division of the cave is about 450 feet long, and has three fine chambers separated by two passages, the first very short, and the second very long. This division ends under high ground, and the true mouth must be in the other, or western cave. The last chamber is characterised by mechanical deposits—blocks of limestone fallen from the roof and a stiff brown clay beneath. In the other chambers and passages are chemical deposits—hard and soft stalagmite. The western division is smaller, but also contains three chambers and two passages. It must be about 250 feet long. Chemical deposits, with some falls from the roof, are present throughout. In former workings by Mr. Farrer, Mr. Denny, and Mr. Jackson, the first chambers were explored in their surface layers at least, and here were found the numerous metal and bone ornaments and implements, together with the bones of animals usually found in the historic layers (of Romano-British age) in caves. The second passages have also been worked, and part of the second chamber on the eastern side. Other parts of the cave appear to be quite untouched. The great

difficulty in working the cave is the removal of the *débris* to prevent its interfering with further work. We therefore put up a windlass over the eastern entrance and cleared a way for barrows through the talus below. Beneath the talus the black earth, in which remains had been previously found, was seen, and many articles of Roman age were taken from it. Chamber III. was marked into parallels, and these into squares. In the centre we sank a shaft and passed through the following layers:—(1) *Romano-British layer*, a black earth with pottery, ornaments, &c., and numerous bones, usually from 1 to 2 inches thick; (2) ***hardish stalagmite***, about 6 inches thick, in one place containing the bones of a dog or small wolf; (3) ***soft stalagmite*** 4 inches thick; (4) ***hardish stalagmite*** 6 inches thick; (5) ***soft stalagmite*** 2 feet 6 inches thick; (6) ***stiff brown clay*** with large angular blocks of limestone fallen from the roof firmly imbedded in it. This layer was 8 feet deep, as far as we saw it. The last two feet are laminated and contain smaller blocks. At the depth of about 12 feet from the surface we came upon part of the solid limestone floor or side of the cave, sloping steeply downwards. There were no indications of a change in the nature of the deposit at the junction with the limestone, and the clay appears to extend much deeper than the level at present reached. Thus below the stalagmite purely mechanical deposits succeed, and no limestone blocks are found above this horizon, although the stalagmite has been removed over a large part of the floor of the chamber. No traces of a fauna have been as yet found below the first hardish stalagmite; indeed all the deposits passed through below the stalagmite indicate the former presence of a still lake in which the great thickness of clay slowly accumulated. Further work was stopped by the heavy rain which flooded the shaft dug in the clay. It is interesting to note that the former condition of Chamber II. is identical with the present state of the third chamber in the preponderance of mechanical over chemical deposits. The change from mechanical to chemical deposits was probably produced by a change from accumulation in still water to accumulation in running water. Possibly also the absence of blocks fallen from the roof in the stalagmite may be due to the bicarbonate of lime contained in the water which percolated through the roof, cementing together the limestone blocks. The absence of this cement when the clay was deposited may be due to the absence of solvent power in the water which then percolated through the roof. For the carbon dioxide would not be evolved from a soil deficient in organic matter, as the soil covering the Yorkshire hills for a period long after the Glacial period must have been. The author expresses his best thanks to Mr. J. R. Tennant of Kildwick Hall, Leeds, and to Mr. J. R. Eddy of Carleton, Skipton, who gave, on behalf of the Duke of Devonshire, the permission to work the cave, and further aided with kind help and advice all through the work.

On Asteromilia Readi, a New Species of Coral from the Oligocene of Brockenhurst, by Prof. P. Martin Duncan, F.R.S.—The author described the characters of this coral, placing it in the genera established to include certain corals from the West Indies, and some dredged up in the Carribean Sea by Count Pourtalès. He referred to the genus Madrepora, which lives in twenty to twenty-five fathoms, 74° Fahr. temperature, reef-building coral, or on banks in a turbulent sea. The specimens are generally rolled, but some are absolutely perfect, and clearly give the history of the physical conditions of the close of the Eocene period in the south of England, which then resembled the climate of the Bermudas.

On the Formation of Coal, by E. Wethered, F.G.S., F.C.S.—The author considers (1) that coal was not formed from vegetation of the Lepidodendroid type, and that therefore the Stigmaria found in the underclays are not the roots of the vegetation which gave rise to the coal; (2) that the varieties of coal and the change which sometimes takes place in one and the same seam are not due to metamorphism, nor are they dependent upon the contorted state of the surrounding strata, but arise from the greater or less chemical decomposition of the vegetable mass, influenced by the circumstances under which it was submerged. On the land grew the vegetation of the period, represented by the Lepidodendrons, Sigillaria, Calamites, &c. As the land sank and the waters encroached, the land vegetation was gradually washed away, but the roots remained in many cases, and those which offered the greatest resistance to decay are the ones preserved in a fossil state—hence the occurrence of Stigmaria. As the waters advanced the ground would become swampy, and then we might expect to see spring up reeds, mosses, and other vegetation suitable to the changed condition; it is to vegetation of this kind that the author ascribes the formation of coal. With a view of ascertaining whether the chemical composition of the beds which overlie a seam of coal which has changed from bituminous to anthracite also changed, the Welsh "nine-feet" seam was selected, which near Cardiff is semi-bituminous, and at Aberdare becomes anthracite. Specimens of the overlying strata were selected from the two districts at each foot above the coal for five feet; these were analysed, and it was found that the beds from near Cardiff were considerably more argillaceous and, as a whole, less ferruginous than those at Aberdare.

On the Palæozoic Rocks of North Devon and West Somerset, by W. A. E. Ussher, F.G.S., Geological Survey of England and Wales.—The classification adopted is as follows:—

LOWER DEVONIAN	FORELAND GRITS	Red and purplish grits, fine-grained, and in places siliceous.
	LYNTON BEDS	Grey, even-bedded, and jointed grits, grey schists, and schistose grits with films of calcareous matter.
MIDDLE DEVONIAN	HANGMAN GRITS	Coarse white quartzose, red-speckled grit, in and upon red and grey rather fine-grained grits associated with shaly and slaty beds.
	ILFRACOMBE SLATES PASSING INTO MORTE SLATES	Grey and silvery slates and shales with arenaceous films, and impersistent bands of limestone passing into pale greenish unfossiliferous quartzose slates.
UPPER DEVONIAN	PICKWELL DOWN BEDS	Indian-red slates upon red, green, and grey grits, with local purple slate basement-beds passing into the Morte slates.
	BAGGY BEDS	Green slates with *Lingula*; brown micaceous grits with *Cucullæa*, positions of these horizons apparently reversed near Wiveliscombe.
	PILTON BEDS	Bluish and greenish grey argillaceous slates, with occasional thin films of limestone and masses of grit (as at Braunton, &c.).

The Foreland Grits occupy an area (superficial) of thirty square miles, extending from Countesbury to Dunster. The Hangman Grits form the range which includes Dunkery Beacon, also the whole northern part of the Quantocks. Their relations to the Ilfracombe Slates are much complicated by faults around Croydon Hill and on the Quantocks; and the prevalence of grits in the Ilfracombe series, whilst indicative of lithological assimilation, makes the boundary rather indefinite.

On the Characters of the "Lansdown Encrinite" (*Millericrinus Pratii*, Gray, sp.), by P. Herbert Carpenter, M.A.—The "Lansdown Encrinite" is a species of *Millericrinus* (*M. Pratii*, Gray, sp. = *Apiocrinus obconicus*, Goldfuss) from the Great Oolite on the top of Lansdown, near Bath. It is remarkable for the very great variation in the characters of its stem and calyx. The former may reach 50 mm. in length, and consist of seventy discoidal joints; or there may be less than ten joints, the lowest of which is rounded off below, and its central canal closed up. Various intermediate conditions may occur between these two extremes, while in some specimens there may be only two to four stem-joints; and in one case the whole stem is represented by a slightly convex imperforate plate on which the basels rest. This specimen, taken by itself, would be naturally regarded as a *Comatula* of advanced age, in which the cirrhus-sockets had disappeared from the centre dorsal just as they do in the recent *Actinometra Jukesii*. The general appearance of the calyx is very similar to that of *Pentacrinus Wyville-Thomsoni* from the North Atlantic. But it is remarkable for the number of small intercalated pieces which it may contain. The basels are frequently separated from one another, or from the radials, by minute plates which, while regularly developed all round the calyx in some specimens, are entirely absent in others. The nearest allies of *M. Pratii* seem to be *M. Munsterianus*, var.

Buchianus and *M. Nodotianus*. It stands on the extreme limit of the genus, connecting it with *Pentacrinus* on the one hand, and with the free *Comatulidæ* on the other. It is thus a synthetic type, as would naturally be expected from its geological position; for it is probably the earliest known species of the genus, except perhaps for two doubtful Liassic forms, which are known only by isolated plates and stem-joints.

Observations on the two Types of Cambrian Beds of the British Isles (the Caledonian and Hiberno-Cambrian), and the Conditions under which they were respectively Deposited, by Prof. Edward Hull, LL.D., F.R.S.—In this paper the author pointed out the distinctions in mineral character between the Cambrian beds of the North-West Highlands of Scotland, and their assumed representative in the east of Ireland and North Wales. In the former case, which included the beds belonging to the "Caledonian type," the formation consists of red or purple sandstones and conglomerates; in the latter, which included the beds belonging to the "Hiberno-Cambrian type," the formation consists of hard green and purple grits and slates, contrasting strongly with the former in structure and appearance. These differences, the author considered, were due to deposition in distinct basins, lying on either side of an archæan ridge of crystalline rocks, which ranged probably from Scandinavia through the central Highlands of Scotland, and included the north and west of Ireland, with the counties of Donegal, Derry, Mayo, Sligo, and Galway, in all of which the Cambrian beds were absent, so that the Lower Silurian repose directly and unconformably on the crystalline rocks of Laurentian age. As additional evidence of the existence of this old ridge, the author showed that when the Lower Silurian beds were in course of formation the archæan floor along the west of Scotland must have sloped upwards towards the east, but he agreed with Prof. Ramsay that the crystalline rocks of the Outer Hebrides formed the western limit of the Cambrian area of deposition, and that the basin was in the form of an inland lake. On the other hand, looking at the fossil evidence both of the Irish and Welsh Cambrian beds, he was of opinion that the beds of this basin were in the main, if not altogether, of marine origin, and that the basin itself had a greatly wider range eastward and southward, the old archæan ridge of the British Isles forming but a small portion of the original margin.

On a Discovery of Fossil Fishes in the New Red Sandstone of Nottingham, by E. Wilson, F.G.S.—The author called the attention of the Section to a recent discovery of fossil fishes in the Lower Keuper Sandstone of England—a circumstance of sufficient rarity in itself, apart from any palæontological results, to deserve at least a passing notice. During the construction of the Leen Valley Outfall Sewer in 1878, a remarkably interesting section was given by the tunnelling driven through Rough Hill, or Colwick Wood, near Nottingham, showing the lower beds of the waterstones resting on a denuded surface of the "Basement Beds" of the Keuper. The lowest stratum of the waterstone was a sandstone about a foot thick, with streaks of red and green marl, and a seam of pebbles at the base. The fishes occurred in this bed, and chiefly in a thin seam of red marl overlying the pebbly seam at the very bottom of the Waterstones; they were present in large numbers, as if in a shoal, for a distance, in the line of section, of about thirty-three feet. The specimens obtained have been examined by several competent authorities, but unfortunately their state of preservation is so bad that nothing certain can be made out as to their precise zoological affinities. Dr. Traquair, however, believes that they probably belong to some species, new or old, of the genus *Semionotus*.

Glacial Sections at York, and their Relation to the later Deposits, by J. Edmund Clark, B.A., B.Sc., F.G.S.—The York area chiefly consists of glacial beds, which form the high ground and various extensive low tracts more or less remote from the Ouse. Glacial depressions have been filled up with brick-earths, and, in exceptional cases, peat-beds. Where the river channel is narrowed below the city, the crests of the banks are capped with gravels. The peat-beds of Campleshon Pond and part of St. Paul's Square rest on the levels covered with brick-earth. Near Ouse Bridge a peat-bed 50 feet down, at Bratt's Brewery, has been called interglacial; but the beds above it cannot positively be asserted to be glacial; for at the waterworks similar beds appear, in which plant-roots were detected 20 feet down. The following sequence of the beds can be established:—

Brick earths.—At the Harrogate Signals, a quarter of a mile further north, the junction of the upper beds with glacial (or probably glacial) beds is seen. At a few points bosses of boulder clay protrude even here through the upper beds, whilst elsewhere depressions are filled with brick-clays, now extensively worked.

Gravels.—The gravel beds at Fulford and on the opposite side of the Ouse are much alike. The beds are irregular, roughly stratified, with boulders of a quarter-ton weight. The stones are precisely the same as those in the boulder clay; some limestone boulders are still striated. At the gravel pits now being worked on the Bishopthorpe Road a metatarsal of *Ursus spelæus* (or *U. arctos*) was found this spring. There seems to be no previous record of any carnivorous remains from this neighbourhood.

Glacial Sections.—The *deepest* glacial sections were some made in drainage-work at the Friends' Retreat, in 1876, a drift, 650 feet long, cutting through the hill from north-west by west to south-east by east. At the highest point this was 47 feet below the surface. Shafts were sunk every 50 feet. Nothing but glacial beds were met, tough boulder clays, gravelly beds, and sand beds. The latter were variously inclined and much cut up, rarely continuing any great distance. Indeed everything pointed to the whole mass being made up of independent parts, heaped and piled against each other. The largest boulder brought up weighed about 600 pounds, which is as much as any seen near York *in situ*, except, possibly, one still to be seen on the Mount. Some of those in the museum grounds must weigh more. Among other stones two lumps of coal were brought up. The most extensive series of sections are those on the site of the New Goods Station. For this a level was obtained four acres or so in extent, and 3 to 12 feet below the old surface. Unfortunately there are no records of the sections made in this part. The stones found, though including many from the Lake District, chiefly come from the Carboniferous beds of the West Riding. Limestones are usually scratched and often beautifully polished. At all the places mentioned occasional specimens occur from Lias and Oolite beds, so that an easterly drift must have sometimes counteracted the prevailing set from the west. These glacial beds approach nearest to the purple boulder clay of Messrs. Searles V. Wood and Harmer. Floating ice, however, rather than the *moraine profonde* of an ice sheet, seems best to account for the mixture of tough boulder-clays with beds of boulders, gravels, and current-bedded sands. The post-glacial deposits are worked to depths of 30 feet and more; in the river-bed they may exceed 50 feet. The river is now 60 or 70 feet above its pre-glacial bed, and probably 40 or 50 above the level to which it first cut down in the opening of the post-glacial epoch.

The Devono-Silurian Formation, by Prof. E. Hull, LL.D., F.R.S., &c.—The beds which the author proposed to group under the above designation are found at various parts of the British Isles, and to a slight extent on the Continent. The formation is, however, eminently British, and occurs under various local names, of which the following are the principal:—

England and Wales

Devonshire.—"The Foreland Grits and Slates," lying below the Lower Devonian beds ("Lynton Beds").

Welsh Borders.—"The passage beds" of Murchison, above the Upper Ludlow Bone bed, and including the Downton Sandstone, and rocks of the Ridge of the Trichrug. These beds form the connecting link between the Estuarine Devonian beds of Hereford (generally, but erroneously, called the "Old Red Sandstone") and the Upper Silurian Series.

South-East of England (Sub-Cretaceous district).—The author assumed, from the borings at Ware, Turnford, and Tottenham Court Road, described by Mr. Etheridge, that the Devono-Silurian beds lie concealed between Turnford and Tottenham Court Road on the south, and Hertford on the north.

Ireland

South.—"The Dingle Beds," or "Glengariff Grits and Slates," lying conformably on the Upper Silurian beds, as seen in the coast of the Dingle promontory, and overlaid unconformably by either Old Red Sandstone, or Lower Carboniferous beds, 10,000 to 12,000 feet in thickness.

North.—"The Fintona Beds," occupying large tracts of Londonderry, Monaghan, and Tyrone, resting unconformably on the Lower Silurian beds of Pomeroy, and overlaid unconformably by the Old Red Sandstone, or Lower Carboniferous beds, 5000 to 6000 feet in thickness.

SCOTLAND

South.—Beds of the so-called "Lower Old Red Sandstone" with fish and crustaceans, included in Prof. Geikie's "Lake Orcadie, Lake Caledonia, and Lake Cheviot," underlying unconformably the Old Red Sandstone and Lower Calciferous Sandstone, and resting unconformably on Older Crystalline rocks. Thickness in Caithness about 16,200 feet. The author considered that all these beds were representative of one another in time, deposited under lacustrine or estuarine conditions, and, as their name indicated, forming a great group intermediate between the Silurian, on the one hand, and the Devonian on the other. He also submitted that their importance, as indicated by their great development in Ireland and Scotland, entitled them to a distinctive name, such as that proposed.

On the Discovery of Coal-Measures under New Red Sandstone, and on the so-called Permian Rocks of St. Helen's, Lancashire, by A. Strahan, M.A., F.G.S., Geological Survey of England and Wales.—The Trias has been penetrated, during the last few years, by three colliery shafts and three boreholes in the district bordering the St. Helen's and Wigan coal-fields on the south. It was thinner than might have been expected, while the Permian formation was altogether absent. This latter formation was believed to underlie the Trias, but to be overlapped, so as not to appear at the surface, excepting at St. Helen's Junction, where a marl-bed, and a soft sandstone beneath it, 30 and 90 feet thick respectively, and supposed by Messrs. Binney and Hull to be Permian marl and Lower Permian sandstone, were found in a quarry and a well. The Bold Hall Colliery shaft, at about one mile from the outcrop of supposed Permian rocks, proved the shale to maintain its thickness, but the sandstone to be 57 feet 9 inches only. The Coal-Measures were entered at 186 feet, and penetrated to a depth of 1800 feet from the surface, when the Florida Mine was met with. The red staining due to the Trias extended to a depth of 365 feet in the Coal-Measures. The Collins Green Colliery shafts, at the same distance from the boundary of the Trias, but three-quarters of a mile north-east of Bold Hall Colliery, proved the shale to be 22 feet, and the sandstone 44 feet in thickness. The latter contained spherical concretions of iron pyrites, binding the grains of sand in their original position in plains of bedding. The Coal Measures were entered at 310 feet 10 inches, and penetrated to the Florida Mine at 1667 feet 7 inches from surface. They were red for 152 feet. The dip of the so-called Permian was to the south-east at 6°, that of the Coal-Measures at 10°. The Haydock Colliery shafts (Lyme Pits), at the same distance from the boundary of the Trias, are one mile north-east of Collins Green. The shale and sandstone had diminished here to 9 feet and 7½ feet respectively. The Coal-Measures were penetrated to a depth of 97 feet 2 inches, or 413 feet 3 inches from surface. In the shafts of this and the Collins Green Colliery, the unconformity of the red sandstone and the Coal-Measures was clearly visible. The above sections show that the so-called Permian marl and sandstone thin out gradually from west to east, the lower thinning out first, and not the upper, as would have been the case if they had been unconformably overlapped by the overlying beds. They also thin out to the south, as proved by a borehole near Farnworth, three miles south of St. Helen's Junction, which, after penetrating 124 feet of yellow and white sandstone, passed through 3 feet of red and white clay, 3 feet of red sandstone, and entered purple marls with bands of limestone, belonging to the Coal-Measures. The so called Permian beds, though unconformable to the Coal-Measures, are quite conformable to the Trias, and are overlapped in consequence of an attenuation in themselves, and not through having suffered denudation before the Trias was deposited upon them. Considering also their lithological similarity to the Trias, it seems that they should be classed with this formation rather than with the Permian. The Permian rocks are probably absent west of Warrington, for two boreholes at Parkside and Winwick, commencing in the Pebble beds, entered the Coal-Measures at 291 and 341 feet respectively without encountering them. The Trias contained a bed of shale about 30 feet thick, and was based by soft sandstone with twig-shaped concretions of iron pyrites. Like the spherical nodules of Collins Green, these probably owed their origin to the action of Coal-Measure water, with sulphides in solution, acting on the colouring matter (peroxide of iron) of the Trias. The Coal-Measures consisted of purple and green marls, and at Winwick were associated with limestone. They, and the same beds found in the Farnworth boring, are precisely similar to the well-known Whiston limestone, and like it contain the *Microconchus carbonarius*. These limestones are probably the equivalents of the Ardwick limestone series in the Upper Coal-Measures of Manchester,[1] and may be found to be underlain by representatives of the coal-seams which are found in connection with it. Without doubt they must be everywhere underlain by the whole of the productive Middle Coal-Measures, but at a great and unknown depth, though there is reason to believe that the thickness of barren measures would be less in West Lancashire.

Remarks upon the Structure and Classification of the Blastoidea, by P. Herbert Carpenter, M.A.—The author and Mr. R. Etheridge, jun., who are preparing a joint memoir upon the Blastoidea, have arrived at the following conclusions respecting the group:—It is very doubtful whether the genus *Pentremites* occurs at all in Britain. Some badly-preserved fragments from the Devonian and the Scotch Carboniferous are possibly referable to it; but most of the Blastoids (besides *Codaster*) which occur in the Carboniferous Limestone belong to the genus *Granatocrinus*, Troost., which is represented by some seven or eight species. Cumberland's *Mitra elliptica* is the representative of a new genus, distinguished by the eccentric position of the spiracles. *Codaster* is a true Blastoid, and not a Cystid, as supposed by Billings. The slit-like openings of its hydrospires are nearly on the same level as the ambulacra, which do not conceal them at all. In the ordinary Blastoids, however, they are below and concealed by the ambulacra, opening externally by pores at the sides of the latter. There are various intermediate forms between these two extremes, in which the hydrospiral slits are more or less concealed by the ambulacra, but are partially visible at their sides. It is proposed to group the species thus distinguished into a genus *Pentremitidea*, which is represented in Britain by the little *Pentremites acutus*, Sowerby, in Belgium by *P. caryophyllatus*, and in Spain by *P. Pailleti*, De Verneuil, for which last the name *Pentremitidea* had been already proposed by D'Orbigny. An arrangement of this kind has been already suggested by Billings. The discoveries of Rofe, Wachsmuth, and Hambach, respecting the perforation of the lancet-piece by a longitudinal canal, are confirmed. This canal probably lodged the water-vessel, which must have been devoid of any tentacular extensions, as in some Holothurians, and in the arms of certain *Comatulæ*. Respiration was effected, however, by means of the hydrospires. The pores usually found at the sides of the ambulacra were not the sockets for the attachment of the appendages, but led downwards into the hydrospires, serving to introduce water, which made its way out through the spiracles. The genital ducts probably opened into some portion of the hydrospires, as they do into the closely similar structures of the *Ophiuroidea*, and the ova were discharged through the spiracles. Billings' statements are confirmed respecting the existence in many species of a single or possibly double row of jointed appendages along each side of the ambulacra; but these appendages are not homologous with the pinnules of the *Crinoidea*. In perfect specimens the peristome is covered in by a vault of small polygonal plates, any definite arrangement of which is rarely traceable. Extensions of this vault were continued down the sides of the ambulacral grooves, which could thus be closed in completely and converted into tunnels, as in recent Crinoids. The classification of the Blastoidea must depend entirely upon morphological principles. Mere differences in the relative sizes of the calyx plates are of very little systematic value; and differences in the numbers of side plates on given lengths of the ambulacra are absolutely worthless. On the other hand, the structure and relative positions of the hydrospires and spiracles are morphological characters of much systematic value.

On the Extension into Essex, Middlesex, and other Inland Counties, of the Mundesley and Westleton Beds, in Relation to the Age of certain Hill-gravels, and of some of the Valleys of the South of England, by J. Prestwich, M.A., F.R.S., Professor of Geology in the University of Oxford.—The author gives in this paper the result of observations commenced more than thirty years since, but delayed publication in consequence of doubts caused by the complexity of the phenomena. As mentioned in the preceding paper, a peculiar group of land, freshwater, and marine beds occupy, on the Norfolk coast, a zone between the Chillesford Clay and the Lower Boulder Clay. As we proceed southward, the land and freshwater conditions are gradually eliminated, and marine conditions then alone prevail. Poorly

[1] This identification was pointed out by Mr. De Rance in the *Trans. Manchester Geol. Soc.* for 1880. ("Further Notes of Triassic Borings near Warrington.")

marked as the marine evidence is in Suffolk, this evidence is entirely wanting further inland, and we have only levels, superposition, and structure to rely on in correlating the fragmentary outliers into which these beds finally resolve themselves. Again on the coast of the Eastern Counties, this group forms a nearly level plain but little above the sea-level, resting everywhere on an undisturbed or very slightly eroded bed of Chillesford Clay, and being succeeded, with but slight evidence of denudation, by the Lower Boulder Clay, or by the Glacial sands and gravel; whereas, as it trends inland, it attains a considerable elevation above the sea-level, passes unconformably over the older Tertiary strata, and has been subjected to a great amount of denndation. On the other hand, the old land, which seems to have extended from the eastward as far as the Norfolk coast, is now in great part below the level of the German Ocean. Further, whereas the succeeding Glacial beds all show a drift from northward to southward, this is the only case that has come under the author's notice of a marine drift from southward to the northward. The Westleton Beds, in their more typical aspect, consist of quartzose sands full of flint pebbles, almost as much worn and as numerous as in the Lower Tertiary sands of Addington. The author then proceeds to trace the beds through Essex, and gives a series of railway sections showing these beds, exhibiting usually the appearance of a white gravel, with intercalated ochreous beds, and reposing on a very eroded surface of the London Clay. In traversing the beds farther westward they undergo further modification. Certain characters remain, however, persistent, and on these we have to rely: (1) The shingle is composed essentially of chalk flint pebbles, becoming less worn as we approach the southern limits of the deposit; (2) it often becomes much mixed with flint pebbles and sub-angular fragments of compact sandstone derived from the underlying Tertiary strata; (3) the chert and ragstone fragments often so increase in numbers as to constitute a large portion of the gravel. They are worn and sub-angular, and the chert is identical with the chert of the Lower Greensand of Kent and Surrey; (4) the pebbles of white and rose-coloured quartz, of Lydian stone, and of white quartzite become rarer, and in places are wanting. The Lydian stone and some of the small quartz pebbles may be derived, with the chert, from the Lower Greensand, but this will not account for the great number of quartz pebbles found in the Eastern Counties. The quartzite pebbles are equally large, but lighter coloured and more ovoid than those of the New Red. They probably have drifted from a continental area on the east, the author having found similar beds in parts of Belgium; (5) the absence of northern drift. The author reserves for another occasion the description of the beds next in order; but he would mention here that the Boulder Clay and some Glacial gravels occupy in Herts and Berks a lower horizon than the Westleton Beds. It would therefore appear that, while the eastern area was submerged, and the strata followed in regular succession upon a surface which did not undergo denudation, the southern and western area was slowly elevated, and underwent partial denudation before the Upper Boulder Clay was deposited. Previous to the period of the Westleton and Mundesley beds, it is probable that the denudation of the Weald had hardly commenced. The area was spread over by Cretaceous strata under water at the beginning of the Crag period (the Lenham beds), and judging from the character of the beds which fringe the North Wealden area at Chelsfield, Cherry Down, &c., the author concludes that there was land south of this fringing shingle, whence the great mass of Chalk-flints and of Lower-Greensand cherts and ragstone must have been derived. This mass of *débris* serves to attest to the great extent of these strata that have been removed from the Wealden area while yet it was an elevated and not a depressed area. After the rise of the area over which the Westleton Beds extended, it underwent extensive denudation, and it was at this period that the great plain of the Thames Valley received its first outlines, although it was not until much later that the river valley received its last impress.

A Contribution to Seismology, by Prof. J. Milne and T. Gray, B.Sc.—It was pointed out that earthquake motion is generally of a very irregular character, that it usually begins gradually, reaches a maximum somewhat suddenly, and afterwards passes through several minima and maxima. The period of vibration of a great number of earthquakes observed by the authors varied between half and one-fifth of a second, while the total time of disturbance varied from one to three minutes. Reasons were given for believing that earthquakes which last for a long time are propagated further than those which last for a short time, even when the intensity of the latter is the greater. As to the determination of the origin of shock, the great value of accurate time observations was pointed out, and a sketch of different modes of making such observations was given. Explanations were entered into with regard to the rotation of bodies during earthquake shocks.

The Glacial Geology of Central Wales, by Walter Keeping, M.A.—The author adduces evidences to show that Central Wales was covered with snow and ice during the glacial period, but all the glaciers of which we have any traces were of strictly local character, each confined to its own drainage area in the present valley system. There is no evidence of any great *mer de glace*, nor of any marine submergence in recent geological times.

On the Lower Keuper Sandstone of Cheshire, by A. Strahan, M.A., F.G.S., Geological Survey of England and Wales.—This paper deals with some of the results of the re-survey of parts of Cheshire, which have been already described in detail in the Geological Survey Memoirs "On the Neighbourhood of Prescot" (third edition), and "On the Neighbourhood of Chester." Several sections, of which the best are at Runcorn and Frodsham, show that there is a strong and constant division between the waterstones and the Keuper Basement Beds. These were formerly classed together under the name of Lower Keuper Sandstone, but, so far as the re-survey has been carried, are now distinguished on the maps. The old and new classifications may be compared as follows:—

Old Classification.	New Classification.
Keuper Marl	Keuper Marl.
Lower Keuper Sandstone ...	Waterstones.
	Lower Keuper Sandstone or Basement Beds.

SECTION D—Biology

Department of Anatomy and Physiology

On the Conario-hypophysial Tract, or the Pineal and Pituitary Glands, by Prof. Owen, C.B., F.R.S.—The author, referring to the latest contributions to the subject of his paper, remarked that they bore upon the functions of the so-termed "glands." Prof. Sapolini, in his work "L'Aire de la Selle Turcique" (8vo, 1880), concludes that "the pituitary gland secretes the fluid of the ventricles of the brain." Prof. Ed. Van Beneden, in reference to the supposed pituitary gland in Ascidians, regards it as their renal secretory organ (*Archives de Biologie*, 8vo, 1881). In pursuance of his aim, which was homological, Prof. Owen traced the modifications of the pineal and pituitary bodies and connecting parts from man down to the lowest fishes possessing a brain; and noted the progressively increased relative size and retention of tubular structure of the tract, including the so-called "pituitary gland," "infundibulum," "third ventricle," and "pineal gland," as the vertebrate series descended; also the further extension of the pineal part of the tract, beyond the brain, to its perforation of the cranium, leaving the so-called "foramen parietale" in some existing and in many extinct Reptilia. These phenomena were then tested and compared with concomitant phases in the development of the vertebrate, especially the mammalian, embryo. It was shown, as had been noted by previous embryologists, that prior to the permanent anterior outlet of the digestive sac, a production from such sac extended to the large cerebral vesicle, subsequently reduced to a "third ventricle"; whence the hollow tract was continued onward to the epithelial covering of the head, by which it was closed. The lower pharyngeal beginning of this trans-cerebral tract also became closed and modified as the "pituitary body." The upper continuation became modified, and in higher vertebrates closed as the "pineal body"; but the intermediate portion of the tract retained its primitive hollow condition as the "third ventricle" and "infundibulum." The "sella turcica" in mammals, like the "foramen parietale" in cold-blooded vertebrates, were modifications in the skeleton of parts of the "conario-hypophysial tract." This tract, under all its modifications, marked vertically the division between the "cerebrum" and the "optic lobes," or divided the "fore-brain" from the "hind-brain."

The author next proceeded to point out the homologies of the parts of the neural axis in invertebrates with those of vertebrates.

The so called "supra-œsophageal ganglion or ganglions" in the former were homologous with the "cerebrum, or cerebral hemispheres" in the latter. The so-called "sub-œsophageal masses" in invertebrates answered to the mes-and ep-encephalic

masses in vertebrates. The neural chords and ganglions continued therefrom backwards in invertebrates, answered to, or were homologous with, the myelon or spinal chord of vertebrates, in which the ganglionic structure was more or less concealed, save in some fishes, by superadded neural substances.

Now the supra-œsophageal mass, or "fore-brain," in invertebrates is divided from the sub-œsophageal masses, or "hind-brain," by the production of a tubular portion of the fore part of the primarily closed alimentary cavity, which, extending between those parts of the neural axis, opens upon the surface of the head so attained, and there establishes the permanent mouth; the tubular extension therefrom similarly retains its functional or œsophageal relations with the alimentary cavity. The neural chords, connecting the so-separated "fore-brain" with the "hind-brain," traversed the sides of this gullet; as the chords or "crura," proceeding to expand into the "fore-brain" of vertebrates, traverse the sides or walls of that persistent part of the conario-hypophyseal tract known in anthropotomy as the third ventricle. The large relative size of the embryonal brain-vesicle in this connection is significative of the homology of the parts extending therefrom.

Passing next to the consideration of the characters which had been held to determine the "back" and "belly" of the animal, the author cited:—"Colour," the "relative position of the body of air-breathers to the ground they stood or moved upon,"[1] and the criterion, which Cuvier adopted to determine these aspects in the notable controversy with Geoffroy St. Hilaire in 1830.[2] That criterion was the cerebrum in vertebrates, and its homologue, the super-œsophageal ganglion, in invertebrates. In an enlarged copy of the diagram by which Cuvier illustrated his position, the author pointed out the grounds on which the great French comparative anatomist exclusively applied the term brain (*cerveau*) to this part of the cerebral centres; moreover, Cuvier expressly rejects the homology of the spinal cord of vertebrates with the ganglionic chord of the body in invertebrates; and he concluded that, however his opponent might turn about his articulate or molluscous subject, the so-called brain would be on opposite sides of the alimentary canal in the two groups compared.

Now, to reconcile this difference, the author pointed out that it only needs to add to Cuvier's diagram of the brain of the mammal the conario-hypophysial tract omitted in that diagram; and, if the facts and deductions in his paper were allowed to be valid, the actual difference would lie in the atrophy of the embryonal homologue of the invertebrate gullet and mouth in vertebrates, and the establishment in them of a new entry into the alimentary cavity.

In the vertebrate embryo this anterior entry makes its first appearance as a capacious branchial or water-breathing organ, and traces of this destination are determinable in the higher vertebrates, in which the respiratory function is ultimately otherwise located and performed in relation to an aërial medium.

The entry to the alimentary cavity in Amphioxus is both a breathing and a feeding mouth: it is a vertical or longitudinal slit bounded by a pair of styles, in which is made the nearest approach to gristle of any part of the sclerous system in that primitive vertebrate. This "mouth" seems to be, or to be formed by, a confluent pair of the branchial openings, such as those which follow after it. To what pair of the costal, hæmal, or vertical side-walls or supports of the higher piscine vertebrate oral cavity, scapular, hyoid, tympano-mandibular, or palato-maxillary ribs, the parial styles of Amphioxus may be homologous, it is hard to say in the absence of skull or brain in that animal. In fishes the double function of the mouth is retained—all are "branchiostomous." In air-breathers the vertical entry becomes exclusively respiratory, and is more or less divided from the alimentary mouth beneath, and the opening or inlet to this becomes transverse by the production of the tympano-mandibular arch and its apposition to the palato-maxillary one above. In ancient forms of vertebrate air-breathers the entry to the narial passage, or respiratory mouth, as it may be termed, is by a pair of openings homologous with a piscine pair of branchial ones, but admitting air instead of water. To these "antorbital nostrils," as they are termed, in Plesio- and Ichthyosaurs, a more anterior single or confluent pair of inlets is added in Teleosaurs. In recent crocodiles the latter becomes exclusively the single, undivided, or partially divided breathing-mouth. In lizards and birds it is commonly divided, or there is a pair of "nostrils." In mammals the nostrils are commonly approximate. But the "feeding-mouth" remains below them as a distinct transverse cleft. In all these modifications the aperture, whether for breathing or feeding, or for both, is on the hæmal aspect of the brain; the vertebrates are hæmastomes; the invertebrates are neurostomes, and the chief part of their brain is "hæmad" of their mouth.

Returning to the criterion of the dorsal and ventral aspects of the animal body, the author maintained that the ganglionic body-chord in invertebrates did answer to the myelon of vertebrates; and adding this to the totality of the brain, the so-called "neural axis" was determined. So determined, he held that its position was the true criterion of the dorsal or neural aspect of the body, whether the animal moved with it next to, or farthest from, the ground, or neither the one nor the other, as in the human pedestrian.

The part or aspect of the body opposite the neural one was characterised by the location of the centre, or chief centres, of the vascular system, and this had led Prof. Owen, at the commencement of his anatomical teaching, to term it the "hæmal aspect."

Referring, finally, to the diagram of the invertebrate and vertebrate animals in corresponding positions, agreeably with the above criterion, the author showed that the so-called "brain" (Cuvier), or the supra-œsophageal brain-mass of comparative anatomy, was not above, but below, the mouth or gullet in invertebrates, and that the sub-œsophageal mass was above the mouth or gullet; also that the reverse relative positions were due to the atrophy of the primitive homologues of such entry in vertebrates, and the substitution of another opening or conduit to the stomach, whereby these anterior openings and conduits are on the lower or hæmal side of the cerebrum in vertebrates, on the upper or neural side of the cerebrum or fore-brain in invertebrates. In briefer terms, the one division was "hæmastomous"; the other division was "neurostomous." The paper was illustrated by drawings, of which enlarged diagrams were exhibited to the Section.

Dr. Montagu Lubbock's paper *On the Development of the Colour Sense* discussed the question of the evidence as to the acquirement of the power of perceiving colour by man within historical times, and also the question whether this perception had been gradually acquired by man or any animal at any time. He concluded that there were good grounds against believing that any such gradual development in the case of man could be proved; and while it was probable that in those animals which lived upon coloured food the power of appreciating colour would gradually arise, yet there was no proof of this yet available, and no idea could be given of the stages by which this had been brought about.

Prof. S. P. Thompson read a paper upon the *Function of the two Ears in the Perception of Space*, in which he stated his view as follows:—Judgments as to the direction of sounds are based in general upon the sensations of different intensity in the two ears; but the perceived difference of intensity upon which a judgment is based is not usually the difference in intensity of the lowest or fundamental tone of the compound sound, or "clang," but the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result on the quality of the sound. Prof. Thompson further remarked that now that the physical bases of the problem were laid down, the acoustic perception of space might be greatly elucidated by experiments upon persons possessed of abnormal hearing, and upon the blind, in whom this perception is abnormally developed.

Prof. J. C. Ewart of Aberdeen gave an account of the researches *On the Influence of Bacilli on the Production of Disease*, which he has communicated to the Royal Society.

Mr. W. A. Forbes read a paper *On the Incubation of the Indian Python* (*Python molurus*), *with special regard to the alleged Increase of Temperature during that Period.* This paper gave an account of a large series of observations made during the last season in the gardens of the Zoological Society. The python laid about twenty eggs, and incubated for about six weeks. Observations were made upon both male and female, kept in adjoining cages under conditions approximately identical, and it was found that there is an increase of temperature in the incubating female analogous to that which occurs in birds; the amount of increase observed was not so great as others had

[1] The anatomists who adopt this criterion call the hæmal aspect of the lobster its "back," the neural one its "belly"; the right side of the animal is its left side, and *vice versâ*.

[2] Reference was here made to the nineteenth vol. of the *Annales des Sciences Naturelles* for 1830 (March), p. 241, Pl. XII.

tated, being about 1·9° and 3° Fahrenheit, according as the temperature was taken on the surface of the body or between its folds.

Dr. D. J. Cunningham's paper *On the Structure and Homologies of the Suspensory Ligament of the Fetlock in the Horse, Ass, Ox, Sheep, and Camel*, described the particular members of the intrinsic group of muscles which enter into the formation of this ligament. He showed further that the process of transformation of muscle to ligament seemed to be effected by a fatty degeneration of the muscle-fibres with a coincident multiplication of the connective-tissue elements of the muscle; that muscular tissue may exist in the body and have no apparent function, unless it were a purposeless contraction, stimulated by the nerve-supply it received from nerves contained within the ligament. In the transformation the nerves remained unchanged; in the sheep, in which there is not a trace of muscular tissue left, the nerves were relatively as large as in the ox or horse.

Other papers were read, by Prof. Struthers, *On the Acetabulum of Animals in which the Ligamentum teres is described as wanting*, and *On the Correspondence between the Articulations of the Metacarpal and Metatarsal Bones in Man*; by Mr. F. M. Balfour, *On the Nature of the Pronephros, or so-called Head-Kidney of Adult Teleosteans and Ganoids*; by Mr. G. E. Dobson, *On the Digastric Muscle, its Modifications and Function*; and Dr. W. H. Stone, *On the Effect of the Voltaic Current on the Elimination of Sugar*. Altogether fourteen papers were read before this department, which only sat on two days. Half the papers were anatomical, and half physiological. It is to be assumed that the energies of anatomists and physiologists had been so largely occupied with the International Medical Congress that no novelties could be produced on this occasion.

Department of Anthropology

Miss A. W. Buckland, in a paper *On the Geographical Distribution of Mankind*, discussed the problems awaiting solution in anthropology, especially the relations of brachycephalic and dolichocephalic peoples, and the questions of the unity of the race, and of the peopling of oceanic islands and of Australia. She considered that nothing definite could as yet be determined regarding any of these matters.

Mr. Staniland Wake read a paper *On the Papuans and the Polynesians*, in which he came to the conclusion that the primitive stock from which both had sprung was now represented by the Australian race, which had formerly a much wider extension than at present. The existence of two types among the Australians showed they were not a pure race, being probably intermixed with the Negrito. The Polynesians showed considerable traces of this intermixture, while the Papuans had been largely affected by contact with a more modern Asiatic people now represented by the Malays, having been further specially influenced by the intermixture of Arab and Indian blood.

General Pitt-Rivers gave an account of *Excavations in the Earthwork called Ambresbury Bank in Epping Forest*, which showed that it was a camp of British erection, but it was not possible from the excavations made to determine whether it was made before or after the Roman conquest. General Pitt-Rivers read another paper *On the Entrenchments of the Yorkshire Wolds and the Excavations in the Earthwork called Danes' Dyke at Flamborough*, in which he showed that the term Danes' Dyke was undoubtedly a misnomer, for the whole district was the scene of the operations of a much earlier people, who were formidable in their means of offence and defence, and in the discipline necessary to construct the entrenchments, which extended for great distances. At Danes' Dyke he found both flints and flint flakes, showing that the defenders of the earthwork used flint, and lived not later than the bronze period, at the period of the tumuli of the Yorkshire wolds. In a further communication General Pitt-Rivers described his discovery of flint implements in stratified gravel in the Nile Valley, near Thebes.

Dr. Beddoe gave an interesting abstract of results *On the Stature of the Inhabitants of Hungary*, based on recruiting statistics. The average Hungarian soldier was about 5 feet 5¾ inches high. The Germans and Croats gave taller men than the Magyars. The citizens of Budapesth were taller than countrymen at the age of twenty. In five western counties (including Pesth), where the population was mainly Magyar, the mean stature at twenty-five years might be taken as 5 feet 5·2 inches.

A paper *On the Physical Characters and Proportions of the Zulus*, read by Mr. Bloxam, gave the details of an examination of sixteen male and three female Zulus brought to this country, and measured in the presence of Prof. Flower, General Pitt-Rivers, Mr. Roberts, and Mr. F. Galton. It appeared that the average stature of the males was 67·3 inches, one-third of an inch less than the average Englishman of the same age. The average chest girth was 36·5 inches; Englishman's, 35·25 inches; average weight: Zulu, 151 lbs.; Englishman, 141 lbs. Of course the Zulus, being exhibited for their dancing and spear-throwing accomplishments, were in high training, and very well developed in muscle.

Mr. E. F. im Thurn, in a paper *On the Animism of the Indians of British Guiana*, dwelt at some length on the confusion introduced by the application to animism of the terminology and conceptions of higher religious systems. The Indians of Guiana had an animism of a very pure and primitive kind, very little affected by the modifications which change animism into higher religion. They had no belief in the everlasting duration of the spirit, no ideas corresponding to heaven, hell, and retribution, no knowledge of purely spiritual beings, *i.e.* gods, and no worship, though certain arts were practised to avoid attracting the attention of malignant beings.

Mr. Park Harrison, in exhibiting a collection of photographs of types of different races in the British Islands and in France, urged the necessity, for the purposes of scientific comparison, of having photographs taken of uniform size, both in full face, and sufficiently in profile to show the brow, the projection of the nasal bone, and also the form of the ear, which appears to be a racial characteristic, though much disguised by mixture of blood. This, however, would be attended with expense greater than the Anthropometric Committee could afford. Prof. Flower, in commenting on this communication, said the subject had scarcely yet been fairly attacked in this country; it was only by the photographing of numbers in each part of England that they might ultimately have a chance of arriving at the types of the principal races that had contributed to the mixtures now prevailing. There was great difficulty in forming an opinion as to what types people really represented; no doubt the comparison of photographs, done on a certain scale, would be of much value in this matter.

The Anthropological Department sat on five days, and thirty-seven papers or reports were presented to it. Among others that we may particularise as of interest were those by Mr. J. R. Mortimer, *On Six Ancient Dwellings found near to British Barrows on the Yorkshire Wolds*; Mr. Francis Galton, *On the Application of Composite Portraiture to Anthropological Purposes*; Mr. J. Harris Stone, *On the Viking Ship discovered at Sandefjord, Norway, in 1880*; Mr. Hyde Clarke, *On the Early Colonisation of Cyprus and Attica, and its Relation to Babylonia*; Mr. H. Stopes, *On Traces of Man in the Crag*; Prof. T. McK. Hughes and Mr. A. W. Wynn, *On the Age of the Deposits in the Caves of Cefn, near St. Asaph, with special reference to the Date of Man's first Appearance in them.*

Department of Zoology and Botany

Sir John Lubbock's paper *On the Sense of Colour in Animals* first dealt with Bonnier's experiments on bees, and showed many fallacies in them, which were avoided in a series of his own observations recently made. He took slips of glass of the size generally used for microscopic work, and pasted on them slips of paper coloured blue, green, orange, red, white and yellow, and induced a bee to visit all in succession when covered by a plain slip on which was a drop of honey. Then the honeyed slips were removed, and the situation of the coloured glasses was changed; when the bee returned from the hive the order of its visits to particular colours was noted, and the result of 100 different experiments was that blue was the bee's favourite colour, then white, yellow, and green. The observations were varied in several different ways, with the same results. The question naturally arose, How then are there so few blue flowers? Sir John believed that all flowers were originally green, and that they have passed through stages in which they were white or yellow, while many have become red, and finally blue. This was supported by facts such as the following:—In Ranunculaceæ many simple open flowers, as buttercups, were yellow or white; while the blue delphiniums and aconites were of highly specialised form, and therefore probably of more recent origin. Among the Caryophyllaceæ the red and purplish species were among those with highly specialised flowers, while the simple flowers, as stellaria and cerastium, were mostly white. Among violets many of the most highly specialised forms were blue; the simpler ones yellow. In gentians, again, the deep-blue

species have long tubular flowers specially adapted to bees and butterflies, while the yellow gentian has a simple open flower with exposed honey. Sir John also described his experiments made on daphnias by illuminating a trough with an extended solar spectrum in such a way that after a given lapse of time he could isolate the portion of the trough illuminated by each principal colour, and count the number of daphnias in it. They appeared to have a very predominant preference for the red and yellow and greenish yellow and green. He also found, contrary to the conclusion of M. Paul Bert, that they clearly perceive the ultra violet rays.

Sir John Lubbock read a paper *On the Mode in which the Seed of Stipa buries itself in the Ground.*—One of the most interesting parts in botany, he said, was the consideration of the reasons which led to the different forms, colours, and structures of seeds; and it was, he thought, pretty well made out that a large proportion of those might be accounted for either as serving to protect the seed or to assist in its conveyance to a place suitable for its growth. If the seeds of trees fell directly to the ground it was obvious that very few of them would have a chance of growing. It was an advantage to them, therefore, of which many availed themselves, to throw out wings, in consequence of which the wind wafted them to a greater or less distance. Others, such as the whole tribe of nuts, being edible, were carried about by beasts and birds, and though some were sacrificed, others survived. Fruits, again, in consequence of their sweetness, were carried about by animals, which, after partaking of the fleshy portion, dropped the seeds themselves. Many seeds were covered with hooks, and thus, adhering to the wool of sheep and other animals, were carried to greater or less distances. Others, like those of our common dandelion, were provided with fairy parachutes, and were thus borne away by the wind. Others again, like some of the violets, geraniums, vetches, brooms, cucumbers, cardamine, oxalis, and others, had beautiful and varied contrivances, by which they actually threw the seeds to a distance, in some cases of more than 20 feet. Others, again, were enabled to penetrate the earth, and thus sow themselves in the ground. In one of our English clovers, *Trifolium subterraneum*, after the flower had faded, it turned downwards, and buried itself in the ground. The ground nut of the West Indies, and more than one species of vetch, had a similar habit. In the *Erodiums* or Crane-bills, the fruit is a capsule, which opens elastically, and as in the case of the allied geraniums, sometimes threw seeds to some little distance. The seeds themselves were spindle-shaped, hairy, and produced into a twisted awn. The number of turns on the awn depended upon the amount of moisture. Mr. Rowe, to whom they were indebted for an account of their mechanism and mode of action, said if a seed be laid upon the ground, it remained quiet as long as it was dry, but so soon as it was moistened the outer side of the awn contracted, and the hairs surrounding the seed moved outwards, the result of which was to raise the seed into an upright position. The awn then gradually unrolled, consequently elongating itself upwards, with the result that if it was entangled amongst any of the surrounding herbage, the seed was forced into the ground. A still more remarkable case was that of the *Stipa pennata*. The actual seed was small, with a sharp point, and with stiff short hairs pointing backwards. The upper end of the seed was continued into a fine twisted rod; then came a plain cylindrical portion attached at an angle to the corkscrew, and ending in a long and beautiful feather—the whole being about a foot in length. That end was supposed by Mr. Francis Darwin, to whom they were indebted for a very interesting memoir on the subject, to act very much in the same manner as that of *Erodium*, already mentioned. He did not doubt that the end would bury itself in the manner described by Mr. Darwin, but he doubted whether it always did so. One fine day, not long ago, he chanced to be looking at a plant of that species, and around it were several seeds more or less firmly buried in the ground. There was a little wind blowing at the time, and it struck him that the long feather awn was admirably adapted to catch the wind, while on the other hand it seemed almost too delicate to drive the seed into the ground in the manner described by Darwin. He therefore took a seed and placed it upright on the turf. The day was perfectly fine, and there could therefore be no question of hydroscopic action. Nevertheless, when he returned after a few hours, he found that the seed had buried itself some little distance in the ground. He repeated the observation several times, always with the same result; thus convincing himself that one method, at any rate, by which seeds bury themselves is by taking advantage of the action of the wind, and that the twisted position of the awn, by its corkscrew-like movement, facilitates the entry of the seed into the ground.

Mr. A. W. Bennett read a paper *On the Constancy of Insects in Visiting Flowers*. He said he was not aware that attempts had yet been made to determine the question whether insects were altogether discriminating in their visits to flowers, or whether on the same journey they confined themselves exclusively or chiefly to one species. That paper, which was the result of observations during the fine weather of the last two years, was intended as a contribution towards the settlement of that question, obviously one of some importance in relation to the cross fertilisation of flowers by insects. Those who had not made the experiment would hardly appreciate how difficult it was to watch continuously for any considerable period the flight of any insect. He had chosen in all cases as points of observation spots where a considerable number of different flowers grew in profusion, and were intermixed, so that the insect would have abundant opportunity of changing its diet if so disposed. In recording the number of flowers of the same kind visited by an insect in the same flight, he always meant flowers at such a distance from one another that the insect had to use its wings in getting from one to another. In August of last year he observed three different flights of the "painted lady" butterfly, and it settled six, three, and ten times respectively, always confining itself to the same species of flower. On the same plot a hive-bee paid nine successive visits to the same species of flower. On another plot a bumble-bee visited the same species of flower fifteen times, and another of the same species eleven times in succession, not touching any other flower, but passing over many. Mr. Bennett gave further results of his observations on different occasions and in different parts of the country. In order to test whether insects were guided by colour only when visiting flowers, he watched one spot where there were white and purple foxgloves, but a large bumble-bee was seen to enter sixteen of the flowers regardless of colour, although to find the succession of foxgloves it had to fly considerable distances over other flowers. No general statement could be made as to the consistency of insects in visiting the same species of flower during the same flight. A decided preference for successive visits to the same flower was unquestionably shown in many instances, but those visits did not depend on the colour of the flower only. The hive-bee appeared to be by far the most constant in that respect, often absolutely so. From their strong and rapid flight and extremely hairy covering of their abdomen, that class of insects was probably the most efficient agent in the dissemination of pollen. So far as could be gathered from observation, the "painted lady" and the small tortoise-shell butterflies were very consistent, while the whites, the blues, and the browns were far from catholic, or less discriminative in their tastes. It was open to question, however, whether more than a very few flowers were dependent upon butterflies for their fertilisation. At all events their visits to flowers were often only interludes in their settlements on grass, leaves, the stems of trees, or the bare ground.

Prof. O. C. Marsh of Harvard, U.S., contributed one of the most attractive papers to this department, *On Jurassic Birds and their Allies*. He detailed the results of his examination of the Archæopteryx in the British Museum, the more recently discovered specimen at Berlin, and of Compsognathus in the Munich Museum, as compared with the forms previously made known by himself in America. His impression was that the two specimens of Archæopteryx were specifically identical, although fuller evidence might prove them to be distinct. He still considered that we knew little that could determine how or at what period birds originated. At present the four oldest known birds were as distinct from one another as any birds of the present day. Yet if he were asked to distinguish between the bones of a reptile such as Compsognathus and a bird such as Archæopteryx, if broken up and mixed together, he should be puzzled to do it. Prof. H. G. Seeley, in the subsequent discussion, stated his belief that the British Museum Archæopteryx was not merely specifically, but generically distinct from that at Berlin.

Dr. A. A. W. Hubrecht of Leyden gave an interesting exposition of *The Structure and Affinities of Proneomenia*, one of the valuable finds of the *Challenger* Expedition. Dr. Hubrecht spoke in excellent English, and was listened to with much appreciation.

Mr. Forbes gave an account of his work *On the Anatomy and*

Classification of the Petrels, based upon those collected by the *Challenger* Expedition. He divided them into two main families—the Occanitidæ or Oceanic Petrels, with four genera and seven species, and the Procellariidæ, divisible into three sub-families of albatrosses, diving petrels, and true petrels. As to descent, he considered the petrels were probably much modified descendants of some ancient form related to the ciconiform birds of Garrod, *i.e.*, the storks, American vultures, and their allies. Mr. P. H. Carpenter, M.A., read papers *On the various Larval Forms of Comatula*, and also *On the Species of British Comatula*. Other zoological papers of interest were by Prof. Busk *On the Use of the Chitinous Appendages of the Skeleton in the Cheilostomatous Polyzoa in the Diagnosis of Species*; Mr. W. T. Blanford, F.R.S., *On our Present Knowledge of the Fauna Inhabiting British India and its Dependencies*; Mr. P. A. Geddes, *Notes on Chlamydomyxa*, and *On a New Sub-Class of Infusorians*; Gen. Sir J. E. Alexander, *On the Improvement of Freshwater Fisheries*, and a further report was made *On the Marine Zoology of South Devon*.

Among other botanical communications we may note those of Mr. J. G. Baker, F.R.S., *On the Botany of Madagascar*; of Mr. A. W. Bennett *On the Colours of Spring Flowers*; of Mr. Joseph Lucas *On some Vestiges of the Ancient Forest of Part of the Penine Chain*. The department sat during five days, and twenty-eight communications were disposed of, including twenty zoological and eight botanical; the latter, however, fully divided the interest with the former, owing mainly to the papers of Sir John Lubbock and Mr. Bennett.

NOTES

DR. RUDOLPH KÖNIG of Paris, whose acoustical fame is world-wide, is about to publish in one volume, in the French language, his remarkable researches in acoustics, which have appeared at intervals in the *Annalen der Physik* and elsewhere, during the past fifteen years. The work will, we understand, be liberally illustrated with drawings of the newer and more important pieces of apparatus which Dr. König has invented.

M. PASTEUR, it is stated, has resolved to visit the Bordeaux lazaretto to study yellow fever, and ascertain whether it is due to a parasite, and can be guarded against by inoculation.

THE building of the Observatory of the Pic du Midi has been completed on the very top of the mountain, at an altitude of 2600 metres. The old building, which was placed in a valley at a less elevated situation, will be used merely as a station for travellers. General Nansouty is now busy fitting the establishment with apparatus and victuals for next winter, as, according to every probability, it will be blocked by snow during more than six months. The storms are so heavy that not less than six electric light conductors have been established for protection.

THE autumn meeting of the Iron and Steel Institute will be held in London this year, on October 11–14, at the Institute of Civil Engineers, under the presidency of Sir Henry Bessemer, F.R.S. Numerous excursions have been arranged for, and the following papers are announced to be read:—On the manufacture of steel and steel rails in the United States (supplementary paper), by Capt. W. R. Jones, Pittsburg, Pa.; on a method of securing homogeneity in the Bessemer process, by Mr. W. D. Allen; on the manufacture of ordnance at Woolwich, by Col. Maitland; on the application of wrought iron and steel to the manufacture of gun carriages, by Mr. H. Butter; on the manufacture of projectiles, by Mr. J. Davidson; on the distribution of elements in steel ingots, by Mr. G. J. Snelus; on the use of brown coal in the blast furnace, by Prof. P. Ritter von Tünner, Leoben, Austria; on certain physical tests and properties of steel, by Mr. Edward Richards; on the tin-plate manufacture, by Mr. Trubshaw; on the use of American anthracite in the blast furnace, by Mr. J. Hartman, Philadelphia; on variation of elements in cast-steel ingots, by Mr. F. Stubbs; and on the recent progress of the basic Bessemer process, by Herr Paul Kupelweiser, director of the Witkowitz Works, Austria.

GREAT preparations are being made in Dublin for the forthcoming meeting of the Social Science Congress, which begins its sittings there on the evening of Monday, October 3, when Lord O'Hagan, as president, will deliver the inaugural address in the Exhibition Palace. Among the other addresses to be given are the following:—"On Education," by Sir Patrick J. Keenan, K.C.M.G., C.B.; "On Health," by Dr. Cameron, M.P.; "On Economy and Trade," by Mr. Goldwin Smith; and "On Art," by Lord Powerscourt. During the week garden parties and *conversazioni* will be given by some of the leading citizens and learned societies.

BARON MIKLUHO MACLAY, before leaving Sydney, gave to the Linnean Society of New South Wales on July 25 a short account of the progress of the Sydney Biological Station at Watson's Bay, which has been opened through his energies, and of which we recently gave some account. The building was to be ready in a week's time, Dr. Maclay stated. The Royal Society of Victoria have agreed to assist the establishment of the station, not only by personal subscription, but also by an annual grant from the funds of the Society. This last decision is most important, opening the prospect of a permanent, if moderate, subsidy for the support of the institution. The Royal Society of New South Wales will also probably, on the representation of the President at the last annual meeting, follow a similar course. "I entertain the hope," Dr. Maclay said, "that the establishment of the Biological Station of Sydney will very probably induce the other colonies to follow this good example, and will be the means of uniting the scientific societies of different colonies. That the Biological Station of Sydney will not remain long isolated in this part of the world is a fact, as Dr. Hector told me that he intended to establish one in New Zealand. The establishment of an Intercolonial Biological Association, which should have for its object to assist in the formation, maintenance, and regulation of biological stations in Australia, was a plan which, in my opinion, ought not to remain long a *pium desiderium* only. Therefore I called a public meeting, June 15, with the object—1. To obtain a number of yearly contributors, as the subsidy from the Government is in proportion to the public subscription, and the yearly subsidies from the Royal Society of New South Wales and Victoria are very moderate. 2. To frame rules for the station. From the gentlemen present at the meeting a committee was chosen for the discussion of the proposed rules, this committee consisting of six members, of which four are at the same time trustees of the Biological Station; after four meetings, agreed to a code of rules, which will be submitted to the trustees of the Biological Station." Certainly science in Australia is greatly indebted to the intelligent energy of the Russian naturalist, and we trust the work so well begun will be continued without abatement.

THE Epping Forest and County of Essex Naturalists' Field Club's annual Cryptogamic meeting is advertised for Saturday, October 1. The Club is to be congratulated for the list of well-known botanists who appear as referees and conductors. Thus for Fungi we see the names of Dr. M. C. Cooke, M.A., F.L.S., Mr. Worthington Smith, F.L.S., Dr. H. T. Wharton, M.A., F.L.S., and Mr. James English; whilst for Mosses and Lichens the names of Dr. Braithwaite, F.L.S., and Mr. E. M. Holmes F.L.S., are announced.

THE Yorkshire Naturalists' Union will have a Fungus Foray on Friday and Saturday, September 30 and October 1, at which they will gladly welcome any mycologists who may be disposed to assist them. The Friday's programme is to consist of an excursion in the neighbourhood of Harrogate. On the Saturday is to be a "show," at which will be exhibited fungi, and any objects illustrative of the subject which may be sent. The dinner is to be on the evening of Saturday. Arrangements are being made to search localities in all parts of Yorkshire for specimens to exhibit; and at the meetings the Union will be

honoured by the presence of Messrs. W. Phillips, C. B. Plowright, G. Massee, and Rev. J. E. Vize.

No less than ten observers are now engaged at the Observatory of Paris in the completion of the catalogue of stars which was begun by Leverrier. The work is progressing at an unprecedented rate, not less than 70,000 observations having been tabulated, after having been duly reduced in a single year. Admiral Mouchez has taken possession of the new Observatory grounds, and the earthworks for the foundation of the great refractor building, and the construction of the underground chambers in which the magnetic observations are to be conducted, is being continued.

During the York session of the British Association a most successful half-yearly meeting of the members of the Natural History Society of the Friends' School in Bootham was held in the lecture-room of that establishment. Among those present were Prof. S. P. Thompson, F R.A.S., J. G. Baker, F.R.S., A. W. Bennett, F.L.S., J. Edmund Clark, F.G.S., Thomas Gough, M.A. (of Elmfield College), Rev. T. A. Preston, M.A. (Science Master of Marlborough College), Dr. W. W. Newbould, Langley Kitching, Ed. Grubb, M.A., Hugh Richardson, R. M. Christy, A. J. Wigham, with J. F. Fryer, B.A. (the present head master), Fielden Thorp, B.A. (the former superintendent), who presided, and many others. Dr. D. Hack Tuke delivered an interesting address strongly advocating the study of science. Mr. Baker of Kew said that a large measure of his success in life was due to the early scientific training he had received when a member of this society. Many other interesting addresses were given by those present. The Society is only three years younger than the British Association itself, having been formed on August 14, 1834. Since that time many ardent naturalists, now well known to science, had passed through its ranks.

A correspondent from Kingussie, in Inverness-shire, writes: "We had just (Sunday, 18th) been reading somewhat sceptically the paragraph about the pink rainbow, when behold, to our astonishment, there appeared just over Glen Feshy the most lovely pink rainbow you can imagine, shaded from crimson to pale pink, but no other colour. It was strange and beautiful, and none of us had ever seen anything like it before."

The just-issued volume of the *Proceedings* of the Natural History Society at Berne (Nos. 979-1003) contains, besides minutes of proceedings and small notes, several valuable papers: by Prof. Studer, on the segmentation of Madreporaceæ, on the corals of Singapore, and on the statistical researches as to the colour of eyes and hair of children in the canton of Berne; by Dr. Graf, on the specific heat of gases at constant volume; on glacial deposits at Berne, by M. Bachmann; on the intrusion of limestones into the crystalline rocks of the Finsteraarhorn, on the dependence of organisms upon oxygen, and on the influence of poisons on invertebrata, by Dr. Arnold; and several anatomical notes by Prof. Luchsinger.

We have received the *Proceedings* of the sixty-third annual meeting of the Swiss Society of Naturalists, which was held in September last year at Brieg. They contain the address of the president, M. Wolf, and minutes of proceedings of the sections, among which we notice communications:—by Prof. Rütimeyer, on the metamorphoses of skulls; by Prof. Yung, on his physiological researches on cephalopods at the Naples Zoological Station; by M. Lory, on geological researches on the Finsteraarhorn; and by M. de la Harpe, on the nummulitic formation in Switzerland. In the Reports of Commissions we notice the report, by Prof. Rütimeyer, on the important work, by M. Ph. Gosset, on the glacier of the Rhone, to which the Schläfli Foundation was awarded. This immense work, which is the result of six years' consecutive measurements of the positions of no less than 156 numbered and painted blocks, carefully chosen on the surface of the glacier, as well as of surveys on the scale of 1 : 5000, contains a thorough description of the glacier of the Rhone, and is accompanied by a most elaborate map of the glacier, numerous transverse and longitudinal sections, and several sheets of drawings, which show the results of the the measurements as to the motion of the glacier.

Statistical researches as to the colour of the hair and eyes of children had been made in all the cantons of Switzerland, with the exception of Berne, Geneva, and Tessino. The investigation as to the first of these cantons is now terminated, and the results of the examination of 94,221 children are published by Prof. Studer in the *Proceedings* of the Berne Society of Natural History (No. 986), and are accompanied by four coloured maps, which show graphically the results. It is seen from these researches that in the canton of Berne the dark type prevails over the fair, but that the pure types are not so numerous, especially in the central parts, as the mixed ones. The pure fair type, which makes 9 to 11 per cent. in the north-eastern parts of the canton, increases to the south (11 to 14 per cent. in the middle parts, and 15 to 20 per cent. in the Alps), and reaches its highest percentage in the secluded valley of the Saanen (28 per cent.). The dark type is most numerous in two regions—that of the western lakes and Old Rhætia (21 to 29 per cent.), whilst in the middle parts it reaches only 21 to 25 per cent., and only 16 to 20 per cent. in some secluded valleys. After having shown the distribution of mixed types, Prof. Studer considers these data in connection with history, and comes to several interesting conclusions.

Messrs. Sonnenschein and Allen have issued a second edition of Prantl's "Elementary Text-book of Botany," revised by Dr. S. H. Vines, who has made considerable alterations in the book, with the view of increasing its usefulness. The most important alteration, it is stated, is the adoption of a Classification of Flowering Plants which will be more familiar to English students than that which was followed in the first edition.

A recent speech of the Governor of Hong-kong, Sir John Pope Hennessy, contains an interesting account of the spread of vaccination amongst the Chinese in the Colony and on the neighbouring mainland. No port in the world is more liable to a visitation of small-pox, yet it never spreads there. The health-officer of the Colony also was astonished to find that nearly all the young Chinese emigrants had vaccination or inoculation marks upon their arms. He says he was often puzzled to know how this vaccination came to be apparently so perfect among the Chinese. On inquiry it turned out that the native doctors of the Tung-wa Hospital—a charitable institution supported by the voluntary contributions of Chinese—not only vaccinated their countrymen in the Colony itself, but actually sent travelling vaccinators over the adjoining provinces of China. In this way thousands of people have been vaccinated during the last four years. The lymph is supplied them by the Governor, who gets it every mail in his despatch-bag from Downing-street. Three dentists also appear in the census of the professions of the Colony. "About eighteen months ago," adds his Excellency, "I visited one, not professionally, but for the purpose of seeing the instruments he used, and I then found he had the same apparatus we find in all dentists' establishments. In fact he did work for the first-rate American dentists we have here, being fully capable of making or repairing sets of teeth. He was a gentleman of intelligence, and impressed me, I must say, as favourably as a dentist could."

The Congress of Orientalists has had a very successful meeting at Berlin. Of the International Geographical Congress and

Exhibition at Venice, we hope to give a detailed report next week. An Archæological Congress was opened at Tiflis on Tuesday; among the delegates is Prof. Virchow. The Caucasus Museum was also opened; the collections were very numerous and varied.

THE ensuing session of the Aristotelian Society for the Systematic Study of Philosophy will open on October 10, at 20, John Street, Adelphi, W.C., with an address by the president, Shadworth H. Hodgson, LL.D., and the Society will thereafter continue its historical studies, alternated with discussions of philosophical questions.

WE gladly welcome the appearance of the "Phænogamous and Vascular Cryptogamous Plants of Michigan," by Charles F. Wheeler and Erwin F. Smith (Lansing, 1881). 1559 species of flowering plants are enumerated, and 75 of horsetails, ferns, and lycopods. The arrangement followed is that of the fifth edition of Gray's Manual, and the authors promise to publish addenda from time to time.

WE have received the first part of Fr. Westhoff's "Käfer Westfalens," forming a supplement to the "Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westfalens," Jahrgang 38 (1881). It is only a List, prefaced by remarks on the district, and with list of authors, &c., but it promises to be of value on account of the thorough manner in which it appears to be worked out, and the beetle-fauna appears to be rich. Adopting the latest European Catalogue as a basis, this first part extends to the *Heteroceridæ*. No new species are described, but several apparently new varieties in the *Carabidæ* and water-beetles receive names.

UNDER the direction of the Council of the Meteorological Society, Mr. W. Marriott has issued "Hints to Meteorological Observers, with Instructions for taking Observations, and Tables for their Reduction" (Stanford). Many of our readers might be able to turn these Hints to good practical account. We have also received the first number of the *Meteorological Record*, containing the monthly results of observations made at the stations of the Meteorological Society, with remarks on the weather for the quarter ending March 31.

THE Report of the Committee of the Queenwood College Mutual Improvement Society for the end of the summer term 1881 is interesting, showing that much useful and varied work is being done by the Society.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mrs. Paterson; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Harding Cox; a Rubiginous Cat (*Felis rubiginosa*) from Ceylon, presented by Mr. Charles E. Pole Carew; a Brown Bear (*Ursus arctos*) from Russia, presented by Messrs. Morgan, Gellibrand, and Co.; two Chukar Partridges (*Caccabis chukar*) from India, presented by Col. Thos. Pierce, 16th Regt. Bombay N.I.; two Dwarf Chamæleons (*Chamæleo pumilis*) from South Africa, presented by Mr. Duncan W. B. Swaine; two Spanish Terrapins (*Clemmys leprosa*) from Spain, presented by Major Rooke; a Diamond Snake (*Morelia spilotes*) from Australia, presented by Mr. C. C. Sharratt; two Cape Crowned Cranes (*Balearica chrysopelargus*), two Wattled Cranes (*Grus carunculata*) from South Africa, deposited; a Black-faced Spider Monkey (*Ateles ater*) from South America, on approval.

PHYSICAL NOTES

DR. R. KÖNIG has just completed a new instrument—a variety of the wave-siren which we recently described—with which he proves an extremely important fact, which probably is *new* to all acousticians, namely, that the quality of a compound tone is very distinctly affected by differences of *phase* in the components. An account of these last researches will be found in the forthcoming number of *Wiedemann's Annalen*.

DR. KÖNIG describes (*Wied. Ann.*, No. 8) a way of exploring the interior of organ-pipes (especially stopped ones) while in action, without disturbing the vibrations. The pipe, with a central longitudinal slit made in the back, and a plate-glass front, with scale, is supported horizontally in a trough, so that the slit and half the back of the pipe dips in water. A thin brass tube, bent twice at a right angle, is supported on the pipe, so that one end enters the slit to about the middle of the pipe. This tube can be slid along the pipe, and is connected by a caoutchouc tube to the ear, a manometric capsule with flame. Passing through a ventral segment, one notices a quite sudden weakening of the sound, then a sudden strengthening (like the stroke of a bell). By noting such points the position of the segment can be exactly determined. Dr. König gives some results which apparently fail to accord with theory. He also describes a drum-like arrangement for exploring pipes.

AN extremely ingenious piece of electric mechanism is now being shown in the Electrical Exhibition in Paris. It is an apparatus by which any number from 1 to 999 is automatically signalled on one wire by a single movement of the operator, the figures appearing at the distant end at an opening in a box. To describe the details of this apparatus would take too long a space. It is the invention of Mr. J. Mackenzie.

M. CORNU has constructed a polarising prism made of a single film of Iceland-spar fixed with Canada balsam between two flint-glass prisms. The polarisation is far from perfect, however, and the field is very narrow, so that the instrument, though of interest from a theoretical point of view, is of little or no practical value.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—An examination will be held at Exeter College on Thursday, October 13, for the purpose of filling up a Natural Science Scholarship, tenable for four years during residence. The examination will be in biology, chemistry, and physics. Candidates will be expected to show proficiency in at least two of these subjects, and the scholar will be required to read for honours in biology in the Natural Science School. The same papers will be set in chemistry and physics as in the examination for the Natural Science Scholarship at Trinity College. Candidates are desired to call on the Rector between 6 and 7 p.m. on Wednesday, October 12. They may obtain further information by application to the Rector, or to Mr. W. L. Morgan, the Lecturer in Biology at Exeter College.

THE Prospectus of Lectures and Classes for the first Session of University College, Nottingham, promises well. There will be both day and evening lectures and classes in Language and Literature (Prof. Symes), Mathematics, Mechanics, and Physics (Prof. Fleming), Chemistry (Prof. Clowes), Natural Science (Prof. Blake).

CONTENTS

THURSDAY, SEPTEMBER 29, 1881

THE STRUGGLE OF PARTS IN THE ORGANISM

Der Kampf der Theile im Organismus: ein Beitrag zur Vervollständigung der mechanischen Zweckmässigkeitslehre. Von Dr. Wilhelm Roux, Privatdocent und Assistent am Anatomischen Institut zu Breslau. (Leipzig: Wilhelm Engelmann, 1881.)

SINCE the first dawn of methodical inquiry one of the largest and most important problems that has always been presented to scientific thought is the explanation of the endless number and complex variety of those apparently purposive adaptations of structures to functions which are everywhere to be met with in organic nature. Until within the last few years the solution of this problem was all but universally sought in the hypothesis of a designing mind, and as no other cause had been suggested as adequate to produce such a multitude of seemingly teleological effects, it became a habit of philosophical thinking to regard these effects as evidences of a creating intelligence. And although the scientific instincts of an individual here and there pointed towards the belief that in some unaccountable manner the facts were due to physical as distinguished from metaphysical causes, the scientific instincts which pointed in this direction were unable to justify themselves on grounds of reason, inasmuch as they were unable to suggest any non-mental principle which could reasonably be taken to explain a class of phenomena bearing so suggestively the appearance of a mental origin. The tide of thought in this matter therefore rose without interruption or perceptible hindrance in the direction of supernaturalism, until it attained its highest level in the "Argument from Design" as elaborated by the natural theologians of the past generation. Then with a suddenness only less surprising than its completeness the end came; the fountains of this great deep were broken up by the power of one man, and never in the history of thought has a change been effected of a comparable magnitude or importance.

But although the theory of natural selection as conceived and elaborated by Mr. Darwin so completely subverted the foundations of what may be termed a scientific teleology, it soon became apparent that natural selection alone was not adequate to explain all the facts of adaptation that are met with in organic nature. Not to enter upon the question, which we can only hope that future generations may be able to answer, as to how far natural selection alone, or unassisted by any other principle, is competent to produce changes of specific type—how far, in other words, we are to attribute the evolution of species to the uncompounded operation of the survival of the fittest, and how far to the probable operation of other and unknown factors—not to enter upon this question, it is enough to observe that many cases of adaptation which occur in the parts of individual organisms cannot possibly be explained by the theory of natural selection as this is applied to explain cases of adaptation which are presented by specific types. Thus, to take the most simple illustration, the effects of use and of disuse in increasing or diminishing the functional utility of an organ in obvious adaptation to the requirements of the individual organism—these effects clearly cannot be attributed to survival of the fittest organisms. Similarly in the morbid processes of disease there is frequently observed "an effort of nature" to throw off the affected part, or otherwise to effect a spontaneous cure. These and other considerations of the same kind have led all the more thoughtful evolutionists—including Mr. Darwin himself—to conclude that over and above the great principle of natural selection, operating from without the organism and therefore called by Mr. Herbert Spencer "indirect equilibration," there must be other principles of an adaptive character at work within the organism itself, and therefore collectively called by Mr. Spencer the principles of "direct equilibration." And it is evident that one of the most important problems now presented to evolutionists is that of ascertaining what are these principles of direct equilibration. The work before us is an interesting effort in this direction.

The idea which Dr. Roux elaborates at much length is that the principle of the struggle for existence and consequent survival of the fittest is in active operation, not only as between individuals of the same or different species, but also between the constituent parts of the same individual. As all the parts of an organism receive their nourishment from a common and limited supply, there necessarily arises among them a competition for food, so that, for instance, in any cellular structure the most vigorous cells will survive by starving out the less vigorous, just as is the case with organisms living in an area of limited food-supply. Also, and especially after the period of full growth of the organism has been attained, the mutual pressure exerted by neighbouring cells must give rise to a further competition—a struggle for room or space wherein to develop—and here again it will be the most favoured elements that will be successful in attaining a vigorous maturity. In these and in several other minor respects which we need not wait to mention, Dr. Roux maintains that all the organs, cells, and even molecular groupings of an organism are so situated as to be constantly under the evolutionary influence of the struggle for existence. If such is granted to be the case, the author proceeds to show how a foundation is supplied for explaining all or many cases of "direct equilibration," or, as he terms it, "capacity of functional adaptation." For this capacity amounts merely to an increase or diminution of the functional power of a part under the influence of an increase or diminution of stimulus, using the latter term in its most comprehensive signification as including any change of conditions acting from without. (This, at least, seems to be the sense in which Dr. Roux uses the term, as he applies it indifferently to an excitation of nerve or muscle, increase of traction upon a bone, blood-pressure in an artery, &c.) But if a stimulus means a change of conditions, it means, when frequently repeated, a change of the physiological environment of the structure affected, and therefore, if the constituent parts of this structure are subject among themselves to a keen struggle for existence, those parts which are best adapted to the change will survive, while the others will succumb, with the ultimate effect of altering the form or function of the structure so as to meet the new circumstances of stimulation.

Such in the most general terms is the doctrine advocated in "Der Kampf der Theile im Organismus." Perhaps the most striking feature in the detailed exposition which the author gives of the doctrine is his ignorance of the fact that the doctrine is not original. His work is pervaded by expressions of the importance which he attaches to his idea as that of a new light shining in a dark place, and he is surprised that in the domain of physiology the thoughts of Darwin should not have been earlier applied. But in this country, at all events, the idea is far from being a novel one. Not to mention writers of less repute, Mr. Spencer has meditated deeply upon the causes of "direct equilibration," and his works are over-charged with analogies drawn between the organism physiological and the organism social—analogies which include the struggle for existence and survival of the fittest in all their ramifications. Nevertheless, although Dr. Roux seems strangely ignorant of the philosophy of evolution as taught by Mr. Spencer, his work is of value in pursuing this branch of the subject into greater detail, and with more extensive knowledge of physiology, than has been hitherto done. The topic is a deeply interesting one, and we therefore welcome this attempt at its elucidation. We must, however, observe that Dr. Roux, in the ardour of speculation, is too prone to endow a "muss sein" with the value of an inductive verification; and we must emphatically express our dissent from him wherever he appears to insinuate that the doctrine of natural selection in the domain of physiology has evidence in its favour at all comparable with that which belongs to it in the domain of zoology and botany.

GEORGE J. ROMANES

OUR BOOK SHELF

Pflanzenphysiologie: ein Handbuch des Stoffwechsels und Kraftwechsels in der Pflanze. Von Dr. W. Pfeffer, Professor an der Universität Tübingen. Band I. "Stoffwechsel." (Leipzig: Engelmann, 1881.)

IN treating of the Physiology of Plants, Prof. Pfeffer very naturally divides his subject into two parts, the first being "Stoffwechsel," or metabolism, the second the concomitant "Kraftwechsel," that is, the conversions of latent into kinetic energy and *vice versâ* which are involved in the metabolic processes. The volume now before us treats of the "Stoffwechsel," and it does so in a very thorough and satisfactory manner. In the first place there is evidence in the work of a very complete acquaintance with the extensive literature of the subject, and further, of a critical power of recognising and bringing into prominence those observations which are worthy of being incorporated in the canon of physiological knowledge. The general treatment, too, of the subject is clear and logical, though it suffers from a fault which is not uncommon with German authors, namely this, that the main line of thought becomes here and there obscured by the cloud of detail with which it is enveloped. Still the book is a mine of information for original workers, and a trustworthy guide for advanced students. It is not too much to say that it is the best work in existence on the subject. If the second volume is as good as the first, Prof. Pfeffer will indeed have to be congratulated.

SYDNEY H. VINES

The Norwegian North Atlantic Expedition, 1876-1878. III. Zoology. (Christiania, 1881.)

PART III. of the account of the animals obtained during the above expedition is by the well-known naturalists, D. C. Danielssen and J. Koren, and treats of the group of the Gephyrea. It is illustrated by six plates and one map. Of the ten genera and the sixteen species collected during the expedition four of the genera and seven of the species prove to have been undescribed, and a new family is formed for the remarkable new genus Epithetosoma. This genus differs in many respects from any known genus of the Gephyrea; most notably so by reason of the fissured opening through which the sea water gains access to the perivisceral cavity. The analogue of this respiratory fissure is probably not to be found in the class, but the general organisation of this new form is still truly Gephyrean. Unfortunately but two examples of this interesting form were dredged up, and even these were not well preserved. They were found in sandy clay at a depth of 870 fathoms, in the cold area. In concluding the memoir the authors remark that the two groups into which the class Gephyrea is subdivided, viz. *G. inermia* and *G. armata*, can hardly be regarded as satisfactory. Of several new forms which they describe, and which by reason of their anatomical structure they refer to the second subdivision, none are furnished with the armature on which that subdivision is based. Had therefore the systematic classification been rigorously applied, these would have been referred to the first subdivision, one with which they have but little in common, compared to the striking resemblance they bear to those forms comprised in the other. A list of all the species met with and their principal synonyms are appended.

A Manual of Injurious Insects, with Methods of Prevention and Remedy for their Attacks to Food Crops, Forest Trees, and Fruits, and with Short Introduction to Entomology. By Eleanor A. Ormerod, F.M.S. Pp. 1-323. 8vo. (London: W. Sonnenschein and Allen; Edinburgh: J. Menzies and Co., 1881.)

THE authoress of this book is well known as an enthusiast in the department of Economic Entomology, and may thoroughly be congratulated upon having produced a work that cannot fail in many ways to be useful to the class of readers for whose instruction and profit it is intended. In many respects it is based upon Curtis's familiar (but somewhat obsolete) "Farm Insects," and many of the usually excellent illustrations are counterparts of those that appeared in that work; many others were originally from the faithful pencil of Prof. Westwood: in both cases the old volumes of the *Gardeners' Chronicle* have furnished contributions; a few are from other sources. As in Curtis's work the subject is dealt with according to the plants attacked, not according to the attacking insects, a plan to be much commended in such a work. In each case a short description of the insect and of its methods of attack precede the consideration of Prevention and Remedies. Naturally much is compiled from previous writers; much information given is the result of records obtained from the many willing assistants of the authoress; much is original from her own observations. It is not our duty to enter into an examination of the suggested "remedies"; we vastly prefer to look with more favour upon the means of prevention, and are glad to see that generally sound advice in the way of scientific cultivation is given throughout. Nor are the meteorological conditions overlooked: we can modify many things —we cannot rule the elements; and in very bad seasons we fear our farmers and gardeners must be content to "pocket the loss" occasioned by insect ravages on crops the constitutions of which have been already ruined by atmospheric conditions. In a few cases subjects appear to have been introduced for the sake of effect. For instance, we doubt if any farmer in the kingdom is one penny the worse for the occasional presence in his potato-fields of the larva of the Death's Head Moth; on the other hand many bee-keepers could tell a different tale from the ravages of the moth itself in their hives. The Colorado beetle, of course, has "honourable mention";

but we are rather sorry to find the authoress enthusiastic at the passing of the "Injurious Insects" Act of Parliament, which we prefer to consider the outcome of a scare furthered by speculators. All we can say for the "Introduction to Entomology" is that it will possibly serve to give the class for whom it is intended sounder ideas on the subject than generally prevail with them; the Glossary at the end is too short to be of much service.

Zinn: eine geologisch-montanistisch-historische Monographie. Von E. Reyer. 8vo. (Berlin: Reimer, 1881.)

IN this monograph, as is indicated by the agglutinative adjective on the title-page, the author has collected the results of his studies on the technological history of the metal tin under the threefold head of geology, mining, and history; or rather the reader may do so for himself from the material which is presented in an abrupt fashion without either preface or index. The first part of the volume is devoted to descriptions of the tin-producing districts of Saxony and Bohemia, the geological features of each district being first considered, then its history as derived from the local archives and notices in published chronicles, the whole of the facts concerning production being summed up in a chronicle of tin mining in Bohemia and Saxony, with tabular statements and diagrams of the production from the earliest period for which records are obtainable, about the year 1400, down to the present time. From these we gather that the total production of both countries, which was about 100 tons in the year 1400, reached in 1500 a maximum of about 1000 tons, since which time it has steadily declined, the produce at intervals of fifty years varying from 75 to 125 tons annually. At the present time the production is practically confined to Altenberg in Saxony, where about 50 tons are obtained from the treatment of a staminiferous granite containing about 8 lbs. of tin ore per ton. In subsequent sections of the volume the productions of Cornwall, Banca, and Australia are treated in a similar manner; a descriptive sketch of the geology of each locality being given in each case, followed by a chronicle of events and prices. These being mainly compiled from well-known sources, such as De la Bêche's "Cornwall and Devon," Von Diest's "Banca," the reports issued by the Australian and Tasmanian Colonial Governments, &c., present less of novelty than the first part, which contains much original matter derived from the author's own investigations; but the skilful manner in which the information is presented is likely to render the volume very useful to those interested in the subject. An unnecessary difficulty has been introduced by the adoption of the new-fashioned phonetic system of spelling which has latterly become prevalent in Berlin, and will doubtless prove a puzzle to many readers.

H. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Progress of Meteor-spectroscopy

IN the profonnd and eloqnent review of the progress of British and other discoveries in science during the last half century given in the Opening Address to the British Association at its recent Jubilee Meeting in York by its President, Sir John Lubbock, I am credited (NATURE, vol. xxiv. p. 409) with some meteor-spectrum observations which, while they certainly unfold some of the most important results arrived at in meteor-spectroscopy since its commencement in the year 1866, yet owe their recognition as scientific discoveries of some material weight and real consequence, to quite a different anthor.

Although with the first use of a meteor-spectroscope I recognised in the persistent streaks of the August Perseids of that year numerous examples of the yellow-sodium line, yet no proof was furnished by the slender spectroscopic power employed, of the existence in the meteor-streaks of any other substance. It was by a Hungarian astronomer, von Konkoly, that the presence of "lithium, potassium, and other substances" in the streaks of shooting-stars was afterwards discovered; and of some of these substances Herr von Konkoly obtained such repeated and well-verified observations, that the identification of their spectroscopic presence in certain meteor-streaks may be regarded as satisfactorily established.

The instruments made by Mr. Browning for the British Association Meteor-Committee in the year just mentioned were intended to be used in studying the spectra of the November Leonids, whose magnificent display took place as expected, but was of such short duration that nothing of great importance was, unfortunately, elicited as regards their spec ra. A more successful trial of the instruments had however been made previously on the 9th–11th of August of the same year,[1] and abnndant evidence was then obtained of the existence of two classes of meteor-streaks, both equally persistent, one of them affording a continuous spectrum only, like what hot sparks or train-matter would produce; the other more or less charged with, and sometimes consisting entirely of the yellow sodium-line.

No distinct evidence was obtained, however, in that first year's experimental trials of the occurrence in meteor-streaks of any other elementary spectrum-lines besides the solitary sodium one. The spectra of the nuclei were continuous, the brightest ones showing all the prismatic colours in perfection; and only one or two at the same time allowed some traceable evidence of sodium to be detected in their light. Bnt a few of the green "Leonid" streaks were noticed in November to be, to all appearance, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light.

Meteor-spectroscopes of a more efficient kind were afterwards devised and produced by Mr. Browning. But they remained, as far as I am aware, without any successful application until the nights of July 25th and 26, 1873, when the specira of three streak-leaving shooting-stars were observed through one of them by the enthusiastic astronomer of O'Gyalla, near Komorn in Hungary, Herr von Konkoly.[2] The streaks of the first two meteors seen showed only the sodinm-line; bnt in that of the third, which was an emerald-green meteor, the green spectral line of magnesium (Frannhofer's solar line *b*) was plainly visible in addition to the yellow sodium-line. The spectra of the nuclei were continnous, only the green region of the spectrum in that of the last meteor being of unusual brightness.

On the morning of the 13th of October in the same year Herr von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fireball, which was visible in the north-east at O'Gyalla. It exhibited the yellow sodium-line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star-spectroscope attached to an equatorial telescope, Herr von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler tube of marsh-gas.[3] They were visible in the star-spectroscope for eleven minutes; after which the sodium and magnesium lines still continued to be very brightly observable through the meteor-spectroscope; and the streak faded out of sight in a comet-seeker, at last, twenty-five minutes after it was first observed.

In Jnly and August, 1879,[4] and in August, 1880,[5] Herr von Konkoly observed spectra of the nuclei and streaks of many Perseids and other meteors with the Browning's meteor-spectroscope. The yellow sodium-line was conspicuous in most of the streak-spectra, and adjoining it there were seen in many cases the red line of lithium and another more distant red line supposed to be that of potassium; but the violet line of potassium,

[1] *The Intellectual Observer*, vol. x. pp. 38 and (with a coloured plate) 61; August and October, 1866.

[2] *Monthly Notices of the Royal Astronomical Society*, vol. xxxiii. (1872-73), p. 575.

[3] *Monthly Notices of the Royal Astronomical Society*, vol xxxiv. (1873-74), p. 82. The description "lightning-gas" there given of the tube is, as Herr von Konkoly afterwards informed me, a misprint for "lighting" or "coal-gas," "mit welchem die Strassen beleuchtet sind."

[4] *The Observatory*, vol. iii. p. 157. [5] Ibid., p. 577.

probably owing to its relative weakness in comparison with the red one, could not be observed. Green and blue lines of other elements were also noticed, among which the most frequently conspicuous one, next to the yellow sodium-line, was, again, the green line of magnesium.

On August 13, 1879, the nucleus of an emerald-green bolide, as bright as Jupiter, produced a splendid continuous spectrum from red to violet, exhibiting first a bright sodium-line, and immediately afterwards the green magnesium-line also, and some others, supposed to be those of copper, with two faint red lines. A similar bolide on August 9, 1880, showed on the continuous spectrum of its nucleus, besides the sodium-line very bright, those of lithium distinctly, and many metallic lines in the green and blue portions of the spectrum.

This occurrence of carbon, magnesium, and other spectral lines (possibly of iron) in the vapour-streaks of shooting-stars and fireballs, establishes a more certain and unequivocal resemblance between their chemical compositions and those of solid meteorites, than does the exhibition of the sodium-line, which, as Herr von Konkoly observes, may possibly be due to the original presence of saline particles in the air itself. But its extreme brightness in some, and total absence in other meteor-streaks, seems yet rather difficult to account for on that supposition. On the other hand the detection of carbon, while it agrees with the element's occurrence in siderites and carbonaceous aerolites, reminds us also of the abundant proofs which Dr. Huggins and other spectroscopic observers have obtained of the same element's prevalence in comets. And indeed the prolonged luminosity of meteor-streaks, with their complex gaseous spectra proceeding for long courses of time from an exceedingly attenuated atmosphere, is itself a physical riddle whose explanation as a mere question of radiation can scarcely be very different from what is demanded by the phenomenon of self-luminosity in the known gaseous nebulæ and in the envelopes of comets.

Of Dr. Huggins' applications of sidereal spectroscopy to neubulæ and comets, it may be mentioned that the extremely eventful discoveries are not individually named and noticed among the many high encomiums rightly bestowed upon that refined use of the spectroscope, in the opening address. But the results therefrom obtained were yet fully as revolutionising as regards the prevailing theories of those bodies, and of the general plan of construction of the sidereal heavens, as some of the spectroscopic discoveries described in the fifty-years' retrospect were (as is there lucidly related) thoroughly subversive of the formerly existing views of the internal physical condition of the sun.

If I have here ventured to disown, and to disclaim for myself some of the major accomplishments of meteor-spectroscopy by showing them to be the results of later, independent, and much more perfect observations, it is because, in comparison with the very significant amplification which those later observations have effected in the subject, the easy recognition of the presence of sodium in meteor-streaks can only claim to be regarded as a slight and inconsiderable first-adventure in a province of spectrum analysis, the additions and improvements subsequently made in which have been attended with much more remarkable success.

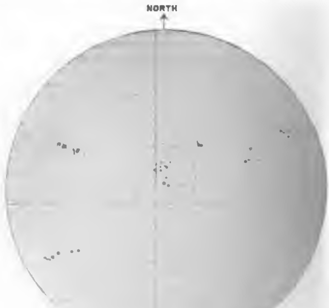
In the wide and accurate survey of the admirable opening discourse, which strays with truly lifelike fidelity over all the broad domains, the well-won fields and gallantly-scaled citadels of modern scientific knowledge, I shall, I trust, be pardoned if, in a matter of very little estimation by itself, I thus attempt to remove and banish from the eulogies of the address a small and unobtrusive and apparently unconscious excrescence of the otherwise harmless and innocent transgression, *magnis componere parva*.

A. S. HERSCHEL

Collingwood, September 12

Sun-Spots

RECORDS concerning phenomena are considerably enhanced in value if they include accurate determinations of the *times* of occurrence. This appears specially applicable to solar phenomena, and particularly to sun-spots, of which there must be many thousands of exact delineations without precise record of the times when the spots first appeared on the visible surface of the sun. No doubt there are several reasons to account for this unavoidable absence of valuable information; amongst others the intense brightness and heat of the sun make it an exceedingly disagreeable object for protracted telescopic inspection; nor yet are we in a position at present to photograph it continuously, so that we are necessarily content to compare photographs taken at intervals of perhaps many hours, and to assume, or at any rate not to dispute, that events of great importance have not occurred in the intervals. This is the more to be regretted because a knowledge of solar events is comparatively of little importance unless it helps us to ascertain what influence those events exercise on the earth and its inhabitants; and it is obvious that in comparing solar and terrestrial phenomena the times of occurrences are of essential importance, if only to avoid ascribing an undue effect to a given cause. It thus follows that even an approximate time of the appearance of sun-spots is not without value. On these grounds, as well as on the score of magnitude, I communicate the following particulars of a recent appearance, or outburst, of sun-spots, which occurred within certain moderate limits of time. I premise briefly that a photoheliograph is in daily use at the Trigonometrical Survey Office, Dehra Doon, India, of which I have executive charge. At present the instrument yields but a 4-inch negative, which is merely a microscopic delineation of $1\frac{1}{2}$ million millions square miles of solar surface; however, as surely



Tracing from sun negative taken at Dehra Observatory, Great Trigonometrical Survey of India. Latitude 30° 19′ 29″ N.; Longitude 78° 5′ 42″ E. Height above sea 2232 feet, on July 25, 1881, at 4.47 p.m. local apparent time. Spots visible in previous negative taken on the same day at 3.58 p.m. are coloured black; and the new spots which appeared between 4 and 5 p.m. are surrounded by a dotted circle.

as the sun shines, so surely are at least two negatives taken of it daily. Interruptions, even in a land of sunshine like this, sometimes occur; notably at the bursting of the monsoons, which occurred here last month (July), when the photographer was compelled to take the sun whenever visible, rather than not take him at all. Under this choice of alternatives the first negative (or say N_1) on July 25, 1881, was taken at 3.58 p.m. of local apparent time; it exhibited several sun-spots, as is now usual, and of which therefore little need be said, since solar observers are well aware that the sun has for some months past resumed a state of considerable energy in respect to development of features; the second negative, or N_2, was taken at 4.47 p.m. On comparing N_1 and N_2 it was at once seen that in the interval of 49m. a considerable group of spots had appeared in the neighbourhood of the sun's centre. It is exceedingly difficult to exhibit an exact delineation of spots when the negative is on so minute a scale; I however inclose a silver print, as well as a hand-tracing of N_2, from which the position and magnitude of the group, *i.e.* the new group, may be nearly inferred. This new group consists of sixteen spots, of which no individual spot is notably large, but there is this peculiarity about them all, that they exhibit hardly any penumbra, but consist almost entirely of well-defined umbra; what penumbra appears, is

confined chiefly to two spots, where it is seen only to the south-east; imagine a round, straight hole bored through a stratum of sand sufficiently adhesive for the sides to remain erect for a time, and after this suppose that the sand begins to fall inwards, creating a partial cone around to the south-east side; this is the sort of progress that these two spots convey. As to magnitude, the spots are scattered over an area of some 6000 millions of square miles; while the collective area of the spots themselves is about 630 millions of square miles, or, say, six times the area presented by the earth to the sun. Remembering that of solar change "a little goes a long way," so far as we are concerned, who shall say that changes of this magnitude are inappreciable on the earth, however ineffectual the instruments we can now command may be able to measure them? But was this sudden change inappreciable? that is now the question. Unhappily the sun remained invisible till July 30, when two negatives were taken, *i.e.* after an interval of just five days; so far as solar rotation could effect, the so-called new group of N_2 should have been visible not far from the sun's western edge; but the entire group had vanished, leaving no trace behind. In the interim of five days two new spots had come out; of *one* of these I may add that the umbra is about 200 millions of square miles, and the penumbra some 700 millions, presenting in all a single feature of more than 900 millions of square miles, or say nine times the area exhibited by the earth to a distant spectator. This ends the purport of my letter. But I cannot help adding that I believe the bright solar features or faculæ will eventually prove to be more effective exponents than the dark features or spots; as a matter of fact, faculæ commonly appear in abundance, covering considerable areas and branching out from one another like coral reefs; and it is a mistake to suppose that faculæ exist only in the vicinity of spots; the former may abound where the latter are quite absent, not only in a 4 inch negative, but in a very fair 5-inch equatorial. But I suppose the world will be better informed some day. Meanwhile, surely the sun is worthy of more earnest attention, not only from points of attack already so ably occupied, but from others none the less important, though at present greatly neglected: need I name solar radiation and photography? Physicians are alarmed for the safety of our bodies on detection of even a trifling change in temperature; but what do we know of fluctuations in the source of all terrestrial heat, though it be measurable with an actinometer? Again, land surveys are often made on huge scales; but for the solar survey of 1½ million millions of square miles, what is our largest delineation, and at how many spots round the world is the required daily record made? If a survey of London pays, depend on it surveys of the sun will pay all nations infinitely better.

J. B. N. HENNESSEY

India, North-West Provinces, Mussooree, August 5

Proneomenia sluiteri, Hubrecht

IN the report of the Proceedings of the Biological Section of the British Association which appeared in NATURE, vol. xxiv. p. 501, there is a slight mistake in the notice of my friend Dr. Hubrecht's paper on *Proneomenia*. This interesting mollusc is erroneously described as "one of the valuable finds of the *Challenger* Expedition." So far as I am aware, neither *Proneomenia* nor either of the other two genera of the *Solenogastres* (*Neomenia*, *Chætoderma*) was obtained by the *Challenger*. The only two specimens of *Proneomenia* which are known to science as yet were dredged by the Dutch Arctic Expedition of 1878 (or 1879), at depths of 110 and 160 fathoms in the Barents Sea. It was not obtained by the *Wilhelm Barents* in 1880, but we may hope that the dredgings of this season have been more productive, for Dr. Hubrecht informs me that 1881 has been a very bad ice year, and that the *Wilhelm Barents* has not succeeded in penetrating so far north as she has done in previous years. The summer has therefore been devoted to dredging operations, and valuable results may be expected. The zoological results of the Dutch Arctic Expeditions of 1878 and 1879 are being published as supplemental volumes of the *Niederländische Archiv für Zoologie*; and in the second of these, which is now in course of publication, will be found an elaborate memoir by Dr. Hubrecht entitled "*Proneomenia sluiteri*, gen. et sp.n., with Remarks upon the Anatomy and Histology of the Amphineura."

Eton College, September 24 P. HERBERT CARPENTER

Polydonia frondosa

THE Medusa mentioned by Mr. Archer in NATURE, vol. xxiv. p. 307, is undoubtedly *Polydonia frondosa*, Ag., figured in the Contributions to the Natural History of the United States. This Medusa was already known to Pallas, who described alcoholic specimens sent him from the West Indies by Drury. It is stated by Agassiz to be quite common along the Florida Keys. I have myself observed it in great abundance at the Tortugas, in the moat of Fort Jefferson, and in the mud flats to the north of Key West. They occur there in from three to six feet of water, the disk resting upon the bottom, the tentacles turned upwards; the disk pulsates slowly while they are at rest. Their habits when disturbed are well described by Mr. Archer. The young sometimes swim near the surface, and are far more active than larger specimens. When kept in confinement they also creep slowly over the ground by means of their tentacles, or, raising themselves sometimes edgeways against the sides of the dishes, remain stationary for a considerable time. The resemblance of Polydonia when at rest upon the bottom to large Actiniæ with fringed tentacular lobes, such as Phythactis, is very striking. The peculiar habits of Polydonia were noticed by Mertens in a species named by Brandt *P. Mertensii* in 1838; and found at the Carolines. The genus Polydonia was established by Brandt, and not by Agassiz, as is stated by Haeckel in his "System der Medusen." ALEXANDER AGASSIZ

Cambridge, Mass., August 27

Constancy of Insects in Visiting Flowers

MR. A. W. BENNETT's paper (NATURE, vol. xxiv. p. 501) on the "Constancy of Insects in Visiting Flowers" recalls a note I made at Cromer during the hot weather of last July. On the cliffs west of that town, where flowers were very abundant and of various colours, I carefully watched the movements of a small tortoiseshell butterfly to ascertain what flowers it visited. It was at first busy with bindweed; then it left this for yellow bedstraw (*Galium verum*), returning presently to bindweed. Then it tried a thistle, which detained it some time, after which it shifted to ragwort, and finally revisited bindweed. It seemed equally busy with all these flowers, though so various in form and colour. My tortoiseshell was therefore less constant than Mr. Bennett's, and its visits were successive, there being no interludes on grass, leaf, tree-trunk, or ground.

Homerton College, E. J. T. POWELL

[In Mr. Bennett's paper, p. 501, col. 2, line 31 from bottom, for *from* read *more.*]

Brewing in Japan

WILL you permit me to point out an error which has crept into the report of my paper on "Brewing in Japan" in last week's NATURE, p. 468. After mentioning the points in which *Kōji* differs from malt, the report continues:—"Kōji is prepared as follows: a mixture of steamed rice and water is allowed to remain in shallow tubs at a low temperature (0°-5° C.) until quite liquid; it is then heated," and so on. The following alterations will make the account of the Japanese brewing process correct:—"*Saké* (rice-beer) is prepared as follows: a mixture of steamed rice, *kōji*, and water is allowed to remain in shallow tubs at a low temperature (0° - 5° C.) until quite liquid; it is then heated . . . ". Not using malt as we do in our breweries, the Japanese have discovered for themselves a means of rendering the rice-grains diastatic with allowing the embryo to germinate. This is effected by exposing the softened rice grains to the action of dry steam, by which treatment the starch is gelatinised; when cold the spores of a mould are caused to grow over the surface of the rice, the mycelium being formed at the expense of the starch, and heat being liberated together with the usual products of combustion. The albuminoid matter of the rice, which previously was for the most part insoluble in water, is, after the growth of the mycelium, found to be almost completely soluble, and the solution possesses diastatic properties resembling those of malt extract. The main point in which it differs from the latter is in its superior hydrating power, for, unlike malt-extract, the solution of *kōji* very quickly converts maltose into dextrose. This material (kōji) is then used instead of malt in the mashing process, the sugar formed from the rice-starch under the influence of the dissolved kōji being dextrose, which is further fermented by the accidental introduction from the atmosphere of the germs of a species of yeast. The change induced in the character of the albuminoid matter under the influence of the growing mould is remarkable, and, I think, novel, and the interest of the observations I have made lies in

the support they give to the opinion that the diastatic property is connected with the degree of solubility of the albuminoid matter, and in the fact that this may result as well from the growth of an organism foreign to the grain as from the germination of the embryo itself. R. W. ATKINSON

College of Science, Newcastle-on-Tyne, September 19

Integrating Anemometer

PERMIT me to observe that the integrating anemometer devised by Mr. Shaw and Dr. Wilson, an abstract of whose paper, read before the British Association (Section A), appeared in your issue of September 15 (p. 467), is in principle and in several of its details identical with a machine intended for the mechanical reduction of anemograms of the Kew pattern adopted by the Meteorological Office, a description of which, with drawings, was placed by me in the hands of Mr. R. H. Scott, and by him transmitted to Prof. Stokes in February last. It is however to be noted that there is a fundamental objection to the mode in which such machines deal with the data submitted to them, namely this, that the air does not, in fact, move parallel to itself, as *these* integrators and Lambert's well-known expression assume that it does. In other words, the integrator should concern itself only with those particles of air which *are passing* the anemometer at each instant, *i.e.* with the directions and velocities of successive elements of the wind at a fixed point. Dr. von Oetlingen (Wild's "Repertorium für Meteorologie," Band v.) has shown this. CHARLES E. BURTON

38, Barclay Road, Walham Green, S.W., September 22

Red Rainbows

THE accounts in NATURE, vol. xxiv. pp. 431, 459, of pink and red rainbows induce me to mention one of a rose colour which was seen in this neighbourhood at sunset yesterday afternoon. Just before setting, the sun shone out with a pale golden glow, but about the north and east there was a general cloudiness, dark inky purple with light masses of cloud floating from north to south, and as the sunset glow lost its golden and assumed a ruddy appearance, these floating clouds took the same colour, the general cloudiness beyond retaining its purple character, and on looking north-east there was the rainbow, or rather the lowest part of the left hand of the bow, almost perpendicular, but inclining, of course, to the east; the general colour was rose, but along the inner side the prismatic colours were plainly seen. It lasted for about five minutes, and was seen by others who were just giving up shooting, about a mile from the house. The clouds in the west soon put on a stormy appearance, and rain began to fall. A. TREVOR CRISPIN

Hyde End, Brimpton, Reading, September 23

Hay Fever

IN Mr. Hannay's letter on Hay Fever (p. 485) two facts are mentioned, viz., that "those who are afflicted with hay fever are so owing to the tenderness of the internal lining of the nose," and that "in Scotland hay fever is practically unknown." By connecting these facts a probable remedy is suggested, viz., the use of snuff. That this habit destroys the natural tenderness of the internal lining of the nose is evident from the insensibility of the snuff-taker to doses that furiously irritate the nostrils that have been differently educated. As Scotchmen generally are either snuff-takers themselves or descended from snuff-takers, a direct or hereditary insensibility may explain their immunity from this affliction. Not being one of its victims, I am unable to try the experiment, which should be started a few weeks before the season commences, in order to gradually develop the acquired insensibility. W. MATTIEU WILLIAMS

Stonebridge Park, Willesden

IN NATURE (vol. xxiv. p. 485) Mr. Hannay remarks that "no remedy yet published will cure hay-fever." Has Mr. Hannay read Dr. Blackley's "Hay Fever" (Baillière, Tindall, and Cox, second edition, 1880)? It will be found that Dr. Blackley has used the treatment mentioned in NATURE, viz. the protection of the mucous membrane of the nose from pollen, with success both on himself and other persons subject to the fever, and Mr. Hannay's experiments offer another proof of the efficiency of this treatment. There is a short article on the subject in the *Lancet* of July 16, p. 82, by Dr. Thorowgood, and another by Dr. Blackley in the *Lancet* of August 27, p. 371. Mr. Hannay's treatment is essentially the same as that published by Dr. Blackley, though in the latter the inconvenience of plugging the entrances to the nasal ducts, and of the stoppage of the proper air-passages, is avoided, whilst the mucous membrane of the eyes is also protected. M. C.

September 24

Electric Light in Collieries

THE writer of the article in NATURE, vol. xxiv. p. 383, has overlooked the long account given in the *Times* of June 14, 1881, of the visit paid by the Accidents in Mines Commissioners to the Pleasley pit, near Chesterfield, where the first important application of the light was made nearly three months ago. Credit should be given to Mr. Swan and to Messrs. Crompton and Co., who for more than a year have been experimenting with, and perfecting, the lamps, &c., rather than to those who may have the good fortune to adopt that which the Pleasley trials proved to be so perfect; and, as one who was present with the Royal Commissioners, I think it only fair to call your attention to what is probably a slip in your report. SESAMY

London

THE ORIGIN AND FUNCTIONS OF THE BRITISH ASSOCIATION

MY attention has been called to a pamphlet published by Mr. W. H. Harrison, purporting to contain a correct account of the first founding of the British Association for the Advancement of Science. I am sure that Mr. Harrison, in common with such other readers of NATURE as take an interest in the affair, will be glad to hear my father speak for himself upon a matter which Mr. Harrison, with the amount of information at his disposal, could only treat of as a subject of speculation. The paper which I inclose was addressed to Sir Edward (then Colonel) Sabine; and I think I may claim for it that it is written with much clearness and impartiality. You may perhaps also consider the letter of importance at this moment, as pointing out what was the view taken in those early days of the proper functions of the Association. The wisdom of this view is abundantly evident now that science has been so widely popularised, and that little more of real work remains for the Association beyond the just apportionment of its funds for scientific purposes. In respect to the numerous scientific letters addressed to my father by Buckland, Murchison, Smith, Sedgwick, Scoresby, Humboldt, Wollaston, Davy, Sabine, Faraday, Brewster, Babbage, Prout, Herschel, Whewell, Forbes, Liebig, De la Bêche, Lyell, and others, I hope some day to cause a selection of them to be produced, in a form which may be of interest, and perhaps of use to the public. E. W. HARCOURT

Nuneham Park, Abingdon, September 23

Account of the Formation of the British Association by the Rev. W. V. Harcourt

"TO COLONEL (AFTERWARDS SIR EDWARD) SABINE

"I HAVE received from the President of the Philosophical Society of Hull (1853), where you know the British Association is about to meet, a memoir which he has put into public circulation descriptive of the nature of that body, its early history, and the specific services rendered to it by individuals.

"The task which Mr. Frost has undertaken is one of a difficult and delicate kind; and I was not surprised to find his description of circumstances with which he had no means of being intimately acquainted somewhat inaccurate and defective.

"Mr. Frost informed the public that when in 1831 Sir David, then Dr., Brewster, made proposals that meetings for promoting science by *réunions* of scientific men similar to those which prevailed abroad should be held in England and commenced at York, the country had been duly prepared and predisposed for such co-operation by the severe strictures which he had then recently passed on the actual state of science in this country, and on the conduct and character of its scientific institutions, and in

particular its universities. It would have been better if these strictures, now forgotten, had not been adverted to, especially with reference to the Association. The truth is, they formed the chief difficulty in carrying such a proposal as had been made into effect. It was clear that any attempt at scientific association not headed or joined by many persons who could not but feel aggrieved by the strictures referred to, and who have been since among the chief lights of the institution, would probably have led to results more mischievous than beneficial to science.

"As soon as Dr. Brewster's proposal was made, and before it could be acceded to, I thought it needful to enter into correspondence with numerous individuals thus situated, and finding that, agreeing for the most part in the opinion that such *réunions* would operate for the benefit of science, they lost sight of all personal feelings, and consented to co-operate on certain conditions, I proceeded to draw up the scheme which was ultimately followed.

"It is a mistake to consider this Association as having been formed on any foreign model. My conception of the manner in which a great scientific combination might be effectually worked in England was founded on different principles. No one could be insensible of the advantage to be derived from bringing men of science together to confer and discuss; but even this point I considered it impossible to gain without extending our views considerably further. I did not believe that the great labourers in science would undergo the inconvenience and interruption of travelling to various places to meet one another, as a continuous system, on mere invitation, and for the sole purpose of discussion, and I knew that if such men should absent themselves from the meetings, those meetings would become no better than *foci* of *sciolism* and vanity.

"I therefore proposed to found the Association on the principle of *acquiring funds* to be devoted to the expenses of unremunerative objects of science, of levying such funds from the multitudes of persons who might be expected to feel interested in scientific discussions at populous places, and giving the appropriation of them first to the selection of committee-men attached to the various sections of science, and secondly to the final determination of the whole body of actual scientific labourers at the meeting assembled in general committee.

"To this principle in the constitution of the British Association its success has been mainly due. To this principle we owed, for instance, the unintermitting attendance, to the time of his lamented death, of one of its ablest members, Mr. Baily, under whose direction one of the largest applications of its funds was made.

"These grants of assistance, conjoined with requests to individuals to execute particular tasks for the interests of science, have given the exertions of the Association as a body a direct utility peculiarly its own, tending far beyond the promiscuous discussions of the sections both to advance material objects and to maintain the attendance at its meetings of persons pursuing such objects.

"The wealth, the public spirit, the intelligence, the curiosity, of the great cities of the United Kingdom, offered great encouragement to the financial part of this plan, which by its adoption has enabled the Association to carry out its entire objects not only in regard to liberal grants for scientific objects and in defraying all expenses incidental to its operations and essential to its permanence, but even in maintaining an establishment of its own for experimental research.

"This plan, proposed by me at York, was adopted in all its detail, and my acceptance of the office of general secretary enabled me, with able and zealous co-operation, to work a machine of great magnitude and complexity with a success surpassing my expectations from 1831 to 1837, during which years I was charged with its chief management, and revised all that was printed in its name.

"The cordial reception of the first meeting of the British Association by the city of York, the hospitality of Bishopthorpe, the countenance of the Royal President of the Royal Society, the presence of Lord Fitzwilliam, the aid of Prof. Phillips, the attendance of the distinguished philosopher Brewster, with Brisbane, Robison, Forbes, and Johnston, the attendance from London of Murchison, from Dublin of Provost Lloyd, from Oxford of Daubeny, from Manchester of Dalton, the concurrence of Buckland and Whewell and Conybeare, and many others of known repute, these incidents helped to launch the vessel; of the early history of which, if any one would write accurately of that part of its history, he may record that Brewster first proposed that a craft should be built wherein the united crew of British science might sail, and manfully embarked in it all his high scientific reputation; but for myself I must be allowed to claim that I manned the ship, that I constructed her charts, and piloted the vessel for six years. The labour which I bestowed on this service has since been divided among more capable hands; but none of us could have worked the vessel at all without the constant and invaluable helping hand of the assistant-secretary, Prof. Phillips.

"I am induced to put down on paper and transmit to you, as actual President of the Association, a statement of the real facts, without the least intention, however, of involving either you or any one else in controversy on the subject."

THE INTERNATIONAL EXHIBITION AND CONGRESS OF ELECTRICITY AT PARIS

THE Exhibition must be pronounced a great success. Even those who are well read in electricity are taken by surprise at the display of power presented; and the first favourable impression is strengthened as further examination discloses the immense variety of applications exhibited and the beauty of much of the machinery.

The first thing seen on approaching the Exhibition from the city is the Siemens electrical railway. It is about a quarter of a mile long, with a sharp turn at one place. The carriage is a good-sized tramcar, and presents no special feature to a casual observer, except two wires which travel with it, and connect it by running contacts with two aërial guides suspended on posts like telegraph posts. The prime mover is a steam-engine near the centre of the Exhibition, which drives a dynamo-electric machine. The continuous current which the latter furnishes is led to the aërial guides, and is conducted by the two travelling wires to an electro-magnetic engine beneath the floor of the carriage which drives the wheels. Passengers are conveyed by this tramcar at a small charge between an outdoor station and a station just within the Exhibition.

On entering the building by this tramway one of the most prominent features is the collection of powerful engines which occupies the whole of the space under one of the side galleries. They are for the most part dynamo-machines driven by steam-engines. The dynamos are close to us as we walk down the main passage on this side; the steam-engines, which drive them by belts, are a little further back; the furnaces and boilers are close to the outer wall. Wires are led from the dynamos to electric lamps, some of them close at hand, and others on the opposite side of the building, or overhead.

Every variety of electric lamp is of course to be seen, and their regulators furnish a very interesting study. To illustrate their diversity we may mention that in the Brush system the regulation depends on the variation of resistance in a series of carbon plates as they are more or less strongly pressed together; in the Crompton it depends on the frictional support of a vertical metallic rod by two pieces which pinch it between them, and pinch it more or less strongly according to the strength of the current; while in the Pilsen lamp a spindle-shaped piece of iron is the common core of two electromagnets one above the other, and is drawn up or down as the contact in one or the other prevails. Then again there are the Serrin and other well-known forms of lamp, in which the carbons are caused to approach by means of clockwork, which is regulated by the strength of the current. There is the Jablochkoff candle, in which the two carbons are parallel and separated by plaster of Paris; the Jamin, which is something like the Jablochkoff, with the plaster of Paris removed, and only air between the carbon pencils; the Werdermann, in which a carbon point below bears against a flat block of carbon above (the point being the positive, and the block the negative terminal); the Joel, in which a carbon point below bears against a disk of copper

above; and the Soleil, in which the point of contact between the carbon point and the copper is surrounded by chalk or lime, which is rendered incandescent.

Then there are the "incandescent lamps," specially so called, in which a thread of carbon a few inches long is inclosed in a vacuous space where, as there is no oxygen, there will be no combustion, and the carbon does not waste. Swan's light, which is of this class, occupies a very conspicuous place in the Exhibition, and is used for the illumination of the Salle des Séances, in which the meetings of the Congress are held. Edison's two rooms are nightly thronged by visitors, who come to see not only his lights, but his numerous other inventions, which are here exhibited. Lane-Fox's light and Maxim's (which has been stopped by some accident) belong to the same class. No opal or ground glass is necessary with incandescent lights, as they are less dazzling than arc lights. They certainly give very beautiful illumination to a room, and their convenience for lecture-room purposes was well seen on the occasion of an illustrated lecture given by M. Mercadier in the Salle des Séances at the meeting of the Society of Telegraph Engineers on Thursday last. They can be extinguished in a moment and re-lighted in a moment.

The Exhibition is open in the daytime from 10 till 6, and in the evening from 8 till 11. The largest attendance is in the evening, when the lights are in full action. Besides those inside, which make the interior almost like daylight, there are two very powerful lights above the roof, which are furnished with reflectors, and throw beams of light like comets' tails in various directions.

The Congress commenced its sittings on the 15th inst., when an opening address was delivered by M. Cochéry, the official president, and the hours of meeting and other details of organisation were arranged. The foreign members were called upon to elect three vice-presidents to join the three French vice-presidents (all official) who had already been named. After a brief conference Sir William Thomson, Prof. Helmholtz, and Prof. Govi of Naples were proposed and unanimously elected. It was agreed to divide the Congress into three sections, devoted respectively to theoretical electricity, telegraphy with telephony, and miscellaneous applications of electricity, including the electric light; the first section meeting at 9.30 a.m., the second at 2, and the third at 4 p.m. Each section has sat for about two hours daily, an interval of two hours between the first and second being allowed for *déjeuner*.

M. Dumas was elected president of the first section, with Prof. Kirchhoff and Dr. De La Rue as vice-presidents, Prof. Mascart and M. Gérard being secretaries. The discussion of the subject of international electrieal units, the choice of which is regarded as the most important work of the Congress, was then begun, and occupied the rest of the sitting. Sir William Thomson introduced the question in a very lucid speech, in which he described the course which had been taken by the British Association, and recommended a substantial adoption of the British Association system. He was followed by Professors Wiedemann and Helmholtz, who favoured the adoption of a mercurial unit of resistance; and a large committee, containing men of both views, was appointed to draw up a Report. This Report was anxiously awaited, and was presented on the 19th inst. It consisted of the following seven recommendations, which had received the unanimous consent of the Committee, and have now been formally adopted by the Congress.

1. The fundamental units for electrical measurements to be the centimetre gramme and second (C.G.S.).

2. The practical units ohm and volt to retain their present definitions, 10^9 for the ohm and 10^8 for the volt.

3. The unit of resistance (ohm) to be represented by a column of mercury of a square millimetre section, at the temperature zero Centigrade.

4. An international commission, to be charged with the duty of determining by new experiments, for practical purposes, the length of the column of mercury, of a square millimetre section, at zero Centigrade, which represents the value of the ohm.

5. The name Ampère to be given to the current produced by a volt in an ohm.

6. The name Coulomb to be given to the quantity of electricity defined by the condition that an Ampère gives one Coulomb per second.

7. The name Farad to be given to the capacity defined by the condition that a coulomb in a farad gives a volt.

It will be observed that the "weber," a unit familiar to British electricians, is not mentioned in these resolutions. The reason, as stated by Prof. Helmholtz to the Congress, is that Weber himself employs a unit of current derived from the millimetre, milligramme, and second, and this unit, which is one-hundredth of the C.G.S. unit, or one-tenth of the weber, as commonly understood by British electricians, is known as "the weber" in Germany.

The reason for adopting a mercurial standard defined by size was explained by Sir William Thomson to be the desire to guard as much as possible against secular change.

It transpired in the discussions which took place in committee that mercurial standards, as actually constructed, are glass tubes which must be refilled with mercury every time they are to be used. The external communications are made by means of platinum wires attached to plates of the same metal, the latter being well amalgamated before use. It is obvious that these operations involve much more labour and risk of error than comparison with a standard coil; and we therefore do not anticipate that recourse will be had to the mercurial standard except on rare occasions. Coils will as heretofore continue to be used for all ordinary measurements of resistance. The international committee which is to make the new determination will be nominated by the governments of the various countries concerned, and independent determinations will doubtless be made by different members of the committee in different laboratories. It will thus be seen what amount of consistency is attainable in such measurements, and whether it is sufficient to render the standard practically accurate. The German authorities assert that accuracy to one part in two thousand can thus be ensured.

THE CAUSE OF COLLIERY EXPLOSIONS

ONE of the most instructive documents ever penned on the subject of the cause of explosions in collieries has recently appeared, in a lately-issued Blue-book, in the form of a Report to the Home Secretary by Prof. Abel, C.B., F.R.S., of Woolwich, who, at the request of the Home Department, conducted a series of experimental researches upon the cause of the terrible disaster at the Seaham Colliery on September 8, 1880. In 1845 Faraday and Lyell first directed attention to the influence exerted by the presence of *coal-dust* in mines upon the magnitude of an explosion of fire-damp. In 1867 and 1875, the subject was further advanced in France by Messieurs Verpilleux and Vital, the latter of whom showed that air charged with fine coal-dust, rich in inflammable material, may explode when there is present a much smaller proportion of true fire-damp than is of itself sufficient to constitute the atmosphere an explosive one. Still more recently Mr. W. Galloway has conducted a valuable series of investigations and experiments, the results of which have been communicated to the Royal Society in three very important memoirs. In the first of these he showed that a certain mixture of air and coal-dust, not itself inflammable, became so when there was also present a much smaller proportion of fire-damp than any Davy lamp could detect. In the second he showed

that the return-air from the ventilating shaft of a mine may actually contain enough fire-damp to become inflammable when coal-dust is diffused into it. In the third he concludes that the influence of the coal-dust must not be considered as merely aggravating and increasing the explosion originating with the presence of fire-damp, but that the presence of the dust must be regarded as the one thing which, if a small explosion takes place anywhere, will accumulate and carry forward the force of the explosion with ever-increasing energy into every empty space in the workings, however ramified.

During the current year, experiments have also been made on the subject; at Harton Colliery (Durham) by Mr. Wood and Prof. Marreco, at Broad Oaks Ironworks by the Chesterfield Committee of Engineers, at Garswood Hall Colliery (Wigan), by Mr. Smethurst and the Royal Commission on Accidents in Mines, and lastly at Woolwich by Prof. Abel. The general character of the experiments has been on a plan originally devised by Mr. Galloway: viz. to expose to a flame, or to the flash of a small cannon, a stream of air in a miniature gallery into which any desired percentage of coal-gas or fire-damp was introduced, and into which coal-dust could be diffused by a hopper; arrangements also being made to raise the temperature of the gases, and to increase their velocity at will. The majority of the experimenters believe that in no case does a mixture of air and coal-dust *without fire-damp* explode, although the Chesterfield Committee think they have evidence that flame will travel in dust-laden air without a trace of fire-damp being present. This matter is of great importance, for it has been shown that in flour-mills explosions which have occurred may be traced to the presence of combustible dust in the air.

Prof. Abel had placed in his care thirteen samples of dust, some burnt, others unburnt—collected from different parts of the Seaham mine; which samples were subjected to careful examination in the microscope, and to chemical analysis. They were found to contain from 64·83 to 99·75 per cent. of pure coal-dust, some of them containing ash, grit, and fine sand in various proportions. They were then tested as to their power to aid in producing explosions in an experimental gallery. The gas employed was an explosive pit-gas, of such a quality that a mixture containing only 3·5 per cent. of the gas with air when travelling with a moderate velocity (from 200 to 1000 feet per minute) was ignited by the flame of a naked Davy lamp. In perfectly still air from 4 to 4·5 per cent. of the same gas was necessary to produce the same result. Currents of mixtures of this gas were conveyed into the experimental gallery at a velocity of 600 feet per minute, and at a temperature of 80° F.; a naked Davy lamp, its flame protected from the draught by a small screen, being placed in the gallery at about 12 feet from the place where the dust was supplied to the current. More and more fire-damp was gradually added until explosion took place; that dust being regarded as *most sensitive* which produced explosion with the least percentage of fire-damp. When the relative sensitiveness of the various samples of dust had thus been ascertained, it was found that, of the four which stood head of the list in point of sensitiveness, three headed the list also in point of richness in combustible matter and in point of fineness of texture. But the sample which stood third in point of sensitiveness was not only not the finest, but stood absolutely bottom of the list, in point of richness. It therefore appeared that porosity and mechanical condition are more important than combustibility of the dust in bringing about the ignition of a fully explosive gas. Prof. Abel was led, in consequence, to try whether the ignition of a mixture of air and fire-damp in a low percentage not inflammable of itself by contact with a lamp-flame could be brought about by the agency of a wholly incombustible dust. Accordingly dust such as calcined magnesia, powdered chalk, and slate dust was tried; and it was found that instantaneous explosion was thereby produced in currents of air containing only 3 to 3·5 per cent. of fire-damp. It appears then that *dust of any kind*, as a finely-divided solid, can operate in determining the explosion of an otherwise harmless mixture of gas and air; probably by furnishing, as the particles pass through the flame, successive red-hot nuclei, by which the heat is localised and rendered more intense.

In the special case of dust that is both fine and combustible, as coal-dust may be, it was proved that so small a proportion of fire-damp as 2 per cent. in moderate currents may determine the propagation of a flame by coal-dust. Now, as it is stated on the best authority that the most experienced eye cannot detect the presence of 2 per cent. of fire-damp by its effect on the flame of the ordinary Davy lamp; and as (in spite of all the host of little inventions to detect smaller percentages) the Davy lamp remains the only practical test of the presence or absence of fire-damp in fiery mines, it follows that, in every mine where there is any fire-damp *at all*, the mere dust of the mine constitutes an element of danger of which the risk is simply incalculable. When we add to this that experiments, made by firing such small blasts as eighty *grains* of gunpowder may represent, show that dust may cause the propagation of flame in air-currents containing percentages of fire-damp *far smaller than any* of those mentioned above, it is clear that whatever the risks with Davy lamps may be, they sink into insignificance beside the frightful dangers attending the firing of a shot for purposes of blasting. The practice of blasting the coal cannot be too emphatically condemned. It is at best a lazy and slovenly process of getting the coal, and considering the risks it entails, ought to be stringently and at once put down by legislation.

The practical moral is that, while the Davy lamp is to be regarded more than ever as a necessary of work in the pit, it cannot be regarded in any way as a safeguard of absolute kind against explosion; still less can it be regarded as an indicator of the presence or absence of impending danger, inasmuch as it is absolutely incompetent to detect such feeble percentages of gas as Prof. Abel has shown to be dangerous in the presence of the inevitable dust of the mine.

Science, which gave us the safety-lamp, must therefore be called upon once more to provide efficient substitutes. (1) A new lamp, electric or otherwise, must be devised, which shall be wholly independent of a supply of air from the galleries in which it is used; (2) an indicator must be invented to do what the Davy lamp fails to do, viz. to detect in the workings of the mine the presence of a proportion of fire-damp less than 2 per cent., and to indicate rapidly and accurately its amount. Let us hope that Prof. Abel will crown his labours by giving us such new instruments.

THE LANDSLIP AT ELM

THE Swiss papers contain valuable information as to the landslip which occurred on September 11 in the valley of the Sernft River, in the canton of Glarus. The month of September is notable in Switzerland for landslips. Thus the great landslip of the year 1618, which buried the whole of the town of Plurs in Graubünden, with its 2340 inhabitants, occurred on September 4; and the great downfall of the Rossberg Mountain, which destroyed the village of Goldau, with three other small villages, burying 111 houses and 457 persons, and filled up the Lake of Lowerz, occurred on September 2, 1807. The very heavy rains of the last few weeks have softened the rocks on the slopes of the Plattenberg Mountain, at the foot of which, at a height of 3330 feet, was situated the village of Elm, now almost completely destroyed by the landslip. The clay-slate quarries which were worked

upon the same slope have divided the masses of the rocks into large pieces, whilst the frequent earthquakes of the last months have given rise to large crevices in the slates and limestones. Already on September 9 it was perceived that the soil at the quarry was in slow motion, and a house situated immediately below was evacuated. Two days later, between five and six o'clock in the afternoon, it was seen that the forest on the slope of the mountain began to move, the trees being bent like a field of corn during a strong wind; they then rushed down, together with the rocks situated above the quarry, breaking up into thousands of pieces. This formidable stone avalanche reached the village, the trees were bent like straw, and the houses moved by the pressure of air pushed by the landslip. Men and houses were thrown on the opposite side of the valley, smashed against rocks, and buried by the landslip, which, as in the catastrophe of the Rossberg, crossed the valley and rose up-hill on its opposite side. The first landslip destroyed that part of the Elm Commune which is named Unterthal; but a second one followed immediately, destroying the village, and throwing the houses on the opposite side of the valley, one kilometre wide. The picturesque valley of Unterthal is now covered with a mass of mud, earth, and stones, thirty to forty metres thick, on the surface of which are seen blocks of the size of a house. The length of the landslip is about two kilometres, and the opposite side of the valley is covered with stones and blocks on a space of about 100 metres. The Sernft River, which flows in the valley, is barred by the *débris*, and has formed a small lake. The number of persons killed is about 160. Another small landslip occurred on the following day, and the slope of the mountain continues to be in motion. According to a report of Prof. Heim the remnant of the village is also threatened by a landslip, the Risikopf, or Grosskopf, being creviced and undergoing subsidences which render a landslip most probable, not so large, however, as the preceding one.

The *Times* Geneva correspondent writes under date September 19:—"According to the measurements and estimates of Prof. Heim, of Zurich University, who has just visited Elm, the earthslip of yesterday week, though less destructive of human life than the earthslip of Plurs and Goldau, probably exceeds in extent either of those catastrophes, great as they were. The portion of the Tschingel Alp which broke away from the parent mountain measured at its base 400 metres by 350 metres. The length of its projection outwards cannot, of course, now be ascertained. The length of what Prof. Heim calls the *débris* stream is 1500 metres, and varies in breadth from 300 to 400 metres. The distance of the extreme end of the stream from the place whence it broke away is 2000 metres. The extent of the valley bottom, which is tolerably even, covered by *débris* is computed at 570,000 metres, while the entire mass makes a total of 900,000 square metres. From the lower part of the valley to the upper joint of rupture the height is 620 metres. The fall was, therefore, a little over 2000 feet. The lowest estimate of the contents of the slip, according to the admeasurements of the engineers, is 10,000,000 cubic metres. It contains, says the Professor, enough stone to build two cities as large as Zurich, and the population of Zurich is 76,000. Some of the blocks, which are heaped 112 metres higher than the village of Elm, measure 1260 cubic metres, and are estimated to weigh 3300 tons. If the other earthslip, which is regarded as imminent, should take place, all that remains of Elm will be destroyed."

The heavy rains of the last weeks have caused several other landslips in Switzerland and Savoy. In the Upper Singine, in the canton of Freiburg, the soil is in slow motion in the valleys of the Gérine and Singine rivers, and a landslip of some importance has occurred at Planfayon. Another landslip occurred on September 2, close by Bernex village, on the slope of the Dent d'Och, and it is rather remarkable by the circumstance that it occurred in a broad open valley where one never would suppose the possibility of a gliding of rocks.

A further interesting result of the recent heavy rains is that the Lake of Bicune, which is somewhat lower than that of Neuchâtel, is now so full that its water runs into the Lake of Neuchâtel, inundating its shores.

PHENOMENA DEVELOPED BY HELIOSTATIC STAR-DISKS

A HELIOSTAT of the highest class is doubtless beyond the means of ordinary observers, but such an instrument as the one now described is readily obtainable. Three sets of achromatic lenses forming a focal power of forty at ten inches, or a miniaturing power of one-fortieth, are in general sufficient. If formed into a microscopic object-glass, the front is turned towards the sun. The glass then refracts a beautifully small star-disk, which, owing to the large angular aperture of the combination, remains steadily in view for several hours. The optical characters of this disk vary considerably with the quality of the lenses; practically a very fine one-quarter by Powell and Lealand produces disks of remarkable beauty and precision. In some cases a plane mirror is conveniently attached to reflect the oblique solar rays.

The instrument thus provides a stationary solar star-disk for continuous observation. No clockwork or machinery is required. The size of the disk is one-fourth of the sine of the solar diameter, or nearly 45-10,000ths of an inch.

A more brilliant form of surpassing effulgence is occasionally employed by a 3-inch lens placed before a right-angled prism An aërial image of the sun thus produced outshines the electric light. These disks are viewed at a distance of ten feet.

It is proposed to describe first their use in microscopic research, and secondly for telescopic vision.

I. Microscopic Research

The Miniature Method.—A strong plate fitting the upper stage of the microscope by means of screws is pierced in the centre by an aperture carrying the "societies" standard screw, into which an objective can be firmly screwed. The stage motions then give readily the necessary adjustments for coincidence of optical axes. This is called the *stage-holder*. All previous methods of fixing the objective in the sub-stage have been abandoned; the necessary steadiness being almost unattainable.

Phenomena of Heliostatic Star-Disks produced by the One-quarter.—Stage-holder armed with an inverted 1-32nd water-immersion. The miniature of the star-disk is now viewed microscopically with a 1-16th immersion. When both of these objectives are adjusted for the most brilliant vision, *distant foliage is distinctly visible*. A flag-staff carrying the Union Jack 180 yards away displays its double cross. The fine lightning-rod surmounting it is distinctly visible. Houses on a hill glisten in the sunshine; but conspicuous above all is the minute solar star-disk blazing with all the glory of a midday Sirius at the open window-sill.

Here the favourite tests for telescopic precision come richly into play. A minute brilliant bead surrounded by the most intensely black ring—the more wonderful as the brilliance seems to heighten its rare and beautiful delicacy and blackness—comes up and plays into expanding coloured rings on each side of the principal focal point. (The delicate beauties of this exquisite phenomenon cannot well be seen without an exceedingly delicate fine focal adjustment.) The focussing wheel (constructed for the lightest contact) is divided into 132 parts; twenty-six give a focal plane the 1000th of an inch deeper or higher,

but a tenth part of this, or a change of focus of the 10,000th of an inch, changes the appearance of the magnified star-disk. These changes, so vivid and sudden, produce a lively impression of the minuteness of the wavelets of light which generate these diffractive phenomena.

The diameter of the brilliant disk, as miniatured, is about the 1-20,000th of an inch; the jet black ring in which it appears set, and indeed well set-off, is about 1-100,000th thick. Slight changes in the focus, but especially slight changes in the corrections of the observing and miniaturing glasses, produce a new order of phenomena, full of a significant meaning and practical import. The minimum visible is perpetually forcing itself upon the observer's attention. The lightning-rod (here visible) 6000 inches away, is miniatured 600 times smaller than at 10 inches by the 32nd objective, which then would diminish an object only 320 times. The rod is therefore depicted nearly 200,000 times smaller. It is exactly half an inch in thickness; its size therefore in the miniature is 1-400,000th.

As the evening light faded away a long row of gas-lights reaching half a mile came into view in pretty perspective, the more distant being very slightly out of focus (six divisions). The 32nd objective required to be advanced the 4000th part of an inch for the distant light. In this case the miniature was produced by a Zeiss 1-32nd, and viewed by another 32nd by the same maker. The lowest eye-piece is employed and a shortened eye-tube. A single glance at these microscopic landscapes satisfies the observer at once as to the quality of the instruments of observation. Achromatism is seldom attained without generating a whitish haze, the inevitable accompaniment of residuary spherical aberration. This haze is an invaluable indication.

The haze observed in miniatures examined by high magnifying power is an invaluable indication of spherical residuary aberration. The method gives a cruel test of the optician's art. Its discovery led to the subject coming before the Royal Society and its being embodied in their *Transactions*. When first seen it was exhibited as a strong yellow fog. The announcement of it occasioned the greatest surprise to the distinguished makers of a "very fine" set for the writer. In most cases the higher the angular aperture the denser was seen the fog. The following is quoted: "Mechanical arrangements are shown by diagrams, Figs. 1, 1*a*, plate LII." (*Phil. Tr.*, vol. ii. 1870, p. 592).

"*Experiment I.*—Miniature of a small thermometer, the ivory scale being graduated 24° to the inch. A power of 300 diameters: low eyepiece 'A' and objective of one-eighth focal length (made expressly for Podura testing) was applied to view the miniature formed by a one-sixteenth objective. The following appearances were carefully noted at the time of observation:—

"*Result.*—The sparkle of light on the bulb of the instrument, the graduation, and the mercurial thread within the glass are invisible, obscured by a nebulous yellow fog which no objective adjustments are able to dissipate."

In consequence of this unexpected discovery regarding the quality of a "very fine" one-eighth, it was returned to the opticians, to their surprise, for better compensation. After improvements a very slight nebulous yellow cloud now only remained.

A new fact now came up. A *miniature* formed by an imperfectly corrected glass is comparatively free from the aberration shown by the same glass used as a microscope. Thus, viewed by a good glass, the miniature of an inferior one bore wonderful magnification by an excellent objective.

In innumerable objects surface markings are shown only: with no perspective and with no foreshadow of deeper structures such objects are opaque. But if transparent, the foreshadow of deeper structures confuses the appearance of surface; strange eidola are generated difficult of interpretation and dispersion. A series of star-disks in deeper foci intermingle their diffractions into beautiful forms.

The strong fact that these diffractions of a given disk are wholly developed towards the eye of the observer, or wholly developed beyond the true focal plane, according as the correction of the glasses is over-wrought or under-wrought, reveals an infallible clue to many spurious effects. This method therefore more severely tests the observing instrumentation than the miniature.

The following experiment, arising out of the phenomena in course of observation, is quite a microscopic study in itself:—

Experiment.—Miniature of garden view formed by a 1-30th plano-convex lens and examined by a microscope armed with 1-8th giving 400 diameters.

Result.—Landscape dark and hazy. But upon using the same power (400) with a deep eyepiece and a half-inch objective, there started forth an exquisite picture brilliantly lit up. Even the foliage glittering in the sunlight was sharp, clear, and decisive, the details being marvellously displayed. This large increase of light with diminished observing angular aperture is at first sight astonishing.

It is to such causes doubtless that the microscopic world so long disputed whether the markings on diatoms were depressions or elevations. The earlier plates of such objects, as given by Quekett, teem with eidolic varieties of form. It was thought impossible to resolve them, as it was called, without complicated stops, which in fact shut off the unsuspected residuary aberration.

But it is necessary to pass on to the *fundamental circular spectrum* of a minute solar disk. A simple plano-convex lens of half an inch focus is placed on the microscopic stage and made to form a miniature of the prism star in the field of the microscope. If the experiment be properly conducted, forty gorgeously coloured rings may be counted. I have frequently examined these phenomena with a power of 1000 diameters. Above the best focal point is a bright fog; below are seen the glorious diffractions. The sun, as far as it could be made out, was a spurious disk nearly 6-100,000ths of an inch; the first black-ring, the blackest thing I ever beheld, was nearly the 1-50,000th. Each ring beyond appeared exactly of the same breadth—nearly the 1-16,000th.[1]

If now an over-corrected lens were substituted, the diffraction rings ascended and the nebulosity descended: they exactly changed positions as regards focimetry. The conclusion follows that all brilliant objects present diffractions above or below the true focus according as the observing instrument is over- or under-corrected by means of the usual screw collar adjustment.

In this way the same object, especially diatoms, may be made to take several very deceptive forms, because spuriously diffracted. The same is true of all brilliantly illuminated transparent structures.

The intersection of an infinite number of cones of light converging to different points of the axis here produces the well-known interference extinction of undulation evolving precisely-formed rings of darkness. The simple lens, in these observations, develops circular spectra, as being formed by an infinite number of prisms.

An important outcome of this phenomenon is the unerring test (here presented by a simple single convex plane lens) of the highest possible order, as to the *quality*

[1] The contemplation of these phenomena, utterly eclipsing in their brilliant beauty and precision of form the fainter diffraction phenomena described by Sir John Herschel as amongst the most gorgeous in nature, astonishes every beholder. Some little skill is required in gradually toning down the excessive, I might say painful, glories of the appearances—lengthening the eye-tube; glass tinted wedges (a single field-lens of a Hyghenian as eyepiece produced a spectrum apparently twelve inches in diameter, measured with the left eye by rule laid on the stage), or by camera.

or inferiority of the observing microscope. Extreme steadiness and a *particularly* delicate fine focussing adjustment are indispensable for successful observations. It is impossible here to detail them.

A plain silvered mirror of the old style must be discarded as a solar reflector; prismatic internal reflexion or reflexion from a metallic surface is indispensable for producing purity of spectra.[1]

But it is when superb objectives are placed as it were eye to eye, that the finest observations can be made (especially when both are used with their noses inserted in the proper immersion fluid for observing landscapes) on distant objects.

The extreme delicacy of the change in focal planes of vision is charmingly illustrated by observing with a good 1·8th the miniature of two brilliant points, the one 200 inches, the other 201 inches away in the same line. If the miniature is formed by a simple lens, theoretically the focal images of these star-disks would be separated by an interval of 2-10,000,000ths of an inch for a 1-10th lens, *perfectly* aplanatic.[2]

These disks, by their rapid change in appearance, give the most exquisite means of determining focal changes in the microscope. Thus for a focal depression of 7·10,000ths of an inch a change took place from *one* pure jet black diffraction-ring round central disk to eleven rings. The rings changed visibly for a focal depression of 100,000th of an inch.

The greatest confusion has existed regarding the terms penetration, definition, and resolution. The study of star disks miniatured by surpassingly well-corrected glasses furnishes the observer with a new order of facts upon which irrefragable conclusions can be founded.

A telescope of very fine quality should have a focus of extreme delicacy. One whose change of focus by 1-10th of an inch produces little or no effect upon the "definition" is contemptible. In the same manner the quality of microscopes may be estimated by the striking effects of a minute focal change. The planes of focal vision, it will be found, vary extremely, their interval varying according to a function of several complex factors.

The prism-heliostat already described is well represented by an Amici prism. Small bulls-eye lenses, laid with the convex surface upwards in the sunshine, present two brilliant images of the sun at a distance of 200 inches. They are of unequal brilliancy. A row of these placed in a line with the axis of the instrument,[3] and somewhat tilted, so that the star-disks may be all seen at once, will develop a series of fine microscopic effects, dependent on the corrections of the systems in use and the immersion-fluid in which the noses of the glasses are inserted (sometimes a piece of adherent glass cover intervening, as the water tends to run off). If a 1·33rd objective be employed to miniature them, and a 1·16th to observe them, extraordinary fine excellence insures a perspective almost as clear as an opera-glass. The minute double stars—which may be brought as close as we like by change of the angle of position of the instrument with the sun's azimuth—produce a variety of diffraction rings, mingling, crossing, and breaking up each other in a manner that could hardly be suspected during the telescopic observation of real double stars. Seldom can a bright landscape be attained without leaving some little outstanding colour. If that be destroyed by changing the glasses or corrections immediately, *all black objects look grey*, and the grey becomes lighter as the colour nearly vanishes. The rest of the view is charged with a thin greyish-white nebulosity, the sure indication—as already described—of residuary aberration. Just within the best focus an exquisite little dark-grey bead margined with black—much less than the primary brilliant disk (half its size)—may be discovered by close attention and a particularly well-adjusted focussing apparatus.

In observing these interesting phenomena it will be seen that larger[1] and smaller spurious disks are formed according to the curvatures of the bulls-eyes; but there is one minimum size; and these, when contiguous, illustrate diffraction spectra in a brilliantly instructive form.[2]

Upon a proper arrangement, putting the bulls-eye and the instrument nearly in a line with the sun's azimuth, a superb representation of the double star, Castor, is seen, the fainter star being that caused by internal reflection. Intensely black diffraction rings round each, and several fainter ones, fewer as the quality of instrumentation is raised. Perfect roundness can only be attained by exact coincidence of the optical axes of the system. Very slight obliquity (even half a degree) causes the rings to overlap and bulge on one side. Much obliquity gives rise to glorious curves of the three orders of the conic sections, of wondrous beauty and precision in effulgent colours.

Mercurial globules near the microscope exhibit very delicate and complex forms when similarly miniatured, as minute solar disks, in sunlight.

Experiment.—An optician's gauge comprising half a dozen lenses of standard foci 1" to 1·6"th, lying in the sunshine, miniatured star disks by reflection (see (1) in figure. Inferior objective 1½ inch examined with fine power of 1000. Two brilliant crimson disks in contact expanding within focus to an oval ring of deep crimson beads.

Experiment.—If the image of the sun be received on white paper from a small lens placed at various degrees of obliquity peculiarly beautiful forms are seen fringed with colour. When the lens is sufficiently minute these spectra exhibit to the microscope exquisitely-arranged curves in jet-black lines; circular elliptic parabolic and hyperbolic, with inexhaustible variety, according to the focal plane of vision and obliquity. Heliostatic star-disks most successfully exhibit these unique phenomena. The superiority of these phenomena to anything telescopic of the sort is insured by the absence of atmospheric disturbance within so short a distance. They are all under instantaneous control.

The limits of human vision among so many bright points are patent enough. So long as there is bright sunshine every glittering point obscures, I might say utterly effaces, the finer traceries of detail. A passing cloud, however, brings them all out with astonishing fidelity. Brilliant diffraction is thus demonstrated to be incompatible with exact portraiture. The limit is reached in brilliant sunshine by the diffraction disks obliterating the very objects which produce them. This limit is well measured by the diameter of the smaller disks seen in contact, which in white compound light generally appears by micrometric measurement to be between the 1-80,000th and 120,000th of an inch in the microscopic field.

We need not be surprised at this variation: the undulatory theory of light gives *one* size only. Yet, as the spurious disk by theory is shaded off *gradually* into the

[1] Some indications of quality in microscopes:—(1) Confused mass of spurious disks oddly arranged; (2) Beauty of rings utterly marred; (3) Very few rings definable; (4) Spectrum notched, grained, and spotted; (5) Systems of excentric rings dark and coloured, much confused; (6) An "engine-turned pattern" mottled and degenerated; (7) Achromatism and freedom from spherical aberration in all cases found incompatible. The universal presence of some residuary spherical aberration is demonstrated by several irrefragable proofs in all the finest-made modern glasses.

[2]

First conjugate focus distance	0·20005002
Second " "	0·20004977
Interval between foci	0·00000025

[3] The miniature-making objective and observing microscope are both placed horizontally.

[1] Luckily the image of the sun is very nearly the hundredth part of the focal length of the lens employed. If a bulls-eye of 1-inch focal length be employed at 200 inches, and a miniature be produced by a 1-8th, diminishing it 20 × 80, or 1600 times, the observed image of the sun would theoretically be 1-1600 × 100 = 1-1,600,000 less than one millionth of an inch. It should be noted that if the primary axial image of the sun be too large, no diffraction spectra will be developed at 200 inches, unless very deep miniature-objectives be employed.

[2] The January sun image formed by 1-inch lens is 1-106th inch.
The April " " " 1-108th inch.
The July " " " 1-109th inch.

first intensely black first ring, fainter stars telescopically show smaller disks.

But whilst a close row of spurious disks are seen to coalesce and obliterate themselves if too close, and become continuous as a thick luminous line—the necessary effect of bright diffractions—duller objects devoid of brilliance are seen of amazing minuteness of tracery.

Example.—The rungs or rounds of a ladder resting against a house half a mile off were distinctly seen when miniatured down to 1-1,000,000th of their actual size, *i.e.* considerably less than 1-1,000,000th of an inch. This feat was accomplished by an immersion 1-32nd by Seibert, which diminishes an object 30,000 inches away just about 1,000,000 times. The bane of minute microscopic research is thus seen to essentially consist of a combination of diffraction with the haze of aberration.

A blue glass evidently diminishes the diffraction phenomena; so do neutral tints. This exactly tallies with the shrinking of spurious telescopic disks during haze and sky-clouding. These facts forcibly point out the great advantages of observing in mild light. In further support of this the writer has thus effected several very difficult resolutions—in the "Ultima Thule" of microscopic investigation glare is the prolific parent of many fallacious interpretations.

These studies have encouraged the writer to continue a research into the limits of human microscopic vision. In the case of bright illuminations the limit is evidently reached at once. A minute refracting spherule thus forms a bright focal point which itself exceeds by expansion into a spurious disk, the diameter of the spherule producing it. Down to a certain size a focal image is discernible. A very interesting study is given by the solar star-disks presented by receiving the rays from the heliostat after passing through a beetle's eye placed on the field of view on the stage of the microscope.

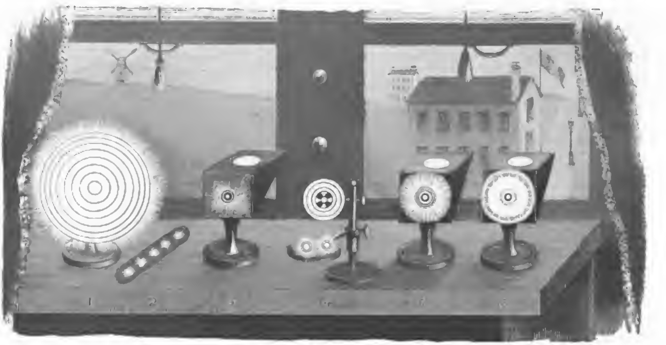


FIG. 1.—Taking the objects from left to right, a representation is given of a miniature magnified a thousand times linear and the various appearances of the heliostatic star disks with slight changes of focussing. 1. Large diffraction rings: *fundamental spectrum* given by a plane convex lens ¼ inch focal length placed on stage of microscope. Forty rings have been counted. 2. Optical gauge: various lenses showing spurious disks with minute diffraction rings similar to those on the "bulls-eye" in centre of picture. 3. Intensely clear bright star disk produced by very perfect instrumentation. 4. A cross given by imperfect glasses. 5. Larger expanding rings, the miniature or observing glasses being either under or over corrected. 6. The finest and clearest spurious disk attainable.

NOTE.—The house on the hill distinctly seen in this case of very finely corrected glasses. A miniature formed by a very fine 1-32nd gives the distant house and window nearly in the same focal plane.

Until the sun shone the window appeared miniatured in each eye. It seems curious to measure the focal length. By measuring the images this was found to be 1-1000th of an inch, giving enormous magnification for ordinary vision.[1] The solar disk, however, appeared spuriously enlarged.

More wonderful diffraction-phenomena are developed by different treatment. A half inch condenser-objective was inserted between the coleopterous eyes and the heliostat—behind or beyond the stage. The solar disks developed then appeared severely beautiful. No such wonderfully sharp black rings are even viewed telescopically. These phenomena are in order of focal classes—

1. Intensely black truly formed rings.
2. Hexagonal black patterns on a brilliant ground.
3. Three such hexagonal rings to each eye-facet.
4. Five such finished off with extremely rich Scotch plaid patterns, highly coloured.

G. W. ROYSTON-PIGOTT

[1] Their focal length was measured by selecting a well-defined object, as a red brick house, carefully measuring micrometrically a given part of it, and then measuring an image of the same thing in a known lens.

If d be the distance of the object from its image, m the size of its miniature, M the size of the object.

$$f = d \times m \div M.$$

A convenient formula for estimating the focal length of a small lens was given by me in the *Phil. Trans.* If it is found to magnify m times at a distance *between object and image d*, and if m be considerable,

$$f = \frac{d}{m+2}, \text{ more accurately } = \frac{d}{m + \frac{1}{m} + 2}.$$

NOTES

THE Delegates of the Clarendon Press will shortly publish an "Elementary Treatise on Electricity," by the late Prof. James Clerk Maxwell, edited by Mr. W. Garnett, formerly Fellow of St. John's College, Cambridge. The book was commenced about seven years ago, but its completion was prevented by the author's other engagements; so that during the last three years of his life very little was added to the work. After his death the first portion of the manuscript, on Statical Electricity, was

found in a finished state, as well as some chapters on Current Electricity. The book has been completed so as to cover the subjects included in the first volume of the larger Treatise on Electricity and Magnetism by a selection of some of the simpler articles from the last-mentioned work. As in the larger treatise, the "method of Faraday" has been followed throughout; but no knowledge of the higher mathematics has been assumed, and geometrical methods have been almost everywhere adopted. Very much of the matter contained in the work will, we are informed, be new to readers who had not the advantage of attending Prof. Maxwell's lectures at Cambridge, and the whole book bears indelibly the stamp of Prof. Maxwell's originality. It is as much unlike any other book on electricity as the "Theory of Heat" or "Matter and Motion" is unlike other books on thermodynamics or mechanics. The Clarendon Press likewise have nearly ready for publication a second edition of Prof. Clerk Maxwell's "Treatise on Electricity and Magnetism," edited by Mr. W. D. Niven, Fellow of Trinity College, Cambridge.

WE regret to announce the death, at the age of forty years, of Dr. Gustaf Linnarsson, the able palæontologist to the Swedish Geological Survey; he died, in consequence of a severe attack of disease of the chest, at the house of his brother, in the town of Sköfde. Even when at school he occupied himself with the geology and palæontology of his native province, Westrogothia. He took his degree as M.A. in 1866 with high honours, and was nominated "Docent" in Geology at the University of Upsala. In 1870 he joined the Geological Survey of Sweden as palæontologist, and since that time has worked at the classification of the Cambrian and Silurian rocks of Sweden. He has expounded his views in a numerous series of geological and palæontological papers, which all prove his accuracy and caution in drawing conclusions. The now adopted classification of the oldest Palæozoic rocks of Sweden is chiefly his work. The fossil groups in which he made his researches are the Trilobites and the Graptolites. His premature decease is a heavy loss to science, the more so as he has left behind him several important works unfinished.

THE death, resulting from a fall from a horse, is announced of Frederick Joy Pirani, lecturer on Natural Philosophy and Logic at Melbourne University. Mr. Pirani was born in Birmingham in 1850, but went to Victoria when a boy, and was there educated. He was an accomplished mathematician, and gave promise of future eminence. He was active in the promotion of science in the Colony. Mr. Pirani was an occasional contributor to our pages.

AT the last general assembly of the Swiss Alpine Club Mr. Whymper was elected honorary member, "in recognition of his having contributed, as few other travellers have done, to the exploration and renown of the Alps."

THE introductory lecture for the present Session at University College in the Faculty of Science and Arts will be given by Prof. Bonney, F.R.S., on Tuesday, October 4, at 3 p.m., in the Botanical Theatre. The subject will be "A Chapter in the Life-history of an Old University," or a sketch of the chief changes, educational and social, at Cambridge during about the last hundred years. The lecture is open to the public without tickets.

MESSRS. SAMPSON LOW AND CO. announce the following books for the forthcoming season:—"The Head Hunters of Borneo: Up the Mahakkam and Down the Barita; also Journeyings in Sumatra," by Carl Bock; "Uganda and the Egyptian Soudan: an Account of Travel in Eastern and Equatorial Africa; including a Residence of Two Years at the Court of King Mtesa, and a Description of the Slave Districts of Bahr-el-Ghazel and Darfour. With a new Map of 1200 miles in these Provinces, numerous Illustrations, and Anthropological, Meteorological, and Geographical Notes," by R. W. Felkin, F.R.G.S., and the Rev. C. T. Wilson, M.A. Oxon., F.R.G.S.; "Magyarland: A Narrative of Travels through the Snowy Carpathians, and Great Alföld of the Magyar," by a Fellow of the Carpathian Society (Diploma of 1881), and author of "The Indian Alps"; "Through Siberia": illustrated with about thirty engravings, two route maps, and photograph of the author, in fish-skin costume of the Gilyaks on the Lower Amur, by Henry Lansdell; "Nordenskjöld's Voyage around Asia and Europe: a Popular Account of the North-East Passage of the *Vega*," by Lieut. A. Hovgaard, of the Royal Danish Navy, and Member of the *Vega* Expedition; "South by East: a Descriptive Record of Four Years of Travel in the Less Known Countries and Islands of the Southern and Eastern Hemispheres," by Walter Coote; "Upolu; or, A Paradise of the Gods: being a Description of the Antiquities of the Chief Island of the Samoan Group, with Remarks on the Topography, Ethnology, and History of the Polynesian Islands in general," by the late Handley Bathurst Sterndale, edited and annotated by his brother.

AMONG Messrs. Macmillan and Co.'s announcements of forthcoming books are the following:—"Voyage of the *Vega*," by Adolf Erik Nordenskjöld (with five steel portraits, numerous illustrations, and maps); "Science and Culture, and other Essays," by Prof. Huxley, F.R.S.; Charles Kingsley's "Water Babies" (a new edition, with illustrations by Linley Sambourne); "Origines Celticæ," by Dr. Guest (with maps); "Physics of the Earth's Crust," by Rev. O. Fisher, M.A., F.G.S.; "A Course of Instruction in Zootomy (Vertebrata)," by T. Jeffrey Parker, B.Sc. Lond., Professor of Biology in the University of Otago; "Elementary Lessons in the Science of Agricultural Practice," by Prof. H. Tanner; "Mathematical Papers," by the late W. K. Clifford, M.A., F.R.S., Professor of Applied Mathematics and Mechanics at University College, London; "Text-Book of Geology," by Archibald Geikie, F.R.S., Professor of Geology, &c., in the University of Edinburgh (with illustrations); "A Treatise on Chemistry," by H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in the Victoria University, Owens College, Manchester (with illustrations): Vol. III. "The Chemistry of the Hydrocarbons and their Derivatives, or Organic Chemistry," Part I.; "Further Steps in the Principles of Agriculture," by Prof. Tanner; "The Organic Method of Studying Languages," by G. Eugène-Fasnacht; "Electricity and Magnetism," by Prof. Silvanus P. Thompson (illustrated).

MR. B. SAMUELSON, M.P., F.R.S., the chairman of the Royal Commission on Technical Instruction, has returned from a visit to Berlin, where, through the courtesy of our Ambassador, Lord Ampthill, he has secured the assistance and co-operation of the German authorities in the collection of preliminary information bearing on the subject of the inquiry. He has also made arrangements for the forthcoming visit of the Commissioners to the manufacturing districts of Westphalia. At the first meeting of the Royal Commission it was resolved that among the points to be examined should be the instruction afforded on the Continent to the proprietors and superior managers, the foremen, and the workpeople engaged in industrial pursuits, and that investigation should also be made into the connection between general and technical instruction, and the sources of the funds from which such instruction is defrayed.

THE last field meeting of the year of the Woolhope Naturalists' Field Club, we learn from the *Gardeners' Chronicle*, will be held at Hereford, on Thursday, October 6, for a foray among the funguses. There will be an exhibition of funguses in the museum room at the Free Library, and an evening meeting will

be held there on Wednesday, October 5, at 8 p.m., to name and study them. The foray will be made in Stoke Edith Park and grounds, by the kind permission of Lady Emily Foley. A meeting of the members will be held in the Woolhope Club-room at 3.45 to elect the officers for the ensuing year, and to transact the ordinary business of the Club. The dinner will take place at the Green Dragon Hotel at 4.30 p.m., and a *soirée* will be held at the house of Mr. Thomas Cam at 8 p.m., to which he kindly invites all who may be present at the meeting. After dinner, or at the evening meetings, papers will be read on the following subjects:—The progress of mycology, by Dr. Bull; fungus mimics, by M. C. Cooke, M.A., LL.D., &c.; the Herefordshire Carices, by the President of the Club; the fungi of the Dolomites, by Thomas Howse, F.L.S., &c.; the fungi which attack the wheat, by the Rev. John E. Vize, M.A.; the germination of the Uredines, and the relationship of *Æcidium berberidis* to *Puccinia graminis*, by Mr. C. B. Plowright; Protococcus, by the Rev. John E. Vize, M.A.; monstrosities in fungi, by W. Phillips, F.L.S., and a curious and abnormal cellar Polyporus will be shown by Mr. Phillips; two tomato diseases, by C. B. Plowright. The Pomona Committee of the Club have decided to hold an exhibition of apples and pears on Wednesday and Thursday, October 26 and 27, and schedules of the prizes offered may be obtained from the hon. secretaries, Woolhope Club-room, Free Library, Hereford.

ALL who have to consult or translate from French scientific and technical works will welcome Dr. F. J. Wershoven's "Technical Vocabulary, English-French, for Scientific, Technical, and Industrial Students" (Hachette and Co.). The vocabulary is arranged according to subjects, beginning with general notions on matter, and going on to force and motion, gravity, and other subjects in physics, mechanics, and chemistry, and their applications, and giving all the words and phrases in use in regular order. An ample alphabetical index renders the vocabulary easy of consultation. There is also an English-German edition.

MR. A. TREVOR CRISPIN, writing from Hyde End House, Brompton, Reading, sends us the the following information:—He is staying with his brother-in-law, Capt. Johnston, and the other morning, as usual, Capt. Johnston had had a cut fluted tumbler brought to his dressing-room filled with milk warm from the cow; into this a small quantity of rum was put, and the whole left standing. While Capt. Johnston was having his bath there was a loud noise, and on looking round he found the tumbler had parted in two, and there was an interval of four or five inches between the two parts. The fracture commences near the top (and the circumference at the top remains unbroken), at the very line of the level of the milk, the mark of which remained quite distinctly on the glass. This was the second occurrence of a precisely similar nature, the first having taken place about a month ago; but then the fracture took place some minutes after the contents of the glass had been consumed.

WE learn from the *Bulletin* of the Physical Observatory at Tiflis, that on August 24, at 11.18 p.m., there was felt in that town an earthquake which consisted of three shocks, direction from north-east to south-west. The same earthquake was felt at Gori at 11.9, at Kutais at 11.40, the direction being from east to west; at the station Kobi of the military route at 11.27, the direction being north-west to south-east; and at Gomi, a station of the Poti and Tiflis railway, where it lasted for about twenty-five seconds.

AN earthquake shock, very slight in this neighbourhood, but stronger further east, was felt in the basin of the Lake of Geneva on Friday. The earthquake was followed by a violent thunderstorm, which seemed to extend from the Alps to the Jura. Several vessels were wrecked and some lives were lost on the Lake of Brienz. A village in the district of Albula is threatened with a disaster similar to that which has just befallen Elm. The village lies at the foot of the Rothorn, a mountain in which there are several deep fissures, a part of which has been actually in movement for some time. Several engineers have already inspected the locality, and the intervention of the Federal and Cantonal Governments is demanded, in order that, if possible, measures may be taken to avert the impending peril.

A SHOCK of earthquake occurred on September 25 at Elmira, State of New York, followed by a destructive hurricane, which was, however, of short duration. On Thursday last, at noon, further shocks of earthquake alarmed the inhabitants of Orsogna, Lanciano, and Castel Frentano, where a landslip did serious damage.

THE Calendar of the Mason College, Birmingham (which has already attained considerable thickness), for the ensuing session contains a very satisfactory programme of the teaching promised by this institution. The session opens on Tuesday next with two introductory addresses, by Prof. R. H. Smith (Civil and Mechanical Engineering), and Prof. Edward Arber (English Literature).

MR. J. W. SWAN, the *Photographic News* states, has entered into an amicable arrangement with Messrs. Siemens Brothers the well-known electricians—they to employ his lamp, and he t use their apparatus. In company, they are to light up the new theatre in Beaufort Buildings with electricity, three hundred of Mr. Swan's lamps being used on the stage and in the auditorium. At the Paris Electrical Exhibition they are making a fuss over the model of a theatre lit up by electricity; in London we shal have the real thing.

M. FERRY, French Minister of Public Instruction, has authorised the opening at Montpellier of the first national college for the education of females.

THE Jamaica Government are offering great advantages to those who are inclined to embark in the cultivation of Cinchona. Suitable land is offered at a very low rate, and it seems to us that, with proper methods and selection of the right kind of plants, there is room in Jamaica for a limited number of plantations of this kind. In connection with this, Mr. D. Morris has issued a valuable series of "Hints and Suggestions for Raising Cinchona plants from seeds, and establishing Cinchona plantations."

"PROFITABLE and Economical Poultry-Keeping," by Mrs. Eliot James, is a useful little work, published by Ward, Lock, and Co.

WE have received the Reports of the Leicester and Nottingham Literary and Philosophical Societies. The former is divided into various science sections, each of which seems efficient. The Leicester Society is printing a record of its early *Transactions*, part vii. extending from June 1860 to June 1865.

A PROPOSITION has just been set on foot for an exhibition of naval and submarine engineering appliances, which is to be held in the early part of next year at the Agricultural Hall, Islington. It is intended to cover the wide field occupied in the production of machinery and mechanical contrivances employed in, or connected with, the construction and equipment of ships of all classes.

MESSRS. GRAS AND CO. of Madrid announce the publication of a Popular Illustrated Encyclopædia of Science and Art, edited by Mr. F. Gillman, mining engineer. It seems to be modelled on the German Conversations lexicon, though, to judge from the

prospectus, the work will be arranged according to subjects, and not alphabetically.

NOWHERE, according to Prof. Porter, President of Queen's College, Belfast, is the vital importance to the nation of technical education more keenly felt than amongst the merchants and manufacturers of Ulster. "Germany" (observes the same authority) "provides buildings, laboratories, and scientific apparatus on the most liberal scale. In France, Belgium, Switzerland, and the United States of America, higher technical education is making rapid strides under the fostering care of the respective Governments, aided by the generous contributions of patriotic citizens. The results of this wise liberality, while enriching those nations, are most seriously affecting the manufacturing interests of this country, and especially of Belfast and Ulster." Prof. Porter considers that in order fully to develop the latent resources of that part of Ireland we must have the means of giving young men a scientific training.

IN the *Bulletin* of the Essex (U.S.) Institute for April, May, and June is a paper by the Rev. G. F. Wright, on the Glacial Phenomena of North America, and their Relation to the question of Man's Antiquity in the Valley of the Delaware.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus*) from India, presented respectively by Mr. J. Thompson and Mr. C. Green; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. W. Thomson; a Banded Ichneumon (*Herpestes fasciatus*) from West Africa, presented by Mr. W. Cubitt; two Common Otters (*Lutra vulgaris*) from Ross-shire, N.B., presented by Mr. H. Mitchell; a Black-crested Eagle (*Lophoaëtus occipitalis*) from Africa, presented by Mr. E. A. Harland; a Brush Turkey (*Talegalla lathami*) from Australia, presented by Capt. F. M. Burke, s.s. *Cheybassa*; a Red-legged Partridge (*Caccabis rufa*), European, presented by Mr. J. E. Clayton; a Common Cuckoo (*Cuculus canorus*), British, presented by Master Alfred Beart; an American Black Bear (*Ursus americanus*) from North America, deposited; four Zebra Waxbills (*Estrelda subflava*) from Africa, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), three Undulated Grass Parrakeets (*Melopsittacus undulatus*), bred in the Gardens. The additions to the Insectarium include larvæ of the Comma Butterfly (*Vanessa C. album*), scarce Swallow-tail Butterfly (*Papilio podalirius*), and Privet Hawk-Moth (*Sphinx ligustri*). Also imagos of *Ranatra linearis*, and a specimen of *Attacus atlas* reared from larvæ hatched in the House.

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF MARS.—The approaching opposition of this planet does not hold out much probability of satisfactory observations of the satellites except with the larger instruments, though in European latitudes the meridian altitude, which is an element in the case, will be considerable. Taking Prof. Asaph Hall's unit for brightness in 1877, viz. that on October 1, when the outer satellite was seen with the 9·6-inch equatorial of the Naval Observatory, Washington, we find the maximum brightness at the next opposition will be represented by 0·4, which is a less value than corresponds to the last date of observation with the 26-inch refractor at the same observatory. It may be remembered that Mr. Common observed *Deimos* on the morning of September 2, 1879, without much difficulty with his reflector of 3-feet aperture, when the degree of brightness in terms of Prof. Hall's unit was 0·50; at the last Washington observation in 1879 it was 0·52. The earth being only about 10° from the line of nodes of the satellites' orbits at the opposition in December next, their apparent paths are reduced almost to straight lines. The longitude of the ascending node of *Deimos* is 88°.

THE SATELLITES OF SATURN.—Mr. Marth has again prepared ephemerides of the five inner satellites of Saturn, which have been published in the *Astronomische Nachrichten*; he appends differences of right ascension between the outer satellite, *Japetus*, and the centre of Saturn, but he does not attack *Hyperion*. Preceding the ephemerides are auxiliary quantities for every fifth noon at Greenwich, by means of which the positions of the five inner satellites may be determined for any time required from the formulæ—

$$s \sin (p - P) = a \sin (l - L)$$
$$s \cos (p - P) = b \cos (l - L).$$

Here p is the angle of position with reference to the planet's centre, and s the distance therefrom; the values of $(l - L)$ and of the semi-axis a and b are tabulated for each satellite, as well as the angle P, which is applicable to all five. The process is simple enough to any one initiated in such calculations, but as there may be observers to whom they are unfamiliar, an example may not be out of place here.

Let it be required to determine from Mr. Marth's tables the angle of position and distance of *Mimas*, at Greenwich midnight, on October 1. We have then—

$(l - L)$ Oct. 1d. 0h. ...	287°·60	log a	+ 1·4856
Motion in 12h.	191°·04	sin $(l - L)$	+ 9·9433
$l - L$	118°·64	Call the sum A ...	+ 1·4289
From the tables P =	359°·58	log b	− 1·0128
a =	30″·59	cos $(l - L)$	− 9·6806
b =	− 10″·30		
		Call the sum B ...	+ 0·6934
		$\frac{A}{B} = \tan (p - P)$...	+ 0·7355
		$p - P$	79°·58
		Add p	359°·58
		p	79°·16
		$\sin (p - P)$	+ 9·9928
		$\frac{A}{\sin (p - P)}$	1·4361
		s	27″·30

THE FOURTH COMET OF 1874.—Dr. Holetschek, of the Observatory of Vienna, has investigated definitive elements of this comet, which was discovered on August 19, 1874, by M. Coggia at Marseilles. He uses four normal places: for August 21, September 18, October 10, and November 9. For the later normals we presume he will have made use of a fine series of observations made with Col. Tomline's 10-inch refractor at the Orwell Park Observatory, near Ipswich, by Mr. J. I. Plummer, which extends, we believe, considerably beyond observations published so far from other observatories. With Col. Tomline's refractor the comet was followed until the middle of November, and great care appears to have been taken with the observations and reductions. They form one of the very best series that has obtained for some years at an English observatory. Dr. Holetschek infers that the comet was moving in an elliptic orbit with a period of about 300 years. His orbit is as follows:—

Perihelion Passage, 1874, July 17·68463, Greenwich M.T.

Longitude of perihelion	5 26 13	M. Eq. 1874·0.
" ascending node ...	215 50 47	
Inclination	34 7 54	
Excentricity	0·9622257	
Log. perihelion distance	0·227275	
Semi-axis major	44·671	
Period of revolution	298·6 years.	

The comet with these elements might approach pretty near to the planet Uranus near the ascending node, but we might rather look to an encounter with Mars at the opposite node as the cause of ellipticity of orbit, the radii-vectores being there identical, with but small difference of latitude.

ANCIENT STAR-POSITIONS.—In the *Vierteljahrsschrift der Astronomische Gesellschaft*, 16 Jahrgang, Dr. O. Danckwortt has tabulated the positions of forty-six fundamental stars of the *Berliner Jahrbuch* for the commencement of each century from − 2000 to + 1800. He adopts Leverrier's precession constants for 1850, and takes account of the proper motions. The tables are preceded by a discussion of formulæ and comparison of constants which will be of service to any one who may have

occasion to carry back to a distant epoch the place of a star not included in Dr. Danckwortt's list. The values of the arcs A, A', and θ, are given for the beginning of every century within the above period. The place of the present Pole-star for the year − 2000 is found to be R.A. 335° 10' 0", Decl. +67° 34' 30".

We may mention that the formulæ for the reduction of the places of stars to distant epochs are investigated in a very interesting paper by Prof. Schjellerup, which Dr. Copeland and Mr. Dreyer have translated in No. 2 of *Copernicus*, and which the reader who desires to acquaint himself with the application of the rigorous method of taking account of precession should consult.

PHYSICAL NOTES

The conducting power of metals for heat and electricity has lately occupied several physicists. Prof. Lorenz of Copenhagen, employing two methods (*Wied. Ann.*, Nos. 7 and 8), gets these results: (1) for the better conducting metals, a confirmation of Wiedemann and Franz's law, that the ratio of the two conductivities, both at 0° and at 100° is nearly constant; in the inferior conductors it increases much with decreasing conductivity; (2) in all metals except iron, the ratio $\frac{k_{100}}{\kappa_{100}} : \frac{k_0}{\kappa_0}$ is constant, and approximately equal to 1·367 (k and κ denoting the conductivities for heat and electricity respectively). Thus, for absolute temperature $T, \frac{k}{\kappa} = T \times$ constant.

A "medical hydrotelephone," contrived by Prof. Sabatucci (*Riv. Sci. Ind.*) is of the following nature:—Two lead cylinders (5 ctm. in diameter and ½ ctm. thick) are closed each with two very fine iron laminæ. To the anterior part of each is fitted a wooden mouthpiece (like that of a Bell telephone) connected to a caoutchouc tube, through which one may hear at a distance. The posterior part has a very sensitive electromagnet communicating with a microphone and battery. One tube is applied to either ear. Words or sounds produced before the microphone, and heard but faintly, are rendered intense and distinct by introducing liquid into the cylinders (the less dense the liquid the better). Two sounds may be compared, and their intensity exactly measured, by varying the quantity of the liquid and noting the effects through the tubes. Various applications of the apparatus, in clinical medicine especially, are looked for.

The effects of lightning on trees placed near a telegraph wire are forcibly illustrated by phenomena lately observed by M. Montigny (*Bull. Belg. Acad.*, No. 7) on a portion of the road from Rochefort to Dinant; which runs from east to west, first on level ground and between poplars for about 1500 metres, then rises gradually 61 metres through woods to a wooded plateau some 200 metres in extent, then comes down to another plain. Of the poplars bordering the road on either side those on the north side, next the telegraph line, have largely suffered, 80 out of 500 having been struck, or about a sixth; those on the other side are very rarely struck. The plain presents only one case, and that doubtful. The instances multiply with increased elevation, and in the wooded plateau on the top reach a maximum (64 per cent.). The most violent discharges have been on the west side of the plateau and rising ground, which is generally first reached by the storms, and the injuries to trees are mostly opposite and under the level of the wire. M. Montigny supposes that while the wire is strongly electrified by induction, the lightning does not strike it, on account of its relative insulation, but strikes the neighbouring poplars directly, which, wet with rain, afford an easier passage for the electric fluid to the ground.

M. Pictet has examined seven varieties of steel (chiefly from a Sheffield and a Vienna house) with regard to magnetic power (*Arch. des Sciences*, August 15). This power he finds to depend on the presence of carbon in the iron, and the aggregation of these substances. One of the two steels giving the best results had ⅞th per cent. of carbon. Samples with 1½ and 1⅛th per cent. were inferior. German steel of poor quality (for springs) yet made a good magnet; it had little homogeneity, and consisted of an intimate mixture of iron, and iron cemented with a small proportion of carbon. A too-small proportion of carbon suppresses or weakens the remanent magnetism. M. Pictet also finds that the increase of magnetic power in a magnet through the mere presence of the armature in contact is a certain fact for some qualities of steel, but not for all. The first magnetic passes develop nearly the whole of the remanent magnetism in all artificial magnets. Detachment of the armature by the dynamometer seemed to have no action on the magnetic power, only the slipping of the armature when near rupture must be avoided.

An experimental inquiry into the production of tones by passage of gases through slits is described by Herr Kohlrausch in *Wiedemann's Annalen* (No. 8). The principal results are these: (1) For all widths of slit between 0·2 mm. and 1 mm. and all densities of gas employed, the pitch n may be represented as linear function of the velocity of outflow u by the formula $n = k(u - u_0)$, where u_0 and k are constant for a given width of slit and variety of gas; (2) with increasing width of slit (0·2 mm. to 1 mm.) u_0 decreases, k increases; (3) u_0 and k (for air) are as good as independent of the thickness of the slit (*i.e.* the thickness of the brass plates forming it), from 1 mm. to 9 mm.; (4) with increasing density of gas (0·7 to 1·5) u_0 generally decreases, k increases, *i.e.* the tones, *ceteris paribus*, become higher; (5) u_0 and k depend in no small measure on other properties of gases besides density; (6) starting from wide slits with constant velocity of outflow, the pitch increases pretty uniformly with narrowing of the slit, reaches a maximum with widths between 0·35 mm. and 0·27 mm.—with thicker slits, smaller pressures, and less dense gases, sooner than in the opposite cases—and then decreases more and more quickly with the width of slit; (7) this maximum pitch characteristic for each velocity of outflow occurs with a greater width of slit, the less the velocity. From a comparison with Strouhal's experiments on the tones excited by motion of cylindrical bodies in air, the author concludes that the production of slit-tones is to be referred to like causes to those of wire-tones.

An electrophotometer recently described by Dr. Nachs (*La Natura*, August 1) has the following arrangement:—A wooden case opening on one side and above is divided into two compartments, an upper and a lower. The lower contains in the inner part a dry pile of 200 elements, and in the outer a Jacobi rheostat, the cylinder of which has thirty windings (the number introduced into circuit by turning a handle is precisely indicated). The upper compartment has interiorly a galvanometer, and exteriorly an adjustable truncated cone with inner surface blackened, and within it a small selenium cell similar to those used by Bell and Tainter, to receive the light. This cell and the three other electrical instruments are connected by wire. The mode of action will be readily comprehended.

Mr. Milne has observed (*Zeits. f. Kryst.*) that if a suspended quartz ball be allowed to impinge in various ways upon a fixed ball of lime spar, or if the ball of lime spar be placed on a billiard table and the distance observed to which it is driven by the shock, in the former case the quartz ball rebounds furthest, and in the latter the lime spar ball is driven furthest, when the impulse is in the direction of the (crystalline) axes.

In a paper to the Bremen Society of Natural Science, Herr Müller Erzbach describes experiments in which he sought to ascertain the relative tension of aqueous vapour over saturated solutions of different hygroscopic substances, the inclosed air being submitted for long periods to the action of these. 1. For saturated solutions one finds in the same series, soda, potash, chloride of calcium, an increase in the vapour-tensions, and a decrease in the contractions. 2. Phosphoric acid anhydride, concentrated sulphuric acid, and hydrate of potash deprived of water, present no essential difference in attraction of water. 3. Caustic soda and chloride of calcium, with small proportion of water, differ little in attraction of water, but they do not bind it so firmly as phosphoric acid or hydrate of potash. 4. Hydrate of soda can be completely deprived of water by inclosure with hydrate of potash. 5. The difference in tension of aqueous vapour over the anhydride of phosphoric acid and chloride of calcium nearly without water amounts to only a small fraction of a millimetre of mercury.

The hypothesis that the luminiferous ether is at rest and the earth moves through it, has been lately put to experimental test by Mr. Michaelson of the U.S. Navy (*Amer. Journ. of Science*, August). Two pencils of light which have travelled over paths at right angles (one path being in the direction of the earth's motion) are permitted to interfere. On rotation of the apparatus 90° a measurable displacement, estimated at about one-tenth of the distance between the fringes, might be looked for (the author considered) if the hypothesis of a stationary ether were correct. The apparatus was first tried in the Physical Institute

in Berlin, then, for greater quiet, in the Astrophysicalisches Observatorium at Potsdam (it was very sensitive to vibrations). The interpretation of the results is that there is no displacement of the interference bands, and the hypothesis (which is presupposed in the commonly accepted explanation of aberration) is inferred to be erroneous.

THE expansion of solid sulphur has been studied by S. Scichilone of Palermo, in the case of natural crystals, and of such as had been heated after fusion to 140° and 240°. The tables (*Wied. Beibl.*, No. 7) show that the expansion depends essentially on the previous heating, inasmuch as different modifications of sulphur are thereby formed. In the first case we have the octahedral sulphur, in another the monoclinic, and in the third a mixture of the latter with that which is not dissolved in sulphide of carbon. In the first two cases the curve representing the volumes as a function of the temperature turns its convex side, in the third its concave side, to the axis of temperature.

GEOLOGICAL NOTES

THE recent geological exploration of the shores of Lake Baikal by M. Tchersky has been fruitful of important results for science. The rocks of which the mountains on the western shore are built up belong to three different ages: pre-Silurian (probably Laurentian), Silurian, and Jurassic. The Laurentian rocks afford several foldings running north east, which enclosed basins of Silurian and Jurassic seas; as to recent formations they are only freshwater ones, and belong to the Tertiary and Post-pliocene; these last, which are remains of several smaller lakes, are found at a great height above the level of Lake Baikal. M. Tchersky's geological researches confirm the suggestion which was made several years ago on geographical grounds by M. Kropotkin, namely, that, like several other lakes, Lake Baikal consists of two longitudinal valleys, connected together in the middle part of the actual basin.

THE important coal-basin of the Don province of Russia has not hitherto been explored with accuracy. During last summer M. Domger undertook a thorough exploration of this interesting geological region, and, as we learn from a communication he has made at the December meeting of the St. Petersburg Mineralogical Society, his researches have led to important discoveries. Thus he discovered a great variety of crystalline rocks, porphyries, &c., and volcanic ores, within the coal-measures, which discovery thus extends the crystalline island of Southern Russia far eastwards as a strip about 500 miles long, which runs from north-west to south-east. At the same meeting the Society awarded its gold medal to M. Romanoffsky for his researches in Turkestan.

M. DOKOUCHAIEFF's researches on the soils of Russia seem to establish a very interesting fact as to the distribution of black-earth. The typical black-earth occupies an elongated zone directed from south-west to north-east from Kishineff, through Kharkoff, Voronesh, Simbirsk, to Bougoulina in the province of Oufa; in this zone the black-earth contains from 7 to 12 per cent. of humus, and from both sides to north-west and south-east it is accompanied by two other elongated zones, where the black-earth contains only 5 to 7 per cent. of humus, whilst the other parts of Russia afford only sporadic spots of black-earth.

M. MOUSHKETOFF's paper on the glacier of Zerafshan, which appeared in a recent number of the *Izvestia* of the Russian Geographical Society, contains further details about the expedition which has explored the glacier throughout its whole length, from its lower extremity to the sixteen miles distant and 13,800 feet high pass of Matcha, whence another glacier, that of the Zardala river, descends on the north-eastern slope for 2200 feet, by a series of mighty icefalls. The paper is accompanied with a pretty map which shows this grand ice-world, where no less than thirteen secondary glaciers are feeding the ice-stream of the Zerafshan. We notice in this paper that formerly the Zerafshan glacier descended far lower than now. M. Moushketoff says that thirty-three miles below its actual extremity, namely, at the village Diaminor, there is a beautiful terminal-moraine which crosses the valley and unites with three longitudinal moraines. Immense boulders, thirty-five and forty feet in diameter, and consisting of granite, syenite, and gabbro, cover the whole space between these old moraines and the actual ones, so that there cannot be the least doubt as to the glacier having descended for at least thirty-three miles lower than now. But when we see how the composition of the drift changes lower down in the valley, the loess, which is the wealth of the inhabitants in the lower countries, changing into mighty conglomerates with immense boulders, we are much inclined to think, that the former glaciers descended yet far lower. Therefore we observe with some regret that M. Moushketoff gives too little attention to the diluvial formations of the Upper Zerafshan and to their relations to the loess.

GEOGRAPHICAL NOTES

WHEN Humboldt determined for the first time the average heights of continents, he could not, because of the want of data, determine that of Africa. Now Dr. Chavanne publishes, in the *Proceedings* of the Geographical Society of Vienna (vol. xxiv.), an elaborate paper on this subject, accompanied with a hypsometrical map of the African continent, which is based on no less than 8000 hypsometrical measurements. After a thorough discussion of the relative value of various measurements, Dr. Chavanne discusses the average heights of separate parts of Africa, and by how much each of them would raise the continent if its mass were distributed over the whole of the surface of Africa. He finds that the Atlas Mountains, if distributed over the surface of Africa, would produce an elevation of 26 metres; the Sahara, 122 metres; the plateaux of Soudan, 85 metres; those of Central and South Africa, 129 metres; and so on; and he accepts for the average height of the whole of the continent no less than 661·8 metres (with a probable error of ± 21 metres), which figure he considers to be rather below the truth. This very high figure obviously is the result of the very great extension of high plateaux, which we do not find to such an extent even in Asia.

IN the Annual Report of the Surveyor General of India, which, though it has been printed for months, has only just been allowed to appear, prominence is given, under the heading of Trans-Frontier Exploration, to an attempt to determine the position of the head-waters of the Irrawaddy by Capt. J. E. Sandeman, through the agency of a native surveyor whom he had trained in imitation of the late Col. T. G. Montgomerie's renowned staff in India. This surveyor alleges that he ascended the river to Mogung-poon, near the point where it divides into two great branches, the Malee and Mehka. The surveyor, we believe, gives as an explanation of his not having prosecuted his journey to a more successful termination, that he was attacked and robbed by wild tribes; but we hear privately that persons in Burma, well qualified to form an opinion, attach little credit to any of the surveyor's statements, and we fear, therefore, that the position of the head-waters of the Irrawaddy is still an unsolved problem.

THE Geographical Society of the Pacific is the title of a new Society formed at San Francisco. The Secretary is C. Mitchell Grant, F.R.G.S. The objects of the Society, it is stated, are to encourage geographical exploration and discovery; to investigate and disseminate geographical information by discussion, lectures, and publications; to establish in the chief city of the Pacific States, for the benefit of commerce, navigation, and the industrial and material interests of the Pacific Slope, a place where the means will be afforded of obtaining accurate information, not only of the countries bordering on the Pacific Ocean, but of every part of the habitable globe; to accumulate a library of the best books on geography, history, and statistics; to make a collection of the most recent maps and charts, especially those which relate to the Pacific coasts, the islands of the Pacific, and the Pacific Ocean; and to enter into correspondence with scientific and learned societies whose objects include or sympathise with geography. The Society will publish a *Bulletin* and an annual *Journal*.

WE learn from the Annual Report for 1880–1881 of the Swiss correspondent of the Geographical Society of Vienna that the following geodetical and geological work was done in Switzerland:—The Geodetical Commission has published the seventh fascicule of the "Nivellement de Précision de la Suisse," which contains the measurements done during the years 1877 to 1879 on the lines of Monte Cenero to Chiasso, Reichenau to Sargans and Andermatt, and Süss to Landquart and Chiavenna, uniting thus the Swiss measurements with the Italian ones. The Geological Commission publishes the fourth volume of its new series, containing the important work, by Dr. Balzer,

on the zone of contact between the gneiss and the limestones of the Berne Oberland; another most interesting work, on the distribution of heat in the interior of the St. Gothard Tunnel, is pursued by Dr. Stapff, and a preliminary notice about it, with maps, has just appeared in the Quarterly Reports of the Federal Council. (vol. vii.). It shows that the temperature of rocks increases to a great degree to the interior of the tunnel, being only 17° Celsius and 19°·7 at the southern and northern extremities of the tunnel, and as high as 30°·8 in the middle parts of it, the decrease at the outer ends being attributed by Dr. Stapff to the cooling influence of the water which circulates in the rocks. As to the geological information collected by Dr. Stapff during his work in the tunnel, which appears complete (with sixty sheets of maps and profiles) in the Reports of the Federal Council, a short *résumé* of the whole has already appeared in a separate fascicule of these Reports, with a geological outline of the tunnel. We notice also in this branch a valuable paper, by M. Salis, on the erosion of the Nolla River, tributary of the Rhine, which has appeared in the engineering paper, *Die Eisenbahn*, published at Zurich.

THE various races which inhabit Austria are studied by Dr. Goehlert in the last number of the *Proceedings* of the Geographical Society at Vienna (vol. xxiv.), with respect to the length of the body. After having collected more than one and a half million of such measurements, which were done on recruits during the years 1870 to 1873, Dr. Goehlert has dressed a map in which he has shown the average height of young men, twenty to twenty-three years old, in Austria. The Dalmatians are the tallest; next to them come the Serbo-Croats and Slovenes, and then the Germans and the Czechs; further down come the Ruthenes and Roumanians, and the smaller ones are the Magyars and Poles, especially the Mazours. But there are also two or three distinct average heights among the Germans, the Slaves, and the Magyars, those of middle Hungary, between the Danube and the Theiss rivers, being far taller than those of the flat country on the left bank of the Theiss. It is most probable, as M. Broca has shown with regard to France, that these notable differences of height among the same race show that there were two, or more, different branches which constituted what we consider now as a single race. As to the supposed decrease of height observed in France, Dr. Goehlert supposes that in Bohemia, which has furnished during this century no less than 600,000 men to the Austrian armies, the decrease of average height can be estimated at little under 39 millimetres during the last hundred years, this decrease being due to the continuous taking away of tall men from the country. He shows also that, the standard height being the same for all provinces of Austria, the provinces where men are taller suffer proportionately more from recruiting.

THE seventeenth meeting of the Swiss Alpine Club was opened at Basel on September 10. The Annual Report shows that since its foundation the Club has built thirty-one huts, or *refuges* for climbers. The Club has also endeavoured to give a certain instruction to guides, and during this year an insurance society has been instituted for them. As to its publications, it has published sixteen volumes of year-books, which contain plenty of valuable information on the Swiss Alps, and publishes two papers, the *Écho des Alpes* and the *Neue Alpenpost*, which have contributed much to the development of Alpine literature. At its last meeting Mr. Ed. Whymper and the meteorologist, Prof. Hamen, were elected Honorary Members.

THAT part of the Ala-taou Mountains which is situated north-east from Tashkent, at the sources of the Arys, Talas, and Pskem Rivers, and which remained until now quite unknown, is described in the *Izvestia* of the Russian Geographical Society (vol. xvii. fascicule 3) by Col. Ivanoff. It is a very complex system of mountains, from 10,000 to 16,000 feet high, covered with mighty glaciers. The upper clefts have still conserved a good deal of forests, and the high Alpine pasturages are the grazing ground for the numerous herds of Kirghizes, as well as for the great species of *Ovis*, common to Thian-Shan. Col. Ivanoff has found numerous proofs that formerly the glaciers had a greater extension than now, and that they formed in the valley of the Maydan-tal River a mighty glacier which descended as low as 7000 feet, but he did not discover traces of a general glaciation.

HERR ERNST MARNO gives, in the last fascicule of the *Memoirs* of the Geographical Society of Vienna (vol. xxiv. Nos. 6, 7, 8, and 9), an interesting description of his expedition for the destruction of the *setts* of the Nile, that is, of the great grass-islands which are formed during the inundations of the steppes watered by the Bahr-el-Gebel and the Bahr-el-Abiad. The accumulation of grass which is driven away during the inundations constitutes, as is known, wide grass-islands, or *setts*, which bar up the river, and, when not cut through for several years, gradually increase by fresh grass and slime, and soon constitute true floating islands twelve and fifteen feet thick, which soon reach even the bottom of the river. It is with the greatest difficulty that Marno's steamer cut passages through these islands and destroyed the smaller ones.

WE see with pleasure that the Austrian Tourists' Club, which numbers as many as 300 members, has begun to publish fortnightly a *Tourists' Newspaper*, richly illustrated, which has as contributors many well-known scientific writers.

IN the *Monatsschrift für den Orient* for September, Herr von Schweiger-Lerchenfeld has a long article full of valuable information on Tripolitania, *à propos* of recent doings in North Africa. There is also an interesting letter from Ernst Marno on the Sudan.

IN the *Bulletin* of the Antwerp Geographical Society (tome vi. 3e fasc.) M. L. Delavaud has brought together a number of valuable notes on the climate of Africa, interesting both from a scientific and a practical point of view.

THE last number of the *Izvestia* of the Russian Geographical Society contains papers, by M. Maeff, on the roads leading from Karshi to the Amu-daria River, and on the valleys of Vaksh and Kafirnahan; by M. Ivanoff on the upper parts of the Talas Alataou, and a map showing M. Mikluho-Maclay's travels in the Melanesian Islands.

THE eighth volume of the *Memoirs* of the Russian Geographical Society, for the section of ethnography, contains several valuable papers on the middle parts of the valley of Zerafshan, on the basin of Lob-nor, on the valley of Ferghana, on the Bekdons Shahrisabs, on the journey of Jenkinson to Khiva in 1559, on the Khiku-nor, and on the customs of the Tartars of Kazan.

IN a pamphlet entitled "Geography" Messrs. Ramsey, Millett, and Hudson have reprinted, from the Kansas City *Review of Science and Industry*, an interesting collection of official documents relating to United States Arctic colonisation and exploration in 1881. There are now no less than six expeditions in progress under Government control, which are divisible into two classes, one comprising those sent out for purposes of exploration and scientific research and the other those whose object is of a humanitarian nature. To the former class belong the *Jeannette*, Lady Franklin Bay, and Point Barrow expeditions, while the latter includes the *Rodgers*, *Alliance*, and *Corwin*, all chiefly engaged in searching for the *Jeannette* and missing whale-ships.

The just published *Bulletin* of the Belgian Geographical Society includes a paper by Capt. Verstraete on the great lakes of inter-tropical Africa from the fifteenth to the nineteenth century. There are also maps of Borneo, the new northern frontier of Greece, &c., which exhibit considerable roughness of execution.

ECONOMICS AND STATISTICS, VIEWED FROM THE STANDPOINT OF THE PRELIMINARY SCIENCES[1]

THE object of the present paper is to show the relation of the preliminary sciences to statistics and economics, and to attempt to make the transition from the former studies to the latter simple and attractive to the scientific man. This must evidently be done by constructing a classification of social knowledge avoiding all immediate reference to practice. That such a classification does not at present exist cannot be better evidenced than by Mr. Baden-Powell, who has kindly drawn my attention to the conclusion of his paper, read on the previous day, "On Protection in Young Communities," in which he states the difficulties he has encountered in many departments of his researches because of the different methods of classification adopted in otherwise excellent statistical records, and insists that "uniformity in the method of registering statistical facts is of the utmost importance to comparative investigations," so that

[1] Abstract of a paper read before Section F of the British Association, 1881, by P. Geddes, F.R.S.E.

"it would be of great importance if such uniformity could be secured in the future."

A survey of the statistical records of various countries, whether under the same or different political rule, shows the most extreme discord, while a detailed examination of the schemes which have been as yet proposed by statisticians results in their rejection as unscientific—the very latest of such schemes being curiously analogous to the very earliest of classificatory attempts in biology. A criticism of the innumerable definitions of statistics, some of which claim statistics as a science, others as a method, others as both or neither, leads to the acceptance of the view recently sustained by Hooper (J.S.S. 1881), that statistics is simply a quantitative record of the observed facts or relations in any branch of science,—a definition which may conveniently be extended diagrammatically as follows:—

RECORD OF FACTS AT GIVEN TIME.

Qualitative	Quantitative			
	Numerical	Linear	Plane	Solid
		Graphic		
Statements.	Statistics.			

By combining such successive records we obtain history, and statistics and history are thus seen to be, within certain limits, the common property of the sciences—all save logic, where there are no ideas of quantity, being statistical, and all save logic, mathematics, physics, and chemistry, where conditions and properties are constant, being historical; the current notions of statistics and history as distinct sciences, or as distinct scientific methods, being therefore entirely erroneous.

The application of the above diagrammatic definition of statistics to all the sciences clearly illustrates the continual progress from qualitative to quantitative knowledge which goes on in each, and the increase of definiteness which quantitative knowledge constantly tends to assume. Thus while the common name of a chemical compound, say sulphate of iron, expresses only a qualitative relation, its ordinary chemical formula, $FeSO_4$, reaches the numerical state, and its graphic and glyptic formulæ are respectively the plane and solid representations of the same relation of quantity. So, too, the astronomer has his star maps and orrery, the biologist his figures and diagrams, while the sociologist so often requires similar aid that the French Government has recently established a Bureau de Statistique Graphique. So by piling up successive graphical representations of statistical observations, a solid historical model might often be constructed. A geologist, for instance, by piling map upon map of a given island at successive times (the margin being, of course, cut away) would thus construct a solid model which would graphically exhibit the changes of increase and decrease, local and general, throughout the entire period.

But what are the desiderata of a system of classification? It must be natural, not artificial,—capable of complete generalisation and specialisation, universal in application, simple of understanding, and convenient in use. Immeasurably the highest example of such a classification is presented by botany and zoology, and it is therefore the biologist, not the logician or the mathematician, and still less the metaphysician or the political economist, whose training prepares him to undertake the still vaster task of classifying the infinitely numerous and varied phenomena of society.

Taking an actual compendium of miscellaneous statistics, we have first then to separate out in order the actual statistics of the preliminary sciences, mathematical, physical, chemical, astronomical, geological, &c., and leave these to their special cultivators. Social statistics now alone remain, and to classify these naturally we must ascertain the fundamental scientific truths respecting society. Just as the biologist is accustomed to classify man along with inferior organisms, and to trace the fundamental resemblances in structure and function which his organisation presents to theirs, so he must inquire wherein human society resembles the societies formed by the lower animals, the more so as no one disputes that these fall strictly within his province (see Huxley, "Anat. Invert." p. 1). As the term indeed assumes, some general truths must be common to societies of *Formica*, *Apis*, *Castor*, and *Homo* alike, and this must therefore underlie our classification of social facts.

First, then, a society obviously exists within certain limits of space and time. It consists of living organisms. Again, these modify surrounding nature, primarily by seizing part of its matter and energy. Again, they apply this matter and energy to the maintenance of their life; *i.e.* the support of their physiological functions. Finally, these organisms are modified by their environment.

These *sociological axioms*, as we may henceforth term them, at once enable us to classify out the facts relating to each and every society as follows: (A) those relating to the limits of time and space occupied by the society; (B) those relating to the matter and energy utilised by the society; (C) those relating to the organisms composing the society; (D) those relating to the application of the utilised matter and energy by the given society; (E) those relating to the results of the preceding conditions upon the organisms. These considerations were developed in a series of tables exhibited as diagrams upon the wall; and an extremely condensed summary is given on the following page.

These tables, which may be read either separately, in horizontal, or in vertical series (the left-hand vertical series being viewed as entries on the creditor side of a balance sheet), were developed into detail, and shown to be applicable to all societies alike, whether animal or human, civilised or savage, and to include the facts (A) of political geography; (B) of economic physics, geology, botany, and zoology, of technology and the fine arts, transport and commerce, in short, of the economics of production; (C) of anthropology, together with the Registrar-General's reports and the census; (D) of distribution and consumption; (E) of a large body of observations made by physicians, biologists, educationists, and philanthropists dealing with the modifications of the organisms by their environment. These tables, therefore, while endeavouring to classify all known statistics, attempt nothing short of an organisation of the social sciences into a more definite and coherent body of knowledge than they have formed heretofore.

It was then shown that while the above outlined considerations are in entire harmony with the economic labours of the geographer, the physicist, the biologist, the anthropologist, and the demographer, they furnish grounds for a destructive criticism of the existent systems of political economy, in so far as these pretend to intellectual completeness; the best proof of this being their applicability to utilise and reconcile the labours of each and all the contending schools, statistical and economical alike.[1]

The application of the conceptions of physics and biology to the interpretation of social facts was then alluded to, interest on money being taken as an example. This was shown to depend neither upon compensation for risk of loss, reward for abstinence, nor wages of superintendence, as asserted by economists (Fawcett, "Pol. Econ."), nor yet to be simply an abstraction from the wealth of other members of the community as supposed by its opponents, but usually to arise, in modern times at least, from the appropriation of the matter and energy of nature, and generally speaking, from the exploitation of the sun.

But the severest test which can be imagined is to apply the ideas of the present paper to the study and classification of all the other papers read during the meeting of the section, since no preparation within such short limits of time has been possible. The first, "On Societies of Commercial Geography," relates to the study of territory (Tables A); the second, "Corn or Cattle," in the language of Table B, I., inquires whether the exploitation of plants or animals be more profitable in the given society; the third, "Report of Committee on Teaching," &c., relates to the state and treatment of cerebral functions of certain organisms in the given society; the fourth, "On Agricultural Statistics and Prospects," is at once classified with the second; the fifth, "A General Banking Law," &c., relates to the co-ordination of commerce (Table B, II., Movement), and so on.

So, too, with the anthropological papers:—the first, "Report on Caves and Kitchen-Middens," is a statement of facts respecting production, partition, and consumption in some ancient community, while the next, "On the Stature of the Inhabitants of Hungary," supplies facts to be classified under Hungary, organisms, structure. Nor are the other sections behind in furnishing

[1] The preceding propositions and tables are abridged from the author's paper "On the Classification of Statistics and its Results" ("Proc. Roy. Soc. Edin." 1881).

CLASSIFICATION OF STATISTICS.

Society .... Date ....

A.—Territory. I. Quantitative.

Existent at last recorded time.	Increase.	
	By social agency.	By geologic agency.

Territory. II. Qualitative.

Unused.	Used.	
	Unspecialised.	Specialised.

Territory. III. Decrease.

By social agency.	By geologic agency.

B.—Production.

I. a. Sources of energy in territory.

Primitive chemical affinity.	Earth's internal heat.	Earth's rotation.	Solar radiation.		
			Kinetic.	Potential.	
				Earth's crust.	Organisms.

β. Sources of matter used for other properties.

Mineral.	Vegetable.	Animal.

II. Development of ultimate products.

Energy.	Exploitation, manufacture, and movement (trade and transport).	Ultimate products.
See Table I. a.		
Matter.		
See Table I. β.		

III. Loss. (Premature dissipation of energy and disintegration of matter.)

Agency.	In
Physical, Biological, Social.	Exploitation, manufacture, movement, remedial effort, &c.

C.—Organisms. I. Quantitative.

Existent at last recorded time.	Increase.	
	Immigration.	Birth.

Organisms. II. Qualitative.

Biological.		Social.
Structure.	Function.	Mutual relations.

Organisms. III. Decrease.

Emigration.	Death.

C.—Organisms. Occupations. I. (Operations on matter and energy.)

Exploitation.	Manufacture.	Movement.

Occupations. II. Direct services to organisms.

Of non-cerebral func'ns.	Of cerebral functions.	Of co-ordination.

Occupations. III.

Unemployed.	Disabled.	Destructive.	Remedial.

D.—Partition (mediate and ultimate) to Class I.

Partition to Class II.

Partition to Class III.

D.—Use by Class I.

Use by Class II.

Use by Class III.

E.—Result to Class I.

Result to Class II.

Result to Class III.

papers essentially economic: witness the numerous interesting papers read to Sections A and G, on the applications of electric energy, and, best of all, the presidential addresses to those sections, of which both were devoted to economic physics, or rather, as the subject should be called, physical economics.

Finally, if the preceding axioms and the soundness of the above classification of social facts be not disproved, it follows that three out of the four great reforms demanded in Mr. Ingram's presidential address,[1] and repeated and enforced in Mr. Grant Duff's, are here introduced into the conduct of economic research, namely, "(1) that the study of the economic phenomena of society ought to be systematically combined with that of the other aspects of social existence; (2) that the excessive tendency to abstraction and to unreal simplifications should be checked; (3) that the *à priori* deductive method should be changed for the historical; while the fourth, that economic laws and the practical prescriptions founded on these should be conceived and expressed in a less absolute form," would readily also be exemplified if the limits of the present paper permitted reference to generalisation and to practice. Again, it is sufficient to quote Mr. Ingram's concluding proposals to show that these have been substantially adopted. The field of the section should be enlarged so as to comprehend the whole of sociology, "since the economic facts of society . . . cannot be scientifically considered apart, and there is no reason why the researches of Sir Henry Maine or Mr. Spencer should not be as much at home here as those of Mr. Fawcett or Prof. Price. Many of the subjects, too, at present included in the artificial assemblage of heterogeneous inquiries known by the name of anthropology really connect themselves with the laws of social development, and if our section bore the title of the sociological, the studies of Mr. Tylor and Sir John Lubbock would find in it their most appropriate place. I prefer the name sociology to that of social science."[2]

THE PROPER PROPORTIONS OF RESISTANCE IN THE WORKING COILS, THE ELECTRO-MAGNETS, AND THE EXTERNAL CIRCUITS OF DYNAMOS[3]

FOR the electro-magnet;

Let L be the length of the wire,

B ,, bulk of the whole space occupied by wire and insulation,

n ,, ratio of this whole space to the bulk of the copper alone (that is, let $\frac{1}{n}B$ the bulk of the copper),

A ,, the sectional area of wire and insulator,

R ,, the resistance of the wire.

For the working coil, let the corresponding quantities be L', B', n', R'. Lastly, let s be the specific resistance of the copper. We have—

$$B = A L$$
$$R = ns\frac{L}{A} = ns\frac{B}{A^2}.$$

Hence, $$A = \frac{\sqrt{(nsB)}}{\sqrt{R}} = \frac{K}{\sqrt{R}} \quad . \quad . \quad . \quad . \quad (1)$$

and similarly, $$A' = \frac{\sqrt{(n's'B')}}{\sqrt{R'}} = \frac{K'}{\sqrt{R'}} \quad . \quad . \quad . \quad . \quad (2)$$

where K and K' denote constants.

Now, let c be the current through the magnet coil, and c' that through the working coil, and let v be the velocity of any chosen point of the working coil. Denoting by p the average electromotive force between the two ends of the working coil, we have—

$$p = I\frac{c}{A}\frac{1}{A'}v. \quad . \quad . \quad . \quad . \quad (3)$$

where I is a quantity depending on the forms, magnitudes, and relative positions of B and B', and on the magnetic susceptibility of iron; diminishing as the susceptibility diminishes with increased strength of current, or with any change of R and R' which gives increase of magnetising force.

In the single-circuit dynamo (that is, the ordinary dynamo) c' is equal to c, but not so in the shunt-dynamo. In each, the whole electric activity (that is, the rate of doing wor) is pc'; or, by (3)—

$$I\frac{cc'}{AA'}v \quad . \quad . \quad . \quad . \quad (4)$$

or, by (1) and (2)—

$$\frac{I\sqrt{(RR')}cc'v}{KK'} \quad . \quad . \quad . \quad . \quad (5)$$

Of this whole work, the proportions which go to waste in heating the coils and to work in the external circuit are—

$$Rc^2 + R'c'^2 \quad . \quad . \text{ waste} \quad . \quad (6)$$
$$\frac{I\sqrt{(RR')}cc'v}{KK'} - (Rc^2 + R'c'^2). \text{ useful work} \quad (7)$$

By making v sufficiently great, the ratio of (6) to (7) (waste to useful work) may be made as small as we please. Our question is, how ought R and R' to be proportioned to make the ratio of waste to work a minimum, with any given speed? or, which comes to the same thing, to make the speed required for a given ratio of work to waste a minimum? To answer it, let r be the ratio of the whole work to the waste. We have, by (5) and (6)—

$$r = \frac{I\sqrt{(RR')}cc'}{Rc^2 + R'c'^2}\frac{v}{KK'} \quad . \quad . \quad (8)$$

For the single-circuit dynamo we have $c = c'$, and (8) becomes—

$$r = \frac{I\sqrt{(RR')}}{R + R'}\frac{v}{KK'} \quad . \quad . \quad . \quad (9)$$

or $$r = \frac{I\sqrt{\{R(S - R)\}}v}{SKK'} \quad . \quad . \quad . \quad (10)$$

where $$S = R + R' \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Suppose now S to be given, and suppose for a moment I to be constant. The problem of making r a maximum with v given, or v a minimum with r given, requires simply that $R(S - R)$ be a maximum; which it is when $R = \frac{1}{2}S$, that is, when the resistances in the working coil and the electro-magnet are equal. But in reality I is not constant; it diminishes with increase of the magnetising force. As it generally depends chiefly on the soft iron of the electro-magnet, and comparatively but little on the soft iron of the moving armature, or on iron magnetised by the current through the moving coils, it will generally be the case that I will, *cæteris paribus*, be diminished by increasing R and diminishing R'. Hence the maximum of r/v is shown by (10) to require R' to be somewhat greater than $\frac{1}{2}S$: how much greater we cannot find from the formula, without knowing the law of the variation of I.

Experience and natural selection seem to have led in most of the ordinary dynamos, as now made, to the resistance in the electro-magnet being somewhat less than the resistance in the working coil, which is in accordance with the preceding theory.

Whether the useful work of the dynamo be light-giving, or power, or heating, or electro-metallurgy, we may, for simplicity, reckon it in any possible case by referring to the convenient standard case of a current through a conductor of given resistance E connecting the working terminals of the dynamo. This conductor, in accordance with general usage, I call the "external circuit," which is an abbreviation for the part of the whole circuit which is external to the dynamo. In the case of the single-circuit dynamo the current in the external circle is equal to that through the working coil and electro-magnet, or c of our notation. Hence, by Ohm's law—

$$c = \frac{p}{E + R + R'} \quad . \quad . \quad . \quad . \quad (12)$$

or, by (3), (1), and (2),

$$c = c\frac{I\sqrt{(RR')}v}{KK'(E + R + R')} \quad . \quad . \quad (13)$$

Hence either $$c = O \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

or $$I = \frac{KK'(E + R + R')}{\sqrt{(RR')}v} \quad . \quad . \quad (15)$$

The case of $c = O$ is that in which

$$v < \frac{KK'(E + R + R')}{I_0\sqrt{(RR')}} \quad . \quad . \quad (16)$$

where I_0 denotes the value of I for $c = O$. To understand it, remember we are supposing no residual magnetism. For any speed subject to (16), the dynamo produces no current. When this limit is exceeded the electric equilibrium in the circuit becomes unstable; an infinitesimal current started in either direction rises rapidly in strength, till it is limited by equation (15), through the diminution of I, which it produces. Thus,

[1] "On the Present Position and Prospects of Political Economy" (British Association, Dublin, 1878).
[2] Quoted by Mr. Grant Duff, Presidential Address to Section F, 1881.
[3] Paper read at the British Association, York, 1881, by Sir William Thomson, F.R.S.

regarding I as a function of c, we have in (15) the equation mathematically expressing the strength of the current maintained by the dynamo when its regular action is reached. Using (15) in (10) we find—

$$r = \frac{E + S}{S} \quad . \quad . \quad . \quad . \quad . \quad (17)$$

which we all knew forty years ago from Joule.

In the shunt-dynamo the whole current, c', of the working coil branches into two streams, c through the electro-magnet, and $c' - c$ through the external circuit, whose strengths are inversely as the resistances of their channels. Still calling the resistance of the external circuit E, we therefore have—

$$c R = (c' - c) E, \text{ which gives } c = \frac{E}{R + E} c' \quad . \quad (18)$$

Hence, by Joule's original law, the expenditures of work per unit of time in the three channels are respectively

$$\left.\begin{array}{ll} R' c'^2 \quad . \quad . \quad . \quad . & \text{working coil} \\ R\left(\frac{E}{R + E}\right)^2 c'^2 \quad . \quad . & \text{electro-magnet} \\ E\left(\frac{R}{R + E}\right)^2 c'^2 \quad . \quad . & \text{external circuit} \end{array}\right\} \quad . \quad (19)$$

Hence, denoting as above by r the ratio of the whole work to the work developed in the external circuit, we have—

$$r = \frac{R' + R\left(\frac{E}{R + E}\right)^2 + E\left(\frac{R}{R + E}\right)^2}{E\left(\frac{R}{R + E}\right)^2} \quad . \quad (20)$$

$$\left.\begin{array}{rl} \text{whence} \quad R^2 r &= R'\frac{(R + E)^2}{E} + R(R + E) \\ &= \frac{R' R^2}{E} + (R + R')E + R(2R' + R) \end{array}\right\} \quad (21)$$

Suppose now R and R' given, and E to be found; to make r a minimum. The solution is—

$$E = \sqrt{\frac{R' R^2}{R + R'}} \quad . \quad . \quad . \quad . \quad . \quad (22)$$

and this makes

$$r = 2\sqrt{\frac{R'(R + R')}{R^2}} + \frac{2R' + R}{R} \quad . \quad . \quad (23)$$

Put now

$$\frac{R'}{R} = e \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

(22) and (23) become

$$E = \sqrt{\frac{R R'}{1 + e}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

$$\text{and} \qquad r = 1 + 2\sqrt{\{e(1 + e)\}} + 2e \quad . \quad . \quad . \quad (26)$$

For good economy r must be but little greater than unity; hence e must be very small, and therefore *approximately*

$$\left.\begin{array}{rl} & E = \sqrt{(R R')} \\ \text{and} & r = 1 + 2\sqrt{e} \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (25)$$

For example, suppose the resistance of the electro-magnet to be 400 times the resistance of the working coil—that is $e = 400$—and we have, approximately,

$$E = 20 R', \text{ and } r = 1 + \tfrac{1}{10}.$$

That is to say, the resistance in the external circuit is twenty times the resistance of the working coil, and the useful work in the external circuit is approximately $\frac{10}{1}$ of that lost in heating the wire in the dynamo.

FUNCTIONAL METAMORPHOSIS OF MUSCLES[1]

THERE is no system in the animal body to which the axiom of Guérin, viz., that "function makes the organ," applies with greater force than to the muscular system. Every student of comparative myology knows that according to the use required of a muscle we have alterations in its volume and connections, or indeed its total disappearance, should its further services in the animal economy be dispensed with. These are the factors which render muscular homologies in many cases so difficult to determine. There is one change, however, which is much more common than is generally believed, viz., the transformation of a muscle into fibrous tissue, or, in other words, its replacement by a ligamentous structure possessing attachments similar to those of its muscular ancestor. It might almost be laid down as a law that whenever a muscle ceases to be of use for contractile purposes, and when, from its attachments, it might be of service as a ligament, that it gradually in course of time becomes transformed into fibrous tissue, and is handed down to posterity in this condition. Indeed should it merely be a case of comparative value, and should the balance of utility be in favour of a ligament, then also will this metamorphosis in all probability take place. Of all adaptations in the muscular system this is perhaps the most beautiful, and instances of it are by no means rare. Thus, in the feet of the armadillo, orycteropus, pig, walrus, and several other animals, certain of the intrinsic pedal muscles have become fibrous bands, indubitably retained for some definite purpose, although their obvious function is often obscure. The most striking examples of this, however, are to be found in the feet of the horse, ox, sheep, camel, and their allies. In these we are able not only to demonstrate with the utmost precision the particular muscles that have become ligamentous, but also the process by which the change has been brought about, and the *rationale* of the transformation.

The *suspensory ligament of the fetlock* in the horse is an exceedingly powerful structure, which lies in the sole of the foot (*i.e.* upon the posterior aspect of the metatarsal bone) under cover of the flexor tendons. It plays an important part in the mechanism of the limb. Its attachments are such that it prevents over extension at the fetlock or metatarso-phalangeal joint, and its value in this respect is evidenced by the fact that when it is ruptured the horse becomes what is termed by veterinary surgeons "broken down." In this condition the fetlock joint sinks downwards towards the ground, whilst the hoof is tilted forwards and upwards.

This ligament is admitted on all hands to be derived from the intrinsic pedal muscles by a transformation of the muscular elements into fibrous tissue; indeed it bears its history written upon its face. Almost invariably a narrow streak of striated muscular fibres can be detected upon its superficial surface which points to its muscular origin. Upon its deep surface fleshy fibres in greater abundance are observed, but these are very pale, owing to a large admixture of fatty tissue.

The question now comes to be—Which of the intrinsic pedal muscles have entered into the formation of this ligament? In making this inquiry we have to keep two points in view: (1) that in the horse the middle or third digit is alone fully developed; and (2) that in a typical pentadactylous foot this digit is supplied by three intrinsic muscles, viz.: a two-headed flexor brevis, and two abductors or dorsal interossei (the second and third) inserted one upon either side of the digit. It is reasonable to conclude, therefore, that the suspensory ligament of the fetlock is derived from one or more of these muscles. But independent remnants of the two dorsal interossei are present, in addition to the ligament, which clearly proves the *flexor brevis medii* to be the source of this structure.

The dorsal interossei in the foot of the horse are of peculiar interest. They are so minute that they can exercise little or no influence upon the movements of the pes. They are simply to be regarded as vestiges of former greatness, and as pointing to retrograde development. They undoubtedly constitute a link in the soft parts between our modern monodactylous horse and its three-toed ancestor. They lead us back to a time when in the foot of this animal there were two distinct interosseous spaces, each filled by a well-marked interosseous muscle.

Still stronger evidence that the suspensory ligament originates solely from the *flexor brevis* of the middle digit is obtained by making thin transverse sections through its substance. We then observe that the sparse remains of muscular tissue are not confined to the surface of the structure, but penetrate into its midst. When the specimen is held against a dark ground, two crescentic opaque outlines are noticed lying side by side in its substance. These undoubtedly represent tracings of the two heads of the flexor brevis, out of which the ligament is developed. On subjecting the outlines to microscopic examination, we find that they are mainly composed of muscular fibres, but every here and there the continuity of this tissue is broken by fatty tissue, in which are observed transversely-divided nerves and blood-vessels.

In the ox, sheep, and camel the suspensory ligament performs the same office as in the horse. The presence, however, of two digits (the middle and annular) complicates somewhat its inferior attachments, in order that it may operate so as to prevent over-extension at both metatarso-phalangeal joints. In each of these animals the structure is undoubtedly formed by the two heads of

[1] Abstract of paper read at the York meeting of the British Association, by D. J. Cunningham, M.D., F.R.S.E., Senior Demonstrator of Anatomy, University of Edinburgh.

two muscles, viz., the *flexor brevis annularis* and the *flexor brevis medii*. Transverse sections of the ligament render this very evident.

In the suspensory ligament of the ox a considerable amount of muscular tissue is found upon both surfaces, and the transverse sections show that this penetrates into its substance in the form of four circular outlines which lie side by side. The fleshy fibres are more abundant than in the case of the horse, but still a considerable amount of fatty tissue enters into the construction of the outlines, and in this are placed nerves and blood-vessels. These four outlines are the remains of the four fleshy bellies of the two flexores breves which amalgamate and transform so as to constitute the ligament.

In the sheep not a trace of muscular tissue is to be found, either on the surface or in the interior of the ligament. The four circular outlines are seen on transverse section, however, but they are entirely formed of fatty tissue. What is of peculiar interest in this case is that in this fat the nerves and blood-vessels are still present.

The camel which the author had an opportunity of examining was a very young specimen, and its foot had been prepared by a fine gelatine and carmine injection. This in some measure obscured the intimate structure of the suspensory ligament. Not a trace of muscular tissue or fatty tissue could be detected either on its surface or in its substance. So complete was its transformation that not a single clue to its origin could be discovered. It is quite possible however that in a fresh uninjected specimen traces might be detected.

The suspensory ligament in these animals has undoubtedly been called into existence by the need for such a structure in the foot, and by the comparatively small value of the intrinsic muscles from which it is developed. The intrinsic muscles of the hand and foot have as their function the production of the more rapid and precise movements of the digits. In the animals which possess a suspensory ligament such a function is of no importance, whereas a powerful brace to provide against over-extension at the metatarso-phalangeal joints is an absolute essential.

But the study of the suspensory ligament of the fetlock suggests other interesting points: 1. The process of transformation of muscle to ligament appears to be effected by a fatty degeneration of the muscular fibres with a coincident multiplication of the connective tissue elements. Here, therefore, is what is usually regarded a pathological change assisting a morphological process. 2. The nerves of supply to the muscles are apparently unaffected by the change. In the sheep, in which there is not a trace of muscular tissue, they are seen in the substance of the ligament of a size relatively as great as in the ox or horse. 3. The presence of muscular tissue, where from its small amount it cannot possibly exercise any appreciable function, is peculiar. To account for its continuous existence we must of course suppose that it remains in a state of tonic contraction. The continuance of nerves in the ligament will enable it to maintain this condition.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 12.—M. Wurtz in the chair.—The following papers were read:—Remarks on a memoir of MM. Lœwy and Perigaud on flexure of telescopes, by M. Villarceau.—On the comparative qualities of water of the Isère and of the Durance, as regards irrigation and provision of soil, by M. de Gasparin. He compared the constitution of the liquids at points where all the affluents were united, and at different epochs. The two rivers are closely alike as to the quality of the slime they deposit, that of the Isère being only a little more argillaceous (which slightly favours the state of suspension). Now the Durance is largely utilised for irrigation, and enriches the departments of Bouches-du-Rhône and Vaucluse especially with fertile soil; and it is suggested that a like benefit should be derived from the Isère, in Isère and Drôme.—On a new mode of exploitation of mines of sulphur, by MM. de la Tour du Breuil. They apply the principle of raising the boiling point of water by means of a dissolved salt. Chloride of calcium is so used; the bath containing 66 per cent. of it. The apparatus consists of two rectangular vessels coupled and inclined. When the operation is terminated in one, the boiling liquid is directed into the other, which is previously filled with ore. While liquation is going on (which takes about two hours) the first vessel is emptied and re-charged. One furnace suffices. The sulphur produced is very cheap (about five francs a ton) and pure. Fusion is possible all the year, as no sulphurous acid is produced; and the extraction is very complete.—The Secretary called attention to the subscription opened for a statue to Lakanal at Foix (Ariège).—On radiophony produced by lampblack, by M. Mercadier. Not only is lampblack the best thermophonic agent at present, but it is susceptible, like selenium, of playing the *rôle* of the electric photophone. Instead of selenising one of the faces of his metallic double-spiral receivers, M. Mercadier covers it with lampblack, and they give good effects with intermittent solar, electric-light, and even gas, radiations. When exposed in dark to a copper plate gradually heated with an oxyhydrogen blowpipe, no sound is heard in the telephone till the plate is raised to a dull red; then it gradually increases in intensity. The author is disposed to consider the phenomenon *photophonic* rather than *actinophonic*. The resistance of these receivers diminishes as the temperature rises (from 2° or 3° to 50°), and the variation (very small) is represented nearly by a straight line.—Explanation of a contrast in double circular refraction, by M. Croullebois.—On the magnetic metals, by M. Gaiffe. He experimented with nickel and cobalt, obtained electro-chemically and variously treated before magnetising; some bars being kept hard, others annealed, and others annealed and forged. The figures show what a comparatively great coercitive force these metals (and especially cobalt), may acquire in a pure state, while pure iron, obtained by the same means, gives inappreciable deflections in the magnetometer. The annealed and forged samples produced the greatest effects (the annealed coming next). The weak coercitive force of the metals on issuing from the galvanoplastic bath, is attributed to the presence of hydrogen in combination with them.—On metaldehyde, by MM. Henriot and Oeconomoides.—On the rotatory power of albuminoid substances of blood-serum, and their determination by circumpolarisation, by M. Fredericq.—On permanganate of potash employed as antidote to the poison of serpents, by M. de Lacerda. A solution of snake poison having been injected subcutaneously under the thigh of a dog, and a 1 per cent. solution of permanganate of potash a few minutes after, the latter prevented all local lesion (abscess, &c.); there was merely a very slight swelling. In other cases of injection into the veins the permanganate proved a powerful antidote.—M. Maumené communicated accounts of a new apparatus for fractional distillation, and of one for measuring the alcoholic richness of mixtures of alcohol and water.

CONTENTS

THURSDAY, OCTOBER 6, 1881

AUSTRALIAN ABORIGINES

Australian Aborigines: the Languages and Customs of several Tribes of Aborigines in the Western District of Victoria, Australia. By James Dawson. (Melbourne: Robertson; London: Macmillan and Co., 1881.)

MR. DAWSON, a settler of old standing in the district west of Melbourne, and his daughter, Mrs. Taylor, who has been familiar from childhood with several native dialects, have in years of careful inquiry collected the present volume of information as to the languages and life of the group of tribes living inland from the coast between Portland Bay and Cape Otway. These tribes form part of the native population described in the "Aborigines of Victoria," compiled by Mr. Brough Smyth for the Victorian Government; but able and extensive as that work is, the anthropologist sees on comparing it with the present volume how far he must still be from thoroughly understanding the native institutions, when a minute study of one district can bring out so many new and difficult points as are to be found here. Take the native marriage laws as set down by Mr. Dawson. The tribes are split up into totem-classes named after animals, both sons and daughters belonging to the mother's class, and not being allowed to marry within it; thus a Pelican youth may not marry a Pelican girl, or a Boa youth a Boa girl, but Pelican may marry Boa. So far, this is like the exogamous rules found in various other parts of the country; but here it is further stated that though the class follows the mother's side, the tribe itself follows the father's side, and the natives are not allowed to marry into their own tribe either, nor may a man marry into his mother's or grandmother's tribe, nor into an adjoining tribe, nor into one that speaks his own dialect. This remarkable set of restrictions, which does not seem to correspond exactly with those of any other district in the world, is considered by the tribes who live under it as intended to prevent marriage between those of "one flesh," and indeed it bars kin-marriage in both the male and female line in a more thorough way than the known laws of any other Australian tribes. No marriage or betrothal is permitted without the approval of the chiefs of each party, who first ascertain that no "illegal" relationship exists. Any symptoms of courtship between those of "one flesh" are put down by rough handling of the culprits, and parents are apt to save their children from breaking the law by betrothing them in proper quarters as soon as they can walk. What can have been the motive which led the ancestors of these savages to carry their prohibited degrees to an extent which our physicians would consider practically absurd? Mr. Dawson speaks of these laws as admirable, and plainly thinks them founded on practical reasons against marrying-in, for he says that where the prohibitions have been disregarded under European influence, the aborigines attribute to this disregard the greater weakness and unhealthiness of their children, and the increase of insanity. This, however, may have got into the native mind from hints by the white doctors, and the whole subject of these marriage-prohibitions is as yet an unsolved problem. This is better seen when one does not look at one particular point, but at the system as a whole, with its network of ceremonial regulations. Among these, the custom of avoiding the mother-in-law is of course described by Mr. Dawson. He gives the usual details how, when a girl is betrothed, her mother and aunts may not look at or speak to the man for the rest of his life, but if they meet him they squat down by the wayside and cover up their heads, and when he and they are obliged to speak in one another's presence, they use a peculiar lingo, which they call "turn-tongue." This queer dialect is not used for concealment, for everybody understands it, and some examples of it are here given which show that it has much in common with the ordinary language. Should the present notice meet the eye of Mr. Dawson, it may be suggested that it would be worth while to find out whether the "turn-tongue" is an old-fashioned dialect kept up for this ceremonial purpose. For the rest of the marriage-customs we must refer to the book itself; but to give an idea of the state of formality into which life has come among these supposed free-and-easy savages, mention may be made of the duties of the bridesmaid and groomsman. When the married pair have been taken to the new hut built for them, for the next two moons the groomsman and the husband sleep on one side of the fire, the bridesmaid and the wife on the other, the new-married couple not being allowed to speak to or look at one another. The bride is called a "not-look-round," and the pair in this embarrassing position are a standing joke to the young people living near, who amuse themselves by peeping in and laughing at them.

Among the interesting questions as to Australian arts and ideas which Mr. Dawson touches on, is whether they had any notion of boiling food. He confirms the general opinion that they had not, and states that there is no word meaning to boil in their native dialects. But it does not always follow that what is true as to one group of tribes is true everywhere. Mr. Brough Smyth gives an account of the fish-hooks of the aborigines in Victoria, but Mr. Dawson declares that in his district they were unknown, though the native fishermen have come so near angling as to use a rod and line with a bunch of worms for bait, with which they pull out the fish before he has time to disgorge. Looking over the grammatical part of the book, we find the list of numerals in the native dialects one of the most perfect examples of the way in which numerals have been developed from counting on the fingers. They say "one hand" for 5, "two hands" for 10, and so on with hands and twenties up to 100. But the unusual and noticeable point is, that though getting so far, they have not worked out words for the intermediate numbers above 10, but fall back on the primitive gestures; thus they have not words for 11 or 12, but they say 10, and hold out one finger or two to make up the number. Mr. Dawson seldom quotes or criticises books, but when he gives the fact that there is a native word for 100 he adds a note that this is wholly at variance with the statement made by Mr. E. B. Tylor ("Primitive Culture," vol. i. p. 220) as to some Australian tribes having no numeral words even so high as 5. To prevent misunderstanding he should have pointed out that the next page of the work in question makes reference to other

Australian tribes reported to have numeral words up to 15 or 20. But the point raised is well worth attention. The statement as to tribes in various districts having no distinct numeral words above three, and only struggling on to four and five by saying "two-two," &c., rests on the authority of Europeans who have studied the native languages, sometimes well enough to write grammars of them. Are we to think that the natives generally had words for large numbers, and yet the Europeans failed to discover them? Or, rather, is it not easy to suppose that some tribes raised themselves (possibly since contact with the white man) above this low level of arithmetic, making, out of their counting on the fingers, numeral words even as high as the words here given for 100? It would be interesting if it could be shown etymologically that the terms here given for 20 and 100 had originally a material meaning, like the word for 5, which still means "hand."

One of the greatest difficulties in studying savages is to know how far to trust or distrust their assurances that what they tell is really their own, and not picked up from foreigners. From this point of view it is worth while to look closely at the story of the lost Pleiad, which here appears among the native myths of the "black-fellows." The author's friends naturally doubted its genuineness, but on further inquiry it was found to be widely known. The tradition is that the Pleiades were a chiefess called Gneeanggar and her six attendants; Waa, the Crow (the star Canopus), fell in love with her, and finding that she and her women were going in search of white grubs, he turned himself into one, and bored into the trunk of a tree, where they were sure to find him. The women, one after another, poked their little wooden hooks into his hole, but he broke the points, till at last his love put in her beautiful bone hook, and he let her draw him out, whereupon he turned into a giant and ran away with her; since then only six Pleiads—the serving-women—have been left. Now between this story and our classical myths there is a difference. Ovid's version seems to carry its origin on its face, agreeing with the fact that only six of the stars in the cluster are bright and plain to common eyes, so the myth tells of a hidden or faint seventh. She is Merope hiding herself for shame at marrying a mortal, or Electra putting her hand before her eyes, not to see the ruin of Troy. But in the Australian tale the vanished star, being the queen, ought of course to be the brightest; so that there is little sense in the story, unless Mr. Dawson is prepared to maintain that the Australians remember a time when there was a Pleiad brighter than the rest, which has now vanished. It would be easier, if more commonplace, to guess that the natives got the idea of a lost Pleiad from some Englishman who had heard the story at home, but missed the point of it.

The anthropological work done by Mr. Dawson and Mrs. Taylor hardly needs praising. It is enough to point out how carefully, not relying on books, they have made their own inquiries on every subject, and recorded them as scientific material. It is to be hoped that they will not cease their researches, for there must still be much valuable evidence to be gleaned in their district, if it is done without delay.

EDWARD B. TYLOR

OUR BOOK SHELF

A Dictionary of Chemistry and Allied Sciences. By H. Watts, F.R.S. Third Supplement. Part II. (London: Longmans, 1881.)

WE have no publication in English strictly corresponding to *Liebig's Annalen* or the *Annales de Chemie et Physique*, and were it not for this now gigantic dictionary of chemistry by Mr. Watts many, both advanced and elementary students of our science, would find their labours considerably increased by the necessity of having to hunt up a great number of facts and records of work done in foreign journals. The chemical record in this volume includes discoveries made in 1880, and in addition a number of exhaustive articles by Professors Armstrong, on Isomerism; G. C. Foster, on Thermodynamics; Schuster, on the Spectrum; Thorpe, on Specific Volumes; and others. This part commences with G, the first large articles being Gallium and Gases, the latter being very complete and up to date. A long section is devoted to Heat, which, with the article on Thermodynamics, is very valuable. In the portion on Isomerism we are very glad to notice a slight but still important definition, or rather restriction of the term isomeric. That is, bodies should only be classed as isomeric when their reactions indicate that they are of the same type of structure. This article is of some length, and contains the main points of the hypotheses brought forward by Van l'Hoff and Le Bel and others. We thoroughly agree with the concluding paragraph of the article, and venture to add that probably when we do know a little about the loss or gain in energy in the case of reacting molecules the terms saturated and unsaturated atoms will cease to be employed. The article dovetails into the one on Light, and together they form an important fraction of the book. The greater part of the volume is of course taken up by "organic" and physical chemistry, a considerable number of mineral substances being however described, the section on the metals allied to yttrium being very interesting. The references to the original papers attached to each article render the work even more valuable to those chemikers and physikers to whom a few languages is no difficulty. Although a dictionary, it is very thick, and probably an index would facilitate the search after any particular description; but the want is a minor one. W. R. H.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Madeira Earth-electric Cloud again

WHAT a valuable paper, Mr. Editor, you have published this week from Mr. J. B. N. Hennessey, with its diagram of the new set of sun-spots which broke out suddenly near the centre of the sun's disk, between 4h. and 5h. p.m., on July 25, as recorded by the photo-heliograph of the Indian Trigonometrical Survey, under his able charge, at Dehra.

His enthusiasm at having localised the appearance of the phenomenon in time, as well as space, is unexceptionable; and his long experience as an observer gives his opinion commanding weight, when he further holds forth on the rarity of such an occurrence, on such a scale and so centrally situated on the sun's disk—whence its probable vast importance for the physics of the earth and the foundations of a new science. All that is admirably true and suggestive for the future; but meanwhile I desire to claim the first fruits of the case as the very thing I have been expecting ever since I left Madeira at midnight on July 29.

And why should I have been expecting such an announcement, do you ask? Well, do you remember my letter to you from

Madeira on June 27 (NATURE, vol. xxiv. p. 212, with a sequence on p. 237) describing the extraordinary cloud that appeared there on June 26, alarming all the inhabitants, the typically "oldest" of whom declared they had never seen such a cloud as that before? It was, too, in very truth a most remarkable affair; and seemed to me only to admit of full explanation as a peculiar case of the earth answering by escape of its interior electricity to the sun; where, according to my own daily solar diagrams, there had just occurred an outburst of solar spots very nearly over the ends of the solar radii that were then pointing towards the earth.

Weeks passed on without anything to interfere with, or undervalue, that explanation; when lo! on July 26 (the very same day, curiously enough, of the next month) another cloud appeared over Madeira, of just the same peculiar physical character as that of June 26. "Why," were inclined many visitors to ask, "is this kind of cloud, in spite of the asseverations of the 'oldest' inhabitants, no very great rarity after all in this part of the world?" There had been certainly thus two cases of it occurring with a very short interval between them; but nevertheless, I was inclined to respect the assertions of the greybeards; and said, "Something unusual must again have happened in the sun; but as my observatory was dismantled on July 23, and the component parts of it packed up ready for shipment on July 25 and 26, I had not then any knowledge of what it might be." Now, however, see how perfectly Mr. Hennessey's Indian solar photographs fulfil all that was required to make this second Madeira cloud phenomenon an exactly similar cosmical case to that of its mensual predecessor; or to testify that an extraordinary, unusual, most sudden outbreak of solar spots did take place over the very part of the sun's surface turned towards the earth late on July 25, and within twenty-four hours afterwards the earth-electric cloud made its appearance above Madeira, where it was thus noted in my pocket journal:—

"Tuesday, July 26.—During this afternoon there was a great cloud-structure formed to the west, with all the characteristics of smooth-rimmed lenticular strata under strata, and the topmost visible one breaking out into fringes of cirro-cumuli, that marked the still grander cloud of June 26." This one was however very splendid after sunset, and when the red tints thereof had faded from it they were replaced by the richest and purest browns, of the burnt-umber variety, I have ever beheld in the sky. The cloud was vertical over the lower southern slopes, almost the southern sea-shore of Madeira; not over the high peaks of the island, in so far at once pointing to a different origination from Dr. Muirhead's (of Cambuslang) cloud in NATURE, vol. xxiv. p. 237. That cloud was an affair plainly of the cold of a snow-covered mountain-top in Britain, and is just such an ordinary local production as any one can see for a large part of the year on the South African hills round about Table Mountain and Table Bay—whenever the south-east trade-wind blows over that country. For there, day after day, it produces very clumsy-shaped masses of vapour either on, or vertically over, the tops of the hills, according to their respective absolute elevations. But never once, during ten years, did I see any approach in the arrangement of the constituent particles of those clouds to the neat, refined, peculiar shapes of what formed the most conspicuous characteristic of the two successive Madeiran earth-electric clouds of June 26 and July 26. They had each been preceded by as peculiar, as rare, a central outburst of solar activity, and probably required no less for their due manifestation, as well as the performance of their functions in cosmical electric radiations and exchanges.

PIAZZI SMYTH,
Astronomer-Royal for Scotland

15, Royal Terrace, Edinburgh, September 30

American Cretaceous Flora

I HAVE only just read Prof. Newberry's clear and concise account of the American Cretaceous series (NATURE, vol. xxiv. p. 191). I regret that I am still unable to agree with him that the relative ages of American and European Cretaceous beds are satisfactorily correlated. I should not again venture to insist so strongly on what must seem to Dr. Newberry to be but an individual opinion, except that he seems to expect a reply, and I have further some new evidence to bring forward. Of my opinions the one to which he takes most exception seems to be that "no American or European so-called Cretaceous land flora can be proved to be as old as our White Chalk." It is this statement therefore which I must substantiate.

In America the plant-beds of Vancouver's Island contain many Angiosperms, and are said to be of the age of the Gault or Upper Greensand, and the Dakota group, which has yielded one hundred distinct species of Angiosperms, is said to be older than our Chalk. The Colorado group is said to represent not only our Grey and White Chalk, but the Mästricht beds. Even the Laramie group or "Lignite series" is placed in the Cretaceous system. I have unfortunately not the books requisite to re-examine critically the American evidence, and must therefore confine myself to stating that which on this side of the Atlantic tends to show the relative age of the American series to be very considerably overestimated. I would propose, however, to Prof. Newberry an exchange of the more abundant Cretaceous mollusca, in order that they may be compared together; after which I might possibly find myself able to visit some of the American sections. This I should the more like to do, as I happen to be acquainted both with the Cretaceous and Eocene mollusca, and with the floras, of England—the very evidence, in fact, upon which the respective ages of the series is to be decided.

In the first place I am able to assure Prof. Newberry most positively that on plant evidence the Laramie series must be bracketed, if anywhere, with our Middle Eocene. Not only is the facies of the flora identical, but identical species appear in both continents in these series. I cannot yet give a list, but I would particularly point to such highly characteristic species as *Lygodium Kaulfussi*, Heer (syn. *L. neuropteroides*, Lesq.), and *Anemia subcretacea* (Saporta) (syn. *Gymnogramma Haydenii*, Lesq.), which were identified by Lesquereux himself after comparison with actual specimens which I forwarded to him. These are fully described in the Palæontographical Society's publications, which I hope Prof. Newberry will glance through. We have beyond all question, in the first stage of the great "Lignite series," a common line to work from, and the age of this line is, assuredly, according to the plants, that of our Middle Bagshot series. Below our Middle Bagshot there is, in France and England, a vast series of Eocene deposits containing many distinct floras of most dissimilar types, and about which, in many cases, scarcely anything is known. Even at the base of these we are very far from the age of our Chalk, we have still an obscure series of local deposits which to some extent bridge the gap between our Secondary and Tertiary periods. Some of the most noted of these deposits I have recently visited.

The highest, I believe, of these so-called Cretaceous beds in Europe is the coral deposit of Faxoe. Its solitary claim to be considered of Cretaceous age is a *Pleurotomaria!* It has no Cephalopods except *Nautilus* and *Aturia zic-zag*, and not even the persistent *Inoceramus*. Except Pleurotomaria, the mollusca are all more of Eocene than Cretaceous type. *Cypræas* are abundant, and there is a *Mitra, Triton, Voluta, Turbinella*, a *Rostellaria* and *Ampullaria*, &c.

In the underlying "Faxœlaget" the Cretaceous element is reinforced by *Baculites* and *Scaphites*. In the Greensand of Bornholme, *Belemnites* and *Inoceramus* are added; and finally, in the Chalk of Moen, a smooth *Ammonite*, one or more large *Hamites*, and a variety of other Cretaceous types appear. We have thus a clear passage downward into the Cretaceous series; but even the age of the Moen's Chalk is not quite definitely known, for the supposed *Belemnitella*, which apparently fixed its zone, is in reality a *Belemnite*. A few forms, however, seem to link it slightly with the Greensand of Aachen, whose age I shall now consider.

The highest of the Aachenian series is Chalk with flints. In this mollusca are few; but this is of less importance, since the Chalk rests upon Greensand, in which they abound. The fossils are in much the same condition as at Blackdown, and among them are about sixteen apparently Gault and Blackdown species. The greater part of the latter are however carried up into our Grey Chalk, where they cease simply, as far as we know, because the succeeding beds were not fitted to preserve them. These shells are mixed with others, about thirty species, of Tertiary aspect, including *Voluta, Murex, Turbo, Fusus, Pyrula, Borsonia, Bulla, Turritella, Corbula, Tellina, Cytherea, Lucina, Pectunculus*, &c., and *Clavagella*. The presence of *Bellemnitella mucronata* and *B. quadrata*, together with *Baculites*, also point to its being at least younger than the Lower Chalk. Below these are the sands with Dicotyledons. The flora these contain, while mainly unlike that of the Eocene, possesses nevertheless some types of leaves which appear identical with Eocene forms, and is of the highest importance in comparing the American

Cretaceous series. I need not refer to the Mästricht beds, except to notice that a mixture of Tertiary and Cretaceous types of mollusca is also apparent in them. One circumstance, however, lessens the value of the evidence presented by the mollusca and the flora; we are so little acquainted with either the Gastropods, the Dimyaria, or the plants of the White Chalk age, that it is possible these may have inclined more to Tertiary types than those of the Grey Chalk would lead us to suspect.

I believe that in the American Cretaceous molluscous faunas there is precisely the same mingling of types described above, and if so, they should surely be bracketed together, rather than with our Neocomian Gault, or even Grey Chalk, which present no such mixture and contain few Tertiary types, except in unimportant groups, as Dentalium. Further, we must not overlook the oft-repeated negative arguments that we have no dicotyledonous plants of these ages in Europe, and that *Baculites*, &c., may have survived longer in America than in Europe. The whole series in America forms, so far as I gather, a natural sequence, the age of one part of which, the Laramie, can be fixed as Middle Eocene, and I think, before correlating the remainder with the older Cretaceous beds of Europe, with which neither their fauna nor flora agrees, the position occupied in the American series by the older Eocene, and the transition beds which I have enumerated, should be as far as possible ascertained. The matter is thus still, and must remain for the present, in an unsatisfactory state; but the importance of removing all doubt as to the relative position of those American beds which have yielded such magnificent palæontological data, and of the more typical British strata, is so great that I hope Prof. Newberry will not let the subject drop.

J. S. GARDNER

Gradations between Hermaphroditism and Gynodiœcism

ABORTION of the stamens in some portion of the flowers occurs in different species of the genus Dianthus. *D. superbus* has been shown to be gynodiœcious in my work on "Alpenblumen" (p. 202, Fig. 79). *D. deltoides*, the only species growing near Lippstadt, has lately been examined by myself, and has been found under certain circumstances to become gynomonœcious and gynodiœcious. Of *D. Carthusianorum* among 167 flowering stalks sent me from Thuringia by my brother, Wilhelm Müller, there were two producing female flowers with greatly aborted stamens. *D. deltoides* near Lippstadt offers interesting gradations from hermaphroditism to gynodiœcism. On the border of a meadow of some hundred stems examined by myself, all flowers, without exception, proved proterandrous, with normal development of anthers and stigmas. In the grass-grown slope of a sandy hill ("die Weinberge") likewise all stems produce proterandrous flowers, but on many stems the stamens, although emerging above the petals before the development of the styles and stigmas, bear diminished whitish anthers not opening at all, and containing only some shrivelled pollen grains. Lastly, in a barren sabulous locality ("Schützenplatz") many of the stems produce female flowers, with stamens aborted in the same degree as shown in *D. superbus* ("Alpenblumen," Fig. 79 D), and not unfrequently such female flowers and proterandrous hermaphrodite ones are found on the same stem.

Lippstadt HERMANN MÜLLER

Red Stars

DR. DOBERCK, who has paid particular attention to colour in his observations of Doubles, has kindly sent me the following list of red stars found by him in 1880. The first column gives the number, and the second and third the positions (for 1855) in the B.D.:—

No.	α h. m.	δ ° ′	Colour.	Date in 1880.
+ 4°877	5 7	4 59	Red	Jan. 30
*	3 12	±64	Glowing red	Feb. 8
5°1790	7 40	5 46	Ruddy	,, 14
20°1775	7 13	20 42	Pale red	,, 14
22°1198	6 1	22 13	Pale red	,, 14
26°2250	11 37	26 2	Red	March 8
33°4456	22 6	33 53	Red, but very pale	Sept. 10
20°5386	23 45	20 51	Pale red	,, 10

* Dr. Doberck does not give the number of this star, but it seems to be, probably, 64°391.

Dr. Doberck remarks that the two stars on both sides of η *Draconis* are pale red; and in *Coma Ber.* and south of it are several ruddy stars.

J. BIRMINGHAM

Millbrook, Tuam, September 18

Bombay Rainfall and Nile Floods

IN looking over data of the rainfall at Bombay and comparing them with the ebb and flow of the Nile for the corresponding years from 1849 to 1880 inclusive, I was so struck with the similarity, almost identity, of magnitudes, that I have been led to copy them out, and perhaps you may consider them worthy of publication in your most valuable journal. Within a trifling fraction the whole of the annual rainfall at Bombay happens in the months of June, July, August, and September. Very rarely a little falls in May, perhaps a little more frequently, some in October, but these small quantities but slightly augment the sum total. They are included in the four months' totals in the following table:—

Year.	Rainfall of June and July in Bombay.	Ditto June, July, and August in Bombay.	Ditto June, July, August, and September in Bombay.	Variation from mean atmospheric pressure.	Lowest ebb of the Nile.	Highest flood of the Nile.	Wolf's sun-spots.
	inches.					feet.	
1849	74·5	88·16	118·88	−·011	1·64	22·31	95·4
1850	36·5	43·11	51·15	−·001	1·64	18·47	69·8
1851	77·7	101·3	106·14	−·013	1·8	23·13	63·2
1852	49·59	60·25	75·46	−·004	2·59	16·73	52·7
1853	52·71	61·27	69·65	+·005	·9	23·01	38·5
1854	55·23	74·43	89·79	−·005	1·8	22·81	21·0
1855	24·98	28·13	35·10	+·015	2·85	16·5	7·7
1856	52·40	62·93	71·08	−·003	1·3	22·81	5·1
1857	38·92	60·93	79·23	−·001	1·42	18·7	22·9
1858	37·92	49·37	61·9	+·003	·32	19·52	56·2
1859	59·86	75·57	81·84	+·003	·29	19·65	90·3
1860	57·69	66·88	74·65	−·003	·08	23·42	94·8
1861	66·43	102·95	106·08	−·012	1·8	23·32	77·7
1862	38·35	62·0	76·56	−·026	1·8	16·53	61·0
1863	58·33	71·8	80·33	−·017	4·13	21·78	45·4
1864	39·37	51·39	56·60	+·023	4·59	14·43	45·2
1865	30·6	69·61	73·46	+·002	3·11	18·47	31·4
1866	64·63	88·5	92·39	+·013	2·54	23·2	14·7
1867	44·93	62·06	73·57	+·015	2·29	19·3	8·8
1868	47·83	71·78	78·43	+·027	2·16	17·1	36·8
1869	58·49	87·2	115·39	+·005	1·57	23·01	78·6
1870	53·39	64·48	81·06	−·012	1·88	22·81	131·8
1871	30·37	39·33	47·2	−·004	2·29	21·98	113·8
1872	60·59	71·21	67·61	−·014	1·23	22·7	99·7
1873	38·69	75·61	87·42	+·002	1·64	19·35	67·7
1874	67·54	78·78	93·56	+·001	·98	25·82	43·1
1875	42·79	58·70	88·08	·0	1·31	21·8	18·9
1876	40·87	52·81	58·93	+·007	−1·88	21·37	
1877	51·99	55·9	70·96	+·037	−1·96	16·8	
1878	73·73	95·63	123·1	−·011	− ·72	26·18	
1879	36·95	66·22	73·41			25·03	
1880	38·79	43·55	71·23			21·45	

The floods of the Nile are mainly caused by the heavy rains which descend upon the high tablelands of Abyssinia, a range of mountains on the opposite side of the Indian Ocean to that of the Ghauts, but parallel to them and under the same latitudes. The inference to be drawn is obvious. The great south-west monsoon which sweeps over the Indian Ocean in the summer months produces a like effect in both cases, inducing fertility and plenty, alike on the plains of the Concan of India and the Delta of Egypt. It may be mentioned that the lowest ebb of the Nile always happens in June, and the highest flood about the end of September and the beginning of October. I have included in the table a column showing the variations of the mean barometrical pressure, and a column giving Wolf's observation of sun-spots, taken from NATURE, vol. xxi. pp. 477–82.

MORGAN BRIERLEY

Port Said, September 8

THE INTERNATIONAL EXHIBITION AND CONGRESS OF ELECTRICITY AT PARIS[1]

II.

THE most crowded place in the Exhibition is the *Théâtre de l'Opéra.* Here from eight to eleven on three evenings in the week are to be seen four long *queues* waiting for their turn to enter one of the four rooms where the mysterious music is to be heard. Round the walls of each room are hung telephones in pairs, some twenty pairs in all, and the same number of persons are admitted. On putting the telephones to your ears you hear the music which is being performed at the opera-house more than a mile distant. Some of the singers seem to be on your right hand, others on your left, and it sometimes happens that a particular voice is quite piercing in its loudness. There are in fact ten transmitters disposed along the front of the stage, near the footlights, and ten wires leading from them, two of which are connected with the telephones intended for your two ears. Special precautions are taken to prevent the action of the transmitters from being disturbed by the tremors of the boards under the feet of the actors, the transmitters being supported on india rubber and loaded with lead. The telephonic apparatus employed is that of the Ader system.

The greatest novelty as regards principle is exhibited in Dolbear's telephone, in the United States department. The receiver has no magnet, but has two parallel metallic plates near together, and electrically insulated from each other. One of them is connected with the line wire, and the other (in the specimen here exhibited) with the return wire. These two wires are connected with the terminals of the secondary coil of a small Ruhmkorff at the sending station; and the voice of the speaker produces variations in the primary current, on the usual plan of varying the resistance in the circuit of a local battery by variations of pressure. The secondary circuit is not completed, inasmuch as the two plates do not touch; but the opposite electricities which are transmitted to them attract each other on electrostatic principles, and the plates are thus made to vibrate in unison with the voice of the speaker at the sending station. The instrument exhibited is very effective, and reproduces a whisper with greatly increased intensity. It is claimed that this invention does away with the disturbance experienced in other telephones from currents in neighbouring wires, inasmuch as such currents will not affect the attraction between the plates. We should add that the instrument exhibited speaks fairly even when the plate next the ear is disconnected from the wire intended for it, but of course less loudly than when the connection is made. This is just what one would expect from electrostatic attraction, the attraction of a charged for an uncharged body being less than that between two bodies oppositely charged.

We have had an opportunity of seeing the system adopted by Mr. Edison for the measurement of the quantity of electricity consumed in each house which receives a supply from one of his mains. A definite proportion (one thousandth part) of the whole current which goes through the house is shunted through a cell containing two copper plates in a solution of sulphate of copper. The positive plate loses, and the negative plate gains, an amount of copper exactly proportional to the quantity of electricity which passes. There are two such cells in series, one serving as a check upon the other, and the whole arrangement is kept under lock and key, to be opened only by Mr. Edison's agents when they come round to inspect the meters. As the lamps supplied (of a given type) are almost precisely alike in their resistance, and the current, when flowing, is always nearly the same, this arrangement gives a practically accurate measure of the illuminating power supplied.

Much interest has been excited by the exhibition of three magneto-electric machines constructed by Prof. Pacinotti of the University of Cagliari. One of these, constructed at Pisa in 1860, is the earliest example of the principle of the ring-shaped armature, since embodied in the machines of Gramme and Brush. It was originally constructed as an engine to be driven by a current from without; but it was also used as a generator of electricity, and both these uses of it were described in a paper in the *Nuovo Cimento* in 1864. The machine contains an iron ring like an anchor ring, round successive portions of which are wound coils of insulated copper wire in depressions cut in the ring to receive them. The intervening portions of the ring are thus (as in the Brush machine) enabled to come very nearly into contact with the surrounding fixed magnets. These consist of two half rings which are the pole pieces of two straight electro-magnets. The coils above mentioned are connected in a series, and their junctions are in connection with the several segments of a commutator, as in the Gramme machine.

The second machine was constructed in 1873, and described in the *Nuovo Cimento* in 1874. It is a generator of electricity, of the kind now known as the shunt dynamo, that is to say, the current generated is divided in parallel circuit between the fixed electro-magnet and the external resistance. This is done by means of two pairs of brushes making contact with different sections of the revolving commutator. The ring is replaced by a flat cylinder, across which the successive coils are wound in depressions made for the purpose, the directions of winding being the same as in Siemens' continuous current machine, which was invented about the same time. The connections of the successive coils with one another and with the segments of the commutator are the same as in the first machine.

The third machine, which was constructed in 1878 on a model dating from 1875, is of a type of which, so far as we know, it is the only example. The idea of it is taken from the well-known experiment (Arago's rotations) in which a revolving horizontal copper disk causes a large magnetised needle balanced above it to revolve in the same direction. The explanation of the effect was first given by Faraday. It depends on the action of a current generated in the copper disk by its motion in the magnetic field due to the needle. The strongest current flows along that diameter which is parallel to the needle, and the current is completed through the circumferential portions of the disk. Pacinotti virtually cuts away all except the diametral portion and one of the two circumferential portions; in other words, he takes a wire and bends it into the shape of the letter D. This is one convolution of his revolving coil; the next is like the same D tilted a little; the next is tilted a little more, and so on; so that some of the convolutions have the positions—

the straight part of the wire passing through or nearly through the axis of the coil, and the curved part being in the circumference. There is no room for a core in the ordinary sense, as the wires occupy nearly the whole interior space; but pieces of iron are so disposed partly within and partly without the coil as to serve the purpose of a core, by increasing the induction of the fixed magnets.

(*To be continued.*)

[1] Continued from p. 512.

ILLUSTRATIONS OF NEW OR RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION[1]

IV.

8. *THE White-nosed Saki* (*Pithecia albinasa*).—The peculiar American monkeys which belong to the closely-allied genera *Pithecia* and *Brachyurus* of naturalists, and are generally known as "Sakis"—a name probably derived from some Indian term—are restricted to the forests of Guiana and Amazonia, and seem to have in the case of each species a very restricted geographical area of distribution, one of these monkeys not intruding within the limits of another. As regards the genus *Brachyurus*, which is little more than *Pithecia* with a shortened tail, Mr. W. A. Forbes has lately shown this to be the case, in an article published in the Zoological Society's *Proceedings*,[2] wherein, after describing the anatomy of *Brachyurus rubicundus*, he has given a map to illustrate the distribution of this and the two allied species of *Brachyurus*. Each of them is limited to a peculiar district of Amazonia, one (*B. melanocephalus*) to the forests of the Rio Negro, a second (*B. calvus*) to those lying between the Putumayo and the Japurá, on the north bank of the Amazons, and the third (*B. rubicundus*) to the district contained between the main stream and the Rio Ica.

FIG. 8.—The White-nosed Saki.

In like manner the few particulars which have yet been recorded as to the exact localities of the *Pithecia* seem to tend to similar conclusions. Although we must suppose them, in obedience to the laws of descent, to have originated in common ancestors, they now occupy restricted areas cut off from one another, and in some cases rather widely separated. Why, in this as in similar cases, the form should have ceased to exist in the intermediate districts, is a subject on which it is at present difficult even to offer a conjecture.

The *Pithecia* are easily divisible into two sections—one embracing the curly-haired species, such as *P. leucocephala*, *P. monachus*, and their allies, and the other the smooth-haired forms, such as *P. satanas* (commonly called by the dealers the Jew-Monkey), and *P. chiropotes*. The White-nosed Saki, of which a figure is herewith given (Fig. 8), belongs to the latter group, and is one of the rarest and least known of the South American monkeys. A single example of it was obtained by the French collector Deville, on the Upper Amazons, during his descent of that river in company with de Castlenau's celebrated expedition, and is now in the Paris Museum. It was first described by Deville and Isidore Geoffrey St. Hilaire jointly, in 1848, and subsequently figured in the "Zoologie" of Castlenau's expedition, but the exact locality where it was procured was unfortunately left unrecorded.

The example of this monkey, lately living in the Zoological Society's collection, was purchased of a dealer in January last. It is uniformly, but rather sparingly covered with black hairs. The nose is broadly naked, and of a bright fleshy red, but shows a few white hairs between the nostrils, which are sufficient to justify its scientific name. The long hairs on the head fall on both sides of the head and over the front. The length of the body is about fifteen inches, of the tail eighteen inches. The latter organ, although clothed with elongated hairs, appears to be slightly prehensile. The specimen is of the female sex, apparently not fully adult.

9. *The Mountain Nestor or Kea* (*Nestor notabilis*).—Whatever may have formerly been thought to the contrary, there can be now no doubt that animals are continually changing their habits in order to suit themselves to the altered circumstances of their existence. A very familiar instance of this is that of the common swallow, which, in Europe at least, usually builds its nest in chimneys. Before chimneys were invented it must obviously have affixed its nest to some other chimney-like structure—probably to the inside of a hollow tree. But a much more striking and less laudable change of habit has of late years taken place in a New Zealand bird, of which we herewith give an illustration (Fig. 9). Parrots, though varying much in the details of their diet, are generally considered to be altogether frugivorous. Fruit and seeds, and in certain special cases moss and honey, are, no doubt, their proper food. But since the introduction of the domestic sheep into New Zealand the Mountain Nestor, which was previously content with a modest repast of an entirely vegetable character, has developed a taste for mutton. Many instances have now been recorded of this bird attacking not only sick and dying sheep, but, it is alleged, even those that are strong and healthy, though we should hardly suppose that this parrot exists anywhere in sufficient numbers to be likely to do the flock-masters any serious injury.[1]

[1] Continued from vol. xxiii. p. 480.

[2] "On the External Characters and Anatomy of the Red Ouakari Monkey (*Brachyurus rubicundus*); with remarks on the other species of that Genus." By W. A. Forbes, B.A., F.L.S., Fellow of St. John's College, Cambridge, Prosector to the Society.—*P. Z. S.*, 1880, p. 627.

[1] From the interesting article by Mr. Potts on the habits of this parrot just

The individual of this species now in the Regent's Park collection, from which the drawing has been taken, was transmitted as a present to the Society by Dr. A. de Lautour of Otago, New Zealand, along with the subjoined particulars concerning it, contained in a letter addressed to the secretary:—

"I have the pleasure of informing you that I am sending home an example of the Kea (*Nestor notabilis*), or Mountain Parrot, a bird celebrated, or rather notorious for its sheep-destroying proclivities.

"Many abler pens than mine have already written about their habits; but I was fortunate enough to be perhaps the first to send home a specimen of their work in the shape of the colon and lumbar vertebræ of a sheep, in which colotomy had been performed by one of these birds.

"This specimen was shown at a meeting of the Pathological Society by my friend and former master, Mr. John Wood, F.R.S., and is now in the Museum of the Royal College of Surgeons of England.

"The bird which I am now sending home has been in my possession for nearly two years. It was caught in the act of attacking some sheep which a shepherd was bringing down off the tops of some ranges in the back country. He luckily succeeded in knocking it over with a stone, cut its wings, and brought his captive down In effecting the capture the shepherd suffered considerable loss as to his trousers and other garments, and not a little injury in scratches from its formidable beak and claws. These same scratches had not entirely healed when he came down here under my care some ten days later, suffering from a broken leg (this by the way was not done by the Kea).

"While I have had the Kea, his diet has consisted mainly of mutton, raw; he does not care for cooked meat, but will take it if very hungry. Occasionally he will take beef, and he is fond of pork. Popularly he is said to prefer fat, but in confinement he chooses the lean and leaves the fat; he does not care for biscuit, but he likes the seed of the sow-thistle."

Again, in his excellent work on the birds of New Zealand, Dr. Buller tells us that the "*penchant* for raw flesh exhibited by this parrot in its wild state is very remarkable. Those that frequent the sheep-stations appear to live almost exclusively on flesh. They claim the sheeps' heads that are thrown out from the slaughter-shed, and pick them perfectly clean, leaving nothing but the bones." An eye-witness has described this operation to Dr. Hector as follows:—"Perching itself on the sheep's head or other offal, the bird proceeds to tear off the skin and flesh, devouring it piecemeal after the manner of a hawk, or at other times holding the object down with one foot, and with the other grasping the portion it was eating, after the fashion of ordinary parrots. The plan usually adopted on the stations for alluring this bird is to expose a fresh

Fig. 9.—The Mountain Nestor.

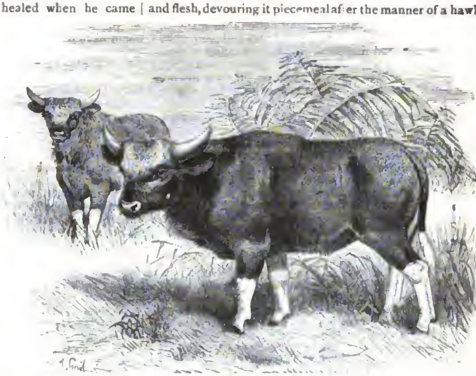


Fig. 10.—The Gayal.

published (in the *Zoologist* of the present month), it would seem, however, that the losses sustained by the attacks of the Kea are in some cases very serious.

sheepskin on the roof of a hut; and whilst it is engaged in tearing up the bait it is easily approached and snared."

10. *The Gayal* (*Bibos frontalis*).—In the mountainous districts of the oriental region three fine species of wild cattle occur which do not belong strictly to the genus *Bos*—the supposed progenitors of our domestic herds—but to a slightly modified form, *Bibos* of naturalists. One of these—the *Gaur*—inhabits the Ghauts of Central India, and is the well-known "Bison" of Anglo-Indian sportsmen. The Gaur is very intolerant of captivity, and although many attempts have been made to rear young specimens for transmission to Europe, none of them have ever proved successful. A second species of *Bibos*, the Bantang (*B. javanicus*), is found only in the hills of the Malay countries. It is more tractable, and examples of it have occasionally reached Europe alive, though they have not done well in this country.

Of the third *Bibos*, the Gayal (*B. frontalis*), we give a figure taken from a fine pair of these animals now in the Zoological Society's Gardens, which were received from the sister Zoological Society of Calcutta. The Gayal, as Jerdon tells us, in a state of nature inhabits the hilly tracts to the east of the Brahmapootra and at the head of the Valley of Assam, extending into the Mishmi Hills and adjacent ranges. It is caught and kept in captivity by the natives very extensively, and to this fact is no doubt due the comparative ease with which specimens of it are brought to Europe.

The Gayal breeds readily with the different forms of Domestic Ox. Many hybrids between the Zebu and Gayal have been produced in former years in the Zoological Society's Menagerie.

SHIPBUILDING A THOUSAND YEARS AGO

MR. COLIN ARCHER read an interesting paper on this subject at the recent meeting of the Institution of Naval Architects, as also at the York meeting of the British Association.

It is a well-known historical fact that as far back as the early part of the Middle Ages, the inhabitants of Scandinavia were a great seafaring nation: in many of the great battles fought between the chiefs and pretenders of that period—and they were not few—we find several hundreds of large war-ships ranged against each other. It seems to have been quite a common practice for the young chiefs, in order to relieve the monotony of life on shore, or to escape the consequences of some lawless act, to equip one or more ships, manned by their retainers, and to launch forth in quest of adventure, plunder, or "the bubble reputation." And these excursions were not always confined to home waters; they were frequently extended not only to the coast countries of the north of Europe, but also to the shores of the Mediterranean. Iceland was discovered about the middle of the ninth century by Norwegian adventurers, and there are good grounds for believing that an expedition starting from Iceland landed and established a colony in the present New England States nearly 500 years before Columbus lived.

But the descriptions which the old Sagas afford of the vessels in which these expeditions were undertaken, and these battles were fought, are very meagre. It was therefore looked upon as an event of great interest when, on excavating a large grave-mound near the entrance to Christiania Fjord, a ship, evidently from the Viking period, was discovered in a wonderful state of preservation. There is reason to believe that this ship, although comparatively small, does not differ materially in her manner of construction or in shape from the more powerful war-ships, or from those used for long voyages. She is probably a true model of the ships which carried Rollo and his brave followers to the coast of Normandy; and it may therefore be assumed that a brief description of her, as she now appears from a shipbuilder's point of view, may not be without interest.

It was not to be expected that a delicate structure such as this Viking ship could remain for eight or ten centuries buried many yards under ground without sustaining some damage, or that she should perfectly retain her original form. It is rather a matter of surprise that the damage is so small as it is. Thanks to careful handling and a judicious arrangement of supports, there is reason to believe that, apart from local strains and contortions of form, the hull as it now stands represents very closely the ship as she appeared when put into the ground. Mr. Archer has taken off her lines with as much accuracy as circumstances would permit, and, referring to these lines, he explains the chief peculiarities of the construction.

The principal dimensions are:—

	Feet.	Inches.
Length between the rabbets at gunwale	77	11
Breadth, extreme	16	7
Depth from top of keel to gunwale amidships	5	9

The vessel is clinker built, and the material all oak. There are sixteen strakes of outside planking, the ordinary thickness 1 inch, average breadth amidships 9½ inches, including 1 inch land. The lengths vary from 8 to 24 feet. The scantling is not, however, uniform throughout; thus the tenth plank from the keel is about 8 inches broad and 1¾ inches thick, and forms a shelf for the beam-ends. The fourteenth plank from the keel, or third from the top, is about 10 inches broad and 1¼ inch thick. This plank, which we may call the "main wale," is perforated with holes for the oars, sixteen on each side, about 4 inches diameter, and provided with a slit at the after and upper edge to allow the blades of the oars to be passed through from inboard. The two upper strakes are the thinnest of all, being scarcely more than ⅞ inch. The gunwale, 3 inches by 4½ inches, is placed in the usual manner inside the top strake. The boards are throughout united to each other by iron rivets about the thickness of an ordinary 3 inch spike, spaced from 6 to 8 inches, with large flat heads 1 inch diameter. The riveting plates are square or nearly so, ¾ inch. The nails are driven from the outside, except near the ends, where riveting inside would have been difficult from the sharpness of the vessel. The nails are here driven from the inside and riveted outside. The garboard strake is fastened to the keel with rivets of the same kind as those used for joining the strakes with each other.

The keel is of a peculiar shape; it is about 14 inches deep, of which 11 inches are below the rabbet, 4½ inches thick at the lower edge, and only 3 inches at the rabbet. The top of the keel is 7 inches broad, thus affording a large surface for the garboard strake, besides combining strength with lightness. Possibly also the increased thickness of the lower edge may have been adopted to improve weatherliness under sail. It is difficult to say where the keel ends and the stem and sternpost begin, as these run into each other with a very gentle sweep; but the piece of wood which may be called the keel proper is 57 feet long; to it are joined a short forefoot and heel piece by short vertical scarfs secured by double rows of rivets. These pieces again are fitted in a similar manner to the stem and stern-post. The posts are sided 3 inches, chamfered to 2 inches outside edge. They are 15½ inches broad outside the rabbet just above the scarf, decreasing in breadth upwards.

The framing of the bottom consists of grown floors extended in one piece from shelf to shelf. The average spacing in the body of the vessel is about 3 feet 3 inches from centre to centre, greater at the ends: there are nineteen frames in all. The floors are neatly finished, of a shape which combines strength with lightness and elasticity. The lower surface has a flat projection in which are holes for receiving the fastenings for the plank. The way these fastenings are managed is very peculiar. The

planks are evidently worked down from stout slabs, and in doing so a ledge an inch high has been left on the inner surface running along the middle of the plank. The floors are not fayed down on the boards; they have only two points of contact with them, the upper edges and the ledge above mentioned, in which are two holes bored transversely, one on each side of the timber. Through these holes and corresponding holes in a fore and aft direction through the timbers are passed ties made of the tough roots of trees. These ties are very slight, scarcely ½ inch diameter; they are crossed over the ledge on the board, only passing once through each hole. The ledge has been removed in the spaces between the timbers, so that the remaining parts under the timbers look like cleats fastened to the plank. With the exception of a nail driven through the "shelf" and riveted on the extreme end of the floors, these ties seem to be the only fastenings used at this part of the vessel. The floors are only about 4 inches diameter, a foot from the garboards, and taper, siding as well as moulding, down to 3 inches or even less at the shelf. They are not fastened to the keel.

As already stated, the beams, which are sided 7 inches, moulded 4 inches, rest on what Mr. Archer has called the "shelves," which however only differ from the ordinary planking by being ⅜ inch thicker, and of greater lengths, the longest piece being about 48 feet. The beam-ends also rest on the ends of the floor timbers. They are secured by knees extending down the ship's side from the upper edge of the "main wale" with an arm on the beam. These knees are fitted close to the planking at the side, and fastened with oak trenails. Being a little narrower than the beams, a ledge is formed on each side for the bottom boards or flooring, which is made to fit into these ledges from beam to beam, thus forming a continuous platform. A strip of wood is nailed on top of the beams in continuation of the knees where these are too short to welt from opposite sides. The beams are supported amidships by pillars resting on the throats of the floors. The top sides, consisting of the two thin boards already mentioned, are connected with the body of the ship by independent timbers intervening between the knees, and extending from the under side of the gunwale some distance down the side, but not so far as the platform. There are no timbers in the upper part of the vessel, overlapping or making a shift with the floors.[1]

It will be seen that by this system of construction the upper portion of the ship is altogether unconnected with the bottom part, so far as framing is concerned, an arrangement which would scarcely be safe where much ballast or a heavy cargo is carried on the ship's bottom. No doubt heavy weights when carried were placed above the platform, in which case there would not be the same tendency for the two sections to part company.

Perhaps the most singular part of this singular ship is the arrangement for stepping and supporting the mast. The step is a solid log of oak 11 feet long and 19 inches broad by 14 inches deep at the middle, tapering to the ends. It is counter-sunk over the throats of the floors, to which it is fastened by means of small knees on either side. From this trunk a branch grows out vertically in front of the mast and quite close to it. This branch, which is nearly 12 inches thick, is fastened to what Mr. Archer has called the "fish."

The fish is a ponderous piece of oak lying along the middle line of the vessel, on top of the beams, and extending over five spaces. It is 16 feet long, 38 inches broad, and 14 inches deep at the middle. This block is modelled so as to represent the tails of two fishes or whales resting on a flat slab or sole piece about 4 inches thick. The slab is counter-sunk over the beams and well

[1] This mode of binding the two sides together by means of beams half-way between gunwale and keel is still practised in the west and north of Norway. Even small skiffs are tied together in this way, loose thwarts being placed over the beams, only resting in a notch cut in the knees which secure the beams, while the floor-timbers merely butt up against the beams.

secured to them by knees. A large slice is taken off the back of the fish, the upper surface thus forming two planes inclining to either end. The extreme ends of the tails are only about 3 inches thick above the slab. A slot 5 feet 9 inches long and 12½ inches wide (the diameter of the mast) is cut in the fish from a point a little in front of the middle towards the stern. The mast is stepped through the forward end of this slot, and when erect kept in its place by a heavy slab fitted into the slot. In the end view this slab is shown with the after end raised level with the forward end. By removing the slab and slacking off the fore-stay the mast would be free to fall aft in the slot, and could thus easily be lowered. In order that the beam nearest the mast should not interfere with this manœuvre there is a depression in it which enables the mast to fall back the whole length of the slot.[1] There is a stanchion about 8 feet high, with a cross-beam at top in which are semicircular depressions for the spars to rest in when not in use. There have been three such stanchions.

The mast, which is 12½ inches diameter, has been cut about 10 feet from the foot. The extreme top of one of the spars found in the ship, corresponding in size to the part which remains, has rotted away; but if this spar, as seems probable, is the upper portion of the mast, the whole length may have been 40 feet. There is another spar which looks as if it might have been the yard. It is broken off near the middle, but Mr. Archer estimates its full length at 35 feet, diameter at slings 8½ inches, at arms 3½ inches. Abreast of the forward end of the fish, strong pieces of wood, one on either side, each with two circular sockets, are fitted down between the timbers just above the platform. Possibly one of these sockets may have served as a step for a squaresail boom. The other may have received a pair of shears to give elevation to the fore stay when raising or lowering the mast.

With regard to the rudder, a conical piece of wood sufficiently long to keep the rudder clear of the ship's side is fitted with its base to the outside planking; through a hole bored through the centre of the cone, and a corresponding hole in the rudder, a stout rope is rove, provided with a knot at the outer end and made fast inboard. This rope acts as a pivot, allowing the rudder to be twisted by means of the tiller fitted athwartships. An iron staple near the lower extremity of the rudder, and a small ring bolt at the upper end may have been fitted with guys leading aft to steady the rudder and keep it immersed when the ship was under way. The rudder-head or stem is round, 6 inches diameter. At the pivot it is 7 inches thick, thence decreasing in thickness downwards. The breadth is 15 inches at pivot, increasing to 22 inches at foot. Both edges are bevelled off, particularly the front one, which is reduced nearly to a feather edge. The rudder is all of one piece of wood.

The extreme ends of the vessel are unfortunately gone, so that it is not easy to see how she has been finished off here. The lower planking takes a very decided turn upwards as it approaches the ends, running in fact almost parallel with the posts. If therefore all the wood ends have joined the posts, these must have been very high. It seems not improbable that part of the planking has been received into a rabbet in the gunwale, or in a breast-hook connecting the gunwale with the stem or sternpost. This however is merely a conjecture.

If the old ship can be looked upon as a fair sample of the ships of her time, it is evident that shipbuilding a thousand years ago was something very different from what we now understand by that term. What strikes one most forcibly on seeing this vessel as she now stands is the extreme lightness of her scantling and the total absence of anything in the shape of lining, longitudinal stringers, or similar contrivances for giving what we

[1] In the Scandinavian languages the technical term for the framing which now takes the place of this colossal structure in our modern ships—the mast partners—is still *Fisken*, the fish.

should consider the strength and rigidity necessary in a sea-going vessel. It would however be unfair to compare her with a ship of modern build of the same size. Even the designation "ship," as applied to her, is apt to convey a false idea. She is in fact a very large sailing rowing-boat.

These ancient vessels may be considered as consisting of two distinct sections, each having its special use and function. The portion above the beams is the hold proper, the useful space. Here the crew had their abode, and here was carried probably all that the vessel had to carry, and this portion is comparatively strong. The material is no doubt here also of small dimensions, but what there is has been judiciously distributed, is of good quality, and has been well put together. It should also be remembered that the weight carried was small in quantity as compared to the carrying capacity, and consisted principally of live cargo, and this kind of loading is much less trying to a vessel in a seaway than a similar loading of dead weight would be. The lower portion of the ship, on the other hand, had a different kind of duty to perform. It had to supply the "form" necessary for small resistance and rapid locomotion, and to float the upper section: keeping this in mind it will be found that her construction gives evidence of a great deal of practical skill and ingenuity. Every part of the vessel is sufficiently strong for the duty expected of her, while at the same time economy of weight of material has been studied throughout. It will be seen that the weight of the superstructure is taken entirely by the floor timbers, the ends of the beams resting on them, while the beams are supported amidships by the props stepped in the throats of the floors. There would therefore be very little stress on the ties of the bottom planking, which latter, there being no counteracting pressure on it from the inside, would always tend to cling to the timbers by the pressure of the water outside. The only weight of any moment which would tend to separate the two sections of the boat is the mast, and this tendency is met by the "branch" of the step being secured to the "fish." Still there can be no doubt that this boat must have possessed a pliancy and mobility in a heavy sea which we should look upon as ominous in a modern sea-going craft. Her real safety consisted in a tough and elastic outer skin, which would be the more invulnerable from not being made unduly rigid at any point. Thus her apparent weakness was her real strength. Mr. Archer has not been able to discover anything deserving the name of a bolt in the whole structure. The stoutest iron fastenings are the rivets in the scarfs of the keel and the nails securing the inside knees, and they are no stronger than ordinary 4-inch spikes.

It seems probable that such a boat would be capable of great speed, even under oars alone; with a fair wind she must have been very fast. Mr. Archer has assumed a low water-line, and finds that at this trim her displacement is 994 cubic feet, or 28·4 tons; area of immersed midships section 24 square feet; extreme length on load-line, 73 feet 3 inches; and draft of water 3 feet 8 inches. Allowing 10 tons for her complement of 100 men with their accoutrements, leaves 18·4 tons for the vessel, with inventory, stores, and equipment, and this allowance is probably ample. The areas of cross-sections are obtained by multiplying the ordinates of the curve by 4 feet.

LEARNED SOCIETIES IN JAPAN

IT is now a little more than ten years since Japanese students began to flock in large numbers to the various schools of Europe and America, after the great revolution which completely altered the political, and in many respects the social, organisation of the country. Many of these young men travelled and studied at their own expense; but the majority was selected by the principal Government departments, and the expenses paid from the Imperial funds. For six or seven years the numbers continued without diminution; but soon after the commencement of the Satsuma rebellion in 1877, when the heavy strain on the Imperial Exchequer caused by the suppression of that outbreak began to be felt, it was decided to economise the public expenditure in various ways, and amongst others by reducing the number of those studying abroad at Government expense. The result of this measure, which was forced on the Ministers by unfortunate circumstances, was that many Japanese young men who spent some years in the principal educational establishments of western countries, returned to their own land with a sound training in their respective branches of study. It would not be desirable, even if it were possible, to enter here into the question how far they have fulfilled the hopes with which they were first sent abroad. Many of them have had brilliant careers amongst their foreign fellow-students, and, on the whole, we believe they have done as much as any body of English students, similarly placed, could have in the same time; but it is another question whether they are fitted to assume the places held by the foreign professors and instructors in the various educational institutions of the country. It was to this that the Government looked when they were first despatched to Europe; but, from a combination of causes, it is doubtful whether the laudable and patriotic desire to be, as far as possible, independent of extraneous assistance, has been so completely fulfilled as was originally anticipated.

One result has undoubtedly attended this great influx of men trained after western methods, namely, the thirst for scientific knowledge of all kinds amongst the educated classes in Japan. It is hardly an exaggeration to say that Japanese literature, as an indigenous product, is for the present almost in abeyance. If we examine the monthly catalogue of books for which licence to print is granted by the Censorate in the Home Department in Tokio, it will be seen that a very large proportion is composed of translations or adaptations of European or American scientific or literary works. Besides translations made at the expense of the public departments, we find private individuals throughout the country utilising their knowledge of a western language by translating from it, for the benefit of their countrymen. Thus, not to mention innumerable "Lives" of Wellington and Napoleon, or translations of "Gulliver's Travels," "Robinson Crusoe," and other books of this description, the works of Huxley, Carpenter, Peschel, Darwin, Tyndall, Quatrefages, Lyell, Buckle, Mill, &c., &c., have all been translated or adapted with more or less success for the Japanese reader. Societies, on the European model, have also been formed, and it is with these that we are chiefly concerned at present.

Centuries before the Royal Society of Great Britain was founded men interested in the pursuit of some study or accomplishment in Japan had formed themselves into societies, some of which still exist. Collectors of antiquities, of coins, of the handwriting of celebrated men of ancient times, met at stated intervals to exhibit and discuss the authenticity of their treasures; *go*-players had their own organisation, with branches in all the chief towns throughout the country, and headquarters in the capital, where the leaders met for trials of skill. These latter even had a kind of magazine in which problems for solution were set, and the moves in remarkable games recorded. These meetings generally took place in the evening, at some well-known house of entertainment. There was no formal reading of papers, with discussions afterwards; a member exhibited some new object, related briefly all he knew about it, and asked for any further information that could be afforded by those present. Frequently also these meetings were used for effecting

sales or exchanges amongst the members. Some of these old societies still flourish in undiminished vigour, unaffected by the changes which have passed over the country and altered all around them. Amongst these are the *Kō-butsu-sha*, or Antiquarian Society, the Numismatic Society, the Association of *Go*-players, and many of the old assemblies for literary and poetical contests. But the new era has been productive of societies of a more scientific kind, based on the models of learned associations in Europe and America. Founded by students fresh from abroad, they have received the support of men of wealth and eminence, and, judging from the experience of the past few years, they seem in a fair way to attain permanent success.

The most important of these associations is the Geographical Society of Tokio, which now numbers about 200 members. The subscriptions, which are very small, are largely increased by donations from the wealthy members. It is under the patronage of several of the imperial princes, and among its members are the chief personages of the Empire. The *Transactions* are neatly printed in small pamphlets of about 100 pages each, and contain much matter which would be valuable even to European geographers. With the exception of China, Japan is the only foreign country having intercourse with Corea. Our information respecting this peninsular kingdom is limited to the imperfect accounts of the Jesuit priests; but the Japanese Geographical Society has already had several interesting and important papers on the subject from its members. The difficulties of the language seriously restrict the circulation of these and other papers, but we believe the Committee are contemplating the publication of translations of their *Transactions*.

During his too brief stay in Japan as occupant of the Chair of Zoology in the University of Tokio, Prof. Morse of Salem, Massachusetts, was instrumental in establishing a Biological Society which attracted much attention. It is now being conducted successfully by Prof. Yatabe, a Japanese gentleman educated in the United States.

Another association, which is, we believe, unique among the societies of the world, is the *Kojunsha*, or Society for the Circulation of Knowledge. Its head-quarters are at Tokio, but there are branches in every town of importance in the Empire. It possesses a secretary and staff of clerks, and a member desiring to obtain information on any subject applies to the secretary. The latter has on his books the names of all the members likely to be able to satisfy the applicant, and immediately transmits the question to them. The answers are forwarded in due course to the inquirer, and should the subject be deemed by the Committee of sufficient general importance, the whole is printed in the weekly *Journal* of the Society. The pains which are taken to obtain satisfactory replies to queries are, we can vouch from personal experience, almost incredible. It is not surprising to learn that this Society has nearly 3000 members scattered throughout the Empire, and even in Europe and America. As a device for bringing together the active and inquiring minds of the country, it is almost unequalled. The subscription, which includes the use of reading-rooms and the numbers of the *Journal*, is about half-a-crown per month.

The Numismatic Society, to which we have already referred, is also very active. It publishes a periodical describing new and strange coins that have been exhibited at its meetings, and supplies other information interesting to collectors.

We need not refer here to the English and German Asiatic Societies founded in Yokohama and Tokio. They are under the control of foreign residents, their papers are in a foreign tongue, and, although their work has been most valuable, they are outside the scope of the present article. Nor need we give more than a passing reference to the innumerable political societies which have sprung up like mushrooms in all parts of the country during the past few years. If the objects of the promoters of these organisations were less palpably selfish, and more in accordance with their high-sounding titles, they would be very important instruments in the education of the people.

But we cannot pass over the latest scientific association of Japan. The Seismological Society, as its name indicates, is founded for the purpose of investigating volcanic and earthquake phenomena of all kinds. Japan is particularly well situated for this object. There are numerous active and extinct volcanoes throughout the island. Mild earthquakes are of very frequent occurrence, so that the student has not, on the one hand, to wait months for his subject, as in most parts of Europe, or, on the other, to run for his life when it does come, as in South America. This society was founded chiefly through the energy of its vice-president, Mr. Milne, professor of geology in the Engineering College at Tokio, who has long made seismic phenomena a special study. A Japanese, Mr. Hattori, himself a student of the subject, is President of the Association, which numbers many foreigners amongst its members. The Central Government have throughout taken a warm interest in the success of the Society, and have, we believe, placed the telegraph lines at its disposal, and ordered the local officials all over the country to report all occurrences connected with earthquakes and volcanic eruptions in their districts. A few months since, under the auspices of the Society, an exhibition of seismological instruments of various kinds—one of them as ancient as A.D. 126—took place in Tokio. The number of visitors in one day to the rooms was over 2000, a fact which attests the interest taken in this study by the Japanese. The *Transactions* of the Society are published in English in the *Japan Gazette* newspaper of Yokohama.

The army, navy, and other professions have their own societies and newspapers, very much as in England. One of the most curious of these class or trade journals is the dancing-girls' paper, containing portraits and biographies of the chief *danseuses*. We have not advanced so far yet in England as to have an organ-grinders' gazette!

On the whole it must be pronounced that the outlook for the propagation of scientific knowledge in Japan is hopeful; and there seems no reason to fear that science will suffer greatly after the approaching and inevitable departure of all foreign instructors in the country. They will leave behind men who, although possibly not such efficient teachers, are animated by all the thirst for knowledge that animates the bulk of scientific men in western lands.

NOTES

Dr. C. W. Siemens has received from the French Government a formal document nominating him "Officier de l'Instruction Publique," the nomination being accompanied by the insignia of the order, which corresponds, we believe, to the Prussian order "Pour le Mérite."

It is proposed to open an International Electrical Exhibition at the Crystal Palace in December.

The anatomical department of Edinburgh University has lost a valuable servant in the death, at the age of seventy, of Mr. A. B. Stirling, the assistant conservator of the Anatomical Museum. He was born in 1811 at Milngavie, Stirlingshire, where his father was a shoemaker. Stirling early evinced a decided liking for natural history studies; he was a born naturalist. His love of natural history brought him into contact with the late Prof. John Reid and Dr. Adamson of St. Andrews, who employed him to

arrange the University Museum there. In 1856 he was introduced to the late Prof. Goodsir, who recognised his aptitude for anatomical work, and saw in him one who would be a congenial helper in the work which he had in view; and Mr. Goodsir appointed him assistant conservator of the Edinburgh Anatomical Museum—a museum which he has enriched with hundreds of anatomical preparations (normal and morbid), and also many comparative anatomy specimens, which are all characterised by great taste in the way in which they are mounted. He soon acquired an extensive knowledge of anatomy, human and comparative; he had so remarkable a mechanical turn, and so inventive a mind, that he devised many new methods for preserving the human body for dissection, for mounting anatomical preparations, for cutting microscopic sections, and for mounting the same. He was an accomplished microscopist and a keen fisher, and this led him to take a great interest in fish, especially the Salmonidæ; and, when the "fungous disease" broke out amongst the salmon in the Tweed and other rivers, he investigated this matter, and communicated his results to the Royal Society of Edinburgh—results which are said to contain by far the best description yet given of the pathological conditions of this remarkable disease. Not only did Mr. Stirling encourage and aid others, but, in turn, he was the esteemed and highly valued friend of the late Prof. Goodsir and of Prof. Turner, both of whom gave him every facility for carrying on his investigations.

THE Royal Commission on Technical Instruction visited Saltaire and Keighley on Tuesday, and were present at the annual meeting of the School of Science and Art in the Keighley Mechanics' Institute. Mr. Slagg, M.P., speaking of the objects of the Commission, said that their great aim would be to develop a plan by which their system of primary education should be linked to a higher system, comprising a higher training and leading up to the highest scholastic education the country could afford. For his own part he did not see anything at the present moment in foreign competition to appal them in the slightest degree, and substantially he believed that they held their ground very well indeed. Mr. Samuelson, M.P., said that it was impossible that they as a nation could continue to hold the superior rank which they had taken among manufacturing countries if they did not cultivate the industrial intelligence of their population, and it was on that account that he thought the Commission would result in great good.

A REMARKABLE phenomenon occurred in New England on September 6, almost exactly similar to one that occurred in the same region on May 19, 1780. The *Springfield Daily Republican* describes it as follows:—In this city the day began with a slow gathering of fog from all the watercourses in the early hours, the thin clouds that covered the sky at midnight seemed to crowd together and descend upon the earth, and by sunrise the atmosphere was dense with vapour, which limited vision to very short distances, and made those distances illusory; and as the sun rose invisibly behind, the vapours became a thick, brassy canopy, through which a strange yellow light pervaded the air and produced the most peculiar effects on the surface of the earth. This colour and darkness lasted until about three o'clock in the afternoon, once in a while lightening, and then again deepening, so that during a large part of the time nothing could be done conveniently indoors without artificial light. The unusual complexion of the air wearied and pained the eyes. The grass assumed a singular bluish brightness, as if every blade were tipped with light. Yellow blossoms turned pale and gray; a row of sunflowers looked ghastly; orange nasturtiums lightened; pink roses flamed; lilac-hued phlox grew pink; and blue flowers were transformed into red. Luxuriant morning-glories that have been blossoming in deep blue during the season now were dressed in splendid magenta; rich blue clematis donned an equally rich maroon; fringed gentians were crimson in the fields. There was a singular luminousness on every fence and roof-ridge, and the trees seemed to be ready to fly into fire. The light was mysteriously devoid of refraction. One sitting with his back to a window could not read the newspaper if his shadow fell upon it—he was obliged to turn the paper aside to the light. Gas was lighted all over the city, and it burned with a sparkling pallor, like the electric light. The electric lights themselves burned blue, and were perfectly useless, giving a more unearthly look to everything around. The darkness was not at all like that of night, nor were animals affected by it to any remarkable extent. The birds kept still, it is true, the pigeons roosting on ridge-poles instead of flying about, but generally the chickens were abroad. A singular uncertainty of distance prevailed, and commonly the distances seemed shorter than in reality. When in the afternoon the sun began to be visible through the strange mists, it was like a pink ball amidst yellow cushions—just the colour of one of those mysterious balls of rouge which we see at the drug-stores, and which no woman ever buys. It was not till between five and six o'clock that the sun had sufficiently dissipated the mists to resume its usual clear gold, and the earth returned to its everyday aspect; the grass resigning its unnatural brilliancy and the purple daisies no longer fainting into pink. The temperature throughout the day was very close and oppressive, and the physical effect was one of heaviness and depression. What was observed here was the experience of all New England, so far as heard from, of Albany and New York city, and also in Central and Northern New York. In reference to this phenomenon the New York *Nation* suggests that it may be worth the while of weather-observers to note the approximate coincidence between the interval separating the two dark days in New England (May 19, 1780, and September 6, 1881) and nine times the sun-spot cycle of eleven years.

THE ceremony of cutting the first sod of the Giant's Causeway and Portrush Tramway was performed the other day at Portrush, in presence of the directors and a large company of the local gentry and visitors at Portrush. Interest was attached to the ceremonial owing to the fact that it is intended to work the tramway by electricity, the company thus being the first to introduce into the United Kingdom electricity as a motive power for tramway and railway propulsion. The chairman of the company, Dr. Traill, said that not many years would elapse before this dynamo-electric power would be supplied, not alone to tramways suitably situated for it, as this one undoubtedly was, but also to railways. To shareholders in a company such as this they could easily see what an important thing such a revolution in locomotive power would represent. The working expenses for haulage on a tramway such as theirs with horses would be about 11*d*. per mile, and by steam power about 7*d*. per mile, but there was every reason to suppose that the working expenses of their motive power need not reach 1*d*. a mile.

SOME time ago we gave an account of the nature and uses of celluloid. Among other things it may be used for preserving typographical *clichés* and stereotypes. The process employed for this purpose, we learn from *La Nature*, consists in taking an impression of the engraved block by means of a special cement, which receives the impression and rapidly hardens. After about twenty minutes the cement can support a pressure of 250 kilograms. The presses used to take the first impression ought to be heated; and the celluloid in sheet is then used to take the counter-impression from which to print. Celluloid shows the typographical reproduction of specimens of lace in a marvellous fashion, by the actual impression of the lace itself. *La Nature* gives an illustration of a piece of lace engraved in this manner, and the reproduction of the pattern is perfect.

A TELEGRAM from Constantinople of September 30 states that an earthquake had occurred at Changeri, in Anatolia, which

caused the death of eleven persons and great injury to the Grand Mosque and numerous dwelling-houses. The amount of damage done in the neighbouring villages is not known.

A GEOGRAPHY of the almost unknown kingdom of Corea has been compiled by a member of the suite of the Japanese envoy to that country. Several valuable papers containing accounts of travels in Corea have been read before the Geographical Society of Tokio, and have appeared in its *Transactions*. As they are written in Japanese they are unfortunately all but inaccessible to European geographers.

THE Prefect of the Seine has established a course of six lectures for the teaching of micrography. An examination has been instituted for inspectors intrusted with the care of detecting trichinæ in the substance of pork and ham of American or German origin.

A CURIOUS experiment will be tried this week at La Villette gasworks, Paris. Two balloons of equal size will be sent up at the same time; one of them will carry an experienced sculler, who is confident that he will produce some effect with a long oar of his invention.

UNDER the title of "School Physical and Descriptive Geography" Mr. Stanford has issued a smaller and cheaper edition of the late Keith Johnston's "Physical, Historical, Political, and Descriptive Geography," reviewed in these pages at the time of its appearance. In the school edition the historical sketch and the elaborately-printed maps have been omitted, while all the strictly geographical information has been retained. In this form it ought to find wide acceptance among all teachers who aim at making geography both interesting and thorough. No better text-book could be recommended.

THE subject of the address by Shadworth H. Hodgson, LL.D., before the Aristotelian Society on Monday evening will be "The Practical Bearing of Speculative Philosophy."

WE have received from Rothschild of Paris an interesting little volume on Pisciculture in France. It consists of two parts—Pisciculture, Fluvial and Maritime, by Jules Pizzetta; and Oyster-Culture, by M. De Bon.

IN its summary of colonial intelligence the *Colonies and India* mentions the discovery of a valuable coal-seam near Victoria, Huon, Tasmania, which has been traced on the surface for about twenty yards, and increased in width from three to four feet, when it was lost in a hill. The coal has been tried and found to be of good quality.

A VALUABLE archæological discovery, which may be said to equal that of the celebrated Kertch antiquities at the Hermitage of St. Petersburg, has recently been made near the Cossack village of Sewersk in the Sakuban district, in one of the *kurdans*, *i.e.* the old tombs, in the steppes of Southern Russia. A number of objects were found, but special attention was drawn to two glass vessels, unfortunately broken, but the pieces of which still give evidence of their remarkable ornamentation. They are profusely covered with gold, the hoops containing large rubies and bearing golden chains, by which heart-shaped pearls are suspended. Another object of cylindrical shape, evidently a cup-holder, consists of pure gold, and shows two griffins in bas-relief. Another important object is a gold plate six inches in diameter, with a fine bas-relief representing a whole episode. M. Felizin, an eminent Russian archæologist, is of opinion that the tomb in question must have been that of an important personage of the Bosphorean kingdom, and that its origin dates back as far as the period of King Perisad II., who began to reign in the year 284 B.C. A gold coin which was found confirms this view.

AN important discovery of very good rock-salt, affording a sheet seventy-five feet thick, was made some days ago in the district of Bakmut, in the Russian government of Ekaterinoslav, at a depth of 430 feet. The discovery was made according to the indications of the geologist, Prof. Erofeeff.

THE anniversary address of the Hon. Prof. Smith, president of the Royal Society of New South Wales, contains an interesting sketch of the history of the Society, both under its old name of Philosophical Society as well as under its present designation.

MESSRS. BLACKWOOD AND SONS have issued a twelfth edition of the "Elements of Agricultural Chemistry and Geology," by the late Prof. J. F. W. Johnston and Dr. C. A. Cameron.

IN the report sent us of the meeting of the Natural History Society of the Friends' School at York, and printed among our Notes a fortnight ago, the Rev. T. A. Preston is referred to as science master at Marlborough College. Of course this is a mistake; Mr. G. F. Rodwell has long held and still holds the post referred to.

THE additions to the Zoological Society's Gardens during the past week include a Tennant's Squirrel (*Sciurus tennanti*) from Ceylon, presented by Mrs. S. A. Cottrell; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mr. J. N. Palmer; a Chacma Baboon (*Cynocephalus porcarius*) from Sout Africa, presented by Mr. W. H. L. Long; a Leucoryx Antelope (*Oryx leucoryx*) from North Africa, presented by Mr. John M. Cook; two Leopards (*Felis pardus*) from Ceylon, presented by Mr. Eustace L. Burnside; a Green Lizard (*Lacerta viridis*) from Jersey, presented by Mr. James Thorn; a Tarantula Spider (*Mygale*, sp. inc.) from California, presented by Mrs. John Leechman; five Robben Island Snakes (*Coronella phocarum*) from South Africa, presented by Rev. G. H. R. Fisk, C.M.Z.S.; two Greater White-crested Cockatoos (*Cacatua cristata*) from Moluccas; two Common Cormorants (*Phalacrocorax carbo*), British, deposited; two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, a Nose-horned Viper (*Vipera nasicornis*), a Crocodile (*Crocodilus*, sp. inc.) from West Africa, purchased.

OUR ASTRONOMICAL COLUMN

COMET V., 1863.—With reference to a remark in this column at p. 111 of the present volume of NATURE, suggesting that a further and more minute discussion of the elements of this comet might be desirable, Prof. Valentiner, director of the Observatory at Carlsruhe, has been good enough to draw our attention to a memoir by himself upon the subject which we had overlooked; it is entitled "Determinatio orbitæ Cometæ V. anni 1863," and was published at Berlin in 1869. The observations, about 130 in number, extend from 1863, December 28, to 1864, March 1, and Prof. Valentiner forms nine normal positions upon them. The perturbations of the earth and Jupiter are taken into account (the comet having approached the former at the end of January within about 0·18) and the following parabolic elements result:—

Perihelion Passage, 1863, Dec. 27·79992 M.T. at Berlin.

Longitude of perihelion	60°	24′	26″·4	M. Eq.
" ascending node ...	304	43	23·2	1864·0
Inclination	64	28	44·2	
Log. perihelion distance	9·8873326			

Motion—direct.

The agreement with the observations is so close as to prove that the orbit did not sensibly differ from a parabola; the conjectured identity with the comet of 1810 is therefore shown to be inadmissible, notwithstanding the striking similarity of the elements, as will appear from the comparison at p. 111.

THE NEW COMET.—Mr. S. C. Chandler, jun., has telegraphed to Lord Crawford's Observatory approximate elements of the comet discovered by Mr. Barnard last month, from which it appears that the orbit does not resemble that of any which has

been previously computed. Expressed in the form usual in our catalogues the elements are:—Perihelion passage, September 15·01 G.M.T.; longitude of perihelion, 250° 4′; longitude of ascending node, 260° 43′; inclination, 72° 33′; log. perihelion distance, 9·70535; motion, retrograde. The intensity of light is diminishing.

MINIMA OF ALGOL.—The under-mentioned Greenwich times of minima of this variable are from Prof. Winnecke's ephemeris, in the computation of which correction depending upon recent observations has been applied:—

		h. m.			h. m.			h. m.
Oct. 9	..	15 52	Nov. 1	...	14 23	Dec. 11	...	17 48
12	...	12 41	4	...	11 12	14	...	14 37
15	...	9 30	7	...	8 1	17	...	11 26
18	..	6 19	21	...	16 5	20	...	8 14
			24	...	12 54	23	...	5 3
			27	...	9 43			
			30	...	6 32			

There would appear to have been perturbations in the period during the last few years which are not reached even by Prof. Schönfeld's formula involving two inequalities, which would make the above times about thirty-five minutes later.

A PROBABLY VARIABLE STAR.—Prof. Pickering notifies his observation of a red star, with banded spectrum, the place of which on September 14 was in R.A. 16h. 31m. 32s.; Decl. + 72° 32′. On September 17 its magnitude was 8·6. It is not found in the "Durchmusterung," nor in Federenko, Schwerd, or other circumpolar catalogue. Its variability is therefore suggested.

CERASKI'S VARIABLE, U CEPHEI.—Mr. Knott informs us that he obtained a good observation of the minimum of Ceraski's variable of short period on the night of October 2; time of min. 11h. 47m. G.M.T., mag. 9·2. Prof. Schmidt's ephemeris in *Astron. Nach.*, No. 2382, has 11h. 37·5m. The star did not fall quite so low, as in the minima which Mr. Knott observed in March, April, and May last.

[ERRATUM.—In last week's "Astronomical Column" (p. 520), *for* "add *p*" *read* "add P."]

CHEMICAL NOTES

MM. SCHUTZENBERGER AND COLSON describe (*Compt. rend.*) several new compounds of silicon. When crystalline silicon is strongly heated in a current of carbon dioxide the compound $(SiCO)_x$ is produced. When nitrogen is passed over a hot mixture of silicon and carbon $(Si_2C_2N)_x$ is formed. The authors regard these compounds as the oxide and nitride respectively of the radicle *carbo-silicon* $(Si_2C_2)_x$. The nitride of silicon $(Si_2N_3)_x$ is also described: it is obtained by the direct union of nitrogen and silicon.

IT is well known that certain metallic chlorides, *e.g.* sodium chloride, are precipitated from aqueous solution by hydrochloric acid; attention has been drawn in these Notes to recent work of Ditte and others on this subject. M. Sabatier describes several hydrates of ferrous and ferric chloride (*Compt. rend.*) produced by this general reaction.

MANY years ago Graham drew attention to the change in properties produced in certain oxides by the action of heat, *e.g.* ferric oxide is soluble in hydrochloric acid, but when strongly heated it becomes almost insoluble. This "department of corpuscular philosophy"—to use Graham's phrase—has not been much studied. The experiments detailed in *Archiv Nêerland* by M. van Bemmelen form an interesting contribution bearing on this subject. It is shown that the amount of water of hydration taken up by the oxides of tin, silicon, and manganese at the moment of the formation and precipitation of the hydrates of these oxides from aqueous solutions, is dependent on the molecular state, *i.e.* on the as yet unknown conditions of molecular equilibrium, of the solid hydrates. The molecular state being the same, the amount of water of hydration rises with temperature and humidity of the surrounding air; to each temperature and degree of humidity corresponds a certain equilibrium of oxide and water; the relations between the weights of the oxide and water are generally too complex for expression by a simple formula. From an examination of the phenomena attending the action of the amorphous hydrated di-oxides of the above-named elements on acids, alkalis, and salts, M. van Bemmelen concludes that weak double compounds are produced, but that these are very easily dissociated; the amount of dissociation varying with the chemical nature and the mass of the reacting substances, and with the temperature. In most cases stable compounds are produced simultaneously with these series of unstable and largely-dissociated compounds. The formation and dissociation of such unstable compounds depend also on the conditions of molecular stability of the hydrated oxides themselves. By arranging these conditions so as to insure considerable molecular stability—*e.g.* by heating the hydrates—the power of forming the unstable compounds is much diminished. That a force of the same nature as chemical affinity is concerned in the formation of some of these weak compounds is shown by the decomposing action exerted by hydrated MnO_2 on the stable compounds K_2SO_4, KCl, and KNO_3, compounds which do not show signs of dissociation in aqueous solution. M. van Bemmelen would thus extend the sphere of chemical phenomena, and would see no sharp division line between the actions of the so-called physical forces—adhesion, absorption, &c.—and the force of chemical affinity.

AN ingenious method for determining the total solid matter in solution in different waters is described in *Chem. Soc. Journal* by Dr. Mills. The method is based on the fact that if a small glass bead with an attached weight is allowed to ascend in a saline solution of known strength, it will rise more slowly, the greater the amount of solvent present. Experiments are given showing that the rate of ascent is also dependent on the nature of the soluble matter, *i.e.* on the viscosity of the solution. For detecting variations in the solids in the same water, for preparing standard solutions, &c., the bulb method is likely to be useful. Experiments detailed in the same paper lead Mills to regard the specific gravity of a potable water as a direct indication of the quantity of total solids in solution.

ANALYSES of the mud deposited round the Buxton thermal spring, by T. C. Thresh (*Chem. Soc. Journ.*), show that when dried at 120° this mud contains about 71 per cent. Mn_3O_4, with oxides of Pb, Cu, Fe, Al, Zn, Ba, Sr, Mg, and Mo, and closely agrees in composition with many specimens of "wad" or "bog manganese." Analyses of the gas evolved at the spring and of the gases dissolved in the water closely confirm those made by Playfair in 1852: the gas evolved at the spring consists of about 99 per cent. nitrogen and 1 per cent. CO_2, that dissolved in the water of about 60 per cent. N and 40 per cent. CO_2. The water in the baths contains as much gas as could be forced into water at a pressure of 1·64 atmospheres.

A LONG and important paper by W. H. Perkin, on "Isomeric Acids obtained from Coumarin and the Ethers of Hydride of Salicyl," appears in the same number (August) of the *Chem. Soc. Journ.* Perkin has obtained two series of compounds, differing in properties, but generally convertible, one into the other, by the action of heat. He thinks that the ordinary theory of isomerism, according to which this phenomenon is traceable to the occupation of different relative positions by the atoms in two molecules, fails to explain the cases of isomerism now described by him. He favours the view that the atoms in the molecules of any pair of the newly-described compounds occupy the same relative positions, but are at different absolute distances from each other. It is, however, to be remembered that the present theory of isomerism is applicable only to gaseous molecules; the molecular phenomena of liquid and solid bodies are too complex to find, as yet, any general explanation. Perkin's new compounds seem to belong to this rapidly-increasing group of "physical isomerides," *i.e.* to liquid or solid bodies whose chemical properties are to be traced to the binding together of molecular groups, the individual members of which occupy relatively different positions, and which groups react as chemical units. The facts concerning molecular volumes of metameric compounds are also, on the whole, opposed to that theory of isomerism favoured by Perkin in his important paper.

A SERIES of papers on the photo-chemistry of silver bromide by Herr Eder has appeared in *Chemisches Centralblatt.* It is shown that silver bromide prepared with an excess of silver nitrate is much more sensitive towards light than when prepared with excess of potassium bromide, provided the silver bromide is disseminated through an indifferent substance, *e.g.* collodion pyroxylene. When disseminated through an easily oxidisable substance, *e.g.* gelatin or gum, silver bromide prepared with a slight excess of soluble bromide is from four to six times more sensitive than when disseminated through indifferent collodion with excess of silver nitrate. An emulsion of silver bromide in gelatin with a slight excess of the soluble bromide after several days digestion at 30°-50° becomes much more sensitive than any

other known body. Herr Eder regards the photo-chemical decomposition of silver bromide as the result of partial reduction with loss of bromine.

THE GERMAN ASSOCIATION

THE fifty-fourth meeting of the Association of German Naturalists and Physicians was held at Salzburg on September 18–24. The number of Members and Associates in attendance was 760. There were also present Foreign Members from Switzerland, the Netherlands, Russia, Denmark, and Japan. The first general meeting, on Sunday, September 18, was opened by the First Secretary, Dr. Günthner (Salzburg), who in his hearty address of welcome mentioned the fact that Salzburg was the last retirement of the celebrated physician and naturalist, Theophrastus Paracelsus. After short addresses given by the Governor and Burgomaster, Prof. Pettenkofer (Munich) read a paper "On the Soil and its Connection with the Health of Man." He pointed out that it was previously believed that the state of the air and water exerts an important influence upon the origin and propagation of epidemics, but this view could not be proved by experiments recently made. The contamination of air and water is caused by products of decomposition of bodies putrefying on or in the soil. The progress of epidemic diseases, especially of cholera, is influenced mainly by the soil. The immunity of special localities against cholera is shown by the example of Lyons, which, notwithstanding communication with infected places, remained free from cholera, though filtered Rhone water was used there. Versailles and Salzburg also were exempt from this disease. It is now generally assumed that cholera is due to the action of schizomycetes, which develop at localities where the soil is impregnated with decomposing organic bodies. The contamination is drawn up by diffusion through the porous soil into the interior of houses, where it becomes dangerous to the health of man.

On Monday the work of the sections was commenced. There were twenty-three sections, eleven of them medical. On Tuesday an excursion was made to Reichenhall (Bavaria), with its salt-mines, where the Congress was addressed by Graf Pestallozza. On Wednesday the second general meeting was held. Prof. Weismann (Freiburg-im-Breisgau) read a paper on the duration of life. After enumerating many examples of longer and shorter duration of life among animals, he pointed out that size, constitution, temper, sex, and growth are not critical for the duration of life. In general the duration of life of an individual represents the minimum of time necessary to insure the existence of the species; it is governed by adaptation and heredity. The death caused by wasting and consumption of the cells, of which the (animal) body is composed, is the result of adaptation. The capacity of unlimited life has been lost, since it has become useless. There is no death at the division of lower animals (Amœba). In higher animals the propagating cells are separated from the somatic cells; only the former preserve unlimited productiveness. The limitation of individuals in time and in space is based on the same principle. At the same meeting Prof. Meyners (Vienna) gave an address on the laws which govern human thoughts and actions. In the conclusion of his very interesting discourse, in which he mainly dealt with feelings, sensations, and the experiments of Munk and Goltz, he expressed the opinion that the phenomena of bodies do not disclose to us their essence, and that there is only a phenomenon of freedom of will. Eisenach (Thuringia) was chosen as the town in which the fifty-fifth meeting of the Association should be held.

On Thursday an excursion was organised to Zell-am-See. On Saturday the third general meeting was held. Prof. Oppolzer (Vienna) read a paper on the question: Is Newton's law of gravitation sufficient for the explanation of the motion of heavenly bodies? Are there reasons for regarding it only as approximately true? In consideration of the theories of the moon, of Mercury, and of Encke's comet, he cannot find the theories based on Newton's law in its present form sufficient, but it would suffice under the (hypothetical) assumption of a cosmic matter surrounding the sun. After an address given by Dr. Kirschensteiner (Munich), on Theophrastus Bombastus Paracelsus, the sitting was closed by Dr. Günthner. We give a list of the papers read in the sections of Natural Science.

Section II. Physics: Walter (Tarnowitz), on the molecular kinetic laws of specific heat and the heat of vaporisation of bodies in different states; Sacher (Salzburg), on a direct measure of the attraction between earth and a determined electric current; Kurz (Augsburg), on dispersion of light and measuring the index of refraction; Spörer (Potsdam), results obtained by observations of the sun; Grunmach (Berlin), on the electro-magnetic rotation of the plane of polarisation of radiant heat; Grunmach (Berlin), comparisons of mercury-thermometers with air-thermometers; Sacher (Salzburg) demonstrated some new physical experiments relating to the theory of the formation of the earth (balls of sulphur and spermaceti with crater-formations); Waltenhofen (Prague) spoke on his apparatus for demonstration of the different action of hollow and solid electro-magnets; Günther (Ansbach), on the parallelogram of forces.

Section III. Chemistry: Brühl (Lemberg), on the connection between the optic and thermic properties of liquid organic bodies; Brauner (Prague), contributions to the chemistry of the rare earths, and on the progress of the system of periodicity of elements; Schwarz (Gratz), short communication on the preparation of nearly perfect alum-cubes by a new method; Zorn (Heidelberg), on hyponitrous acid; Bernthsen (Heidelberg), on the nomenclature of the proper derivates of carbonic acid, taking special notice of isomers.

Sections IV. and V. Geology, mineralogy, palæontology, geography: Bernáth (Budapest), on the mineral waters of Hungary; Gümbel (Munich), on the geological structure of the Untersberg (near Salzburg); Hauer (Vienna) presented a new geological map of Montenegro (designed by E. Tietze); Zittel (Munich), on Spongiæ as rock-forming materials, and on Plicatocrinus; Baltzer (Zurich), on curved strata; Neumayer (Vienna), on fresh-water Conchylia from China; Alth (Krakau), on the Jurassic formation of Niezniow; Hauer (Vienna), on the Arlberg; Tschermak (Vienna), on the definition of species in mineralogy; Hoernes (Gratz), on earthquakes in general; Woehner (Vienna), on the earthquake of Agram; Richter (Salzburg), on observations made at the Obersalzbach glacier; Doelter (Gratz), on the Cape Verde Islands; Dücker (Bückeburg) on the occurrence of petroleum in Northern Germany.

Section VIII. Botany: Kraus (Triesdorf), communications on the sap-pressure of plants; De Bey (Aachen), report on five new and peculiar genera (Coniferæ) of the Aachen chalk-flora; Holzner (Weihenstephan), on agrostological theses; Hildebrand (Freiburg-im-Breisgau), some observations on the flowering and the fruits of plants; Woronin (St. Petersburg), contribution to the knowledge of Ustilagineæ; Kirchner (Hohenheim), on the longitudinal growth of plants.

Sections VIII. and IX. Zoology, comparative anatomy, entomology: Troschel (Bonn), classification of Gastropods; Fraisse (Leipsic), on cell-division and free nucleus-formation; Weidersheim (Freiburg), on the genesis of Jacobson's organ; Grobben (Vienna), on the variation of generations of Doliolum.

BIOLOGY AS AN ACADEMICAL STUDY[1]

I.

IT is told of the late Dr. Norman Macleod that, on paying his first visit in his first parish, he was peremptorily desired to sit down and "go over the fundamentals." I feel that some such demand may, not unreasonably, be made of me to-night.

Five-and-twenty years ago one's position in this respect would have been a comparatively easy one, for then biology may be said to have had no "fundamentals" at all. In spite of the labours of Buffon, Erasmus Darwin, and Lamarck, the great bulk of naturalists at that time believed in the immutability of species; as a natural consequence botany and zoology remained mere "classificatory sciences," and the extraordinary facts of comparative anatomy, of embryonic development, of geographical distribution, of palæontology, were incapaple of rational explanation. Indeed, classification itself was nothing more than a logical expression of likenesses and unlikenesses, and was devoid of all real meaning.

But with the publication of the "Origin of Species," in 1859, a better day dawned for biology. The whole history of science has been a succession of attempts to bring group after group of natural phenomena within the scope of some natural law; and Charles Darwin's great service to science lies in the fact that, although not himself the discoverer of the doctrine of descent, he succeeded, by the immense array of well-arranged facts and sound generalisations contained in his epoch-making book, in

[1] Inaugural Lecture delivered in the University Library, May 2, 1881, by T. Jeffery Parker, B.Sc., Lond., Professor of Biology in the University of Otago.

bringing those natural phenomena which have to do with living things within the all-embracing law of evolution, thus making belief in the theory of special creation once for all impossible to the student of nature.

One may say then that since the publication of the "Origin of Species" evolution has taken its legitimate place as the central doctrine of biology, the key to the infinite number of problems with which the study of animals and plants brings us face to face. Without evolution these problems are incapable of explanation, and any attempt to explain them is little better than a roundabout acknowledgment of ignorance; but with the doctrine of descent as a standpoint, problem after problem yields to patient investigation, biology thereby gradually growing into a perfect and harmonious whole, as did astronomy when once the Law of universal gravitation was established.

Not that the real mystery of things is in any way diminished by this, any more than by other great discoveries. As Herbert Spencer finely says: "Positive knowledge does not, and never can, fill the whole region of possible thought. At the uttermost reach of discovery there arises, and must ever arise, the question, What lies beyond? As it is impossible to think of a limit to space, so as to exclude the idea of space lying outside that limit, so we cannot conceive of any explanation profound enough to exclude the question, What is the explanation of that explanation? Regarding science as a gradually increasing sphere, we may say that every addition to its surface does but bring it into wider contact with surrounding nescience."

But the fact that no explanation of natural phenomena can ever be final has no right to diminish our profound thankfulness for every proximate explanation which the genius of a Newton, a Dalton, or a Darwin gives us. To the true man of science these explanations come like a revelation, and he feels that his most cherished beliefs, his most ingrained prejudices, must be brought into harmony with the new light that is in him, or be cast aside as no longer tenable.

A few years ago—even at the time when this University was founded—something more than a bare statement of belief in evolution would have been required from a professor of biology giving his inaugural lecture. For then the doctrine of descent was only just emerging from the fiery trial through which all great truths, scientific or otherwise, have to pass, and it was honestly believed by many estimable persons that "Darwinism" was in direct and necessary opposition to religion and morality, and was the secret ally of atheism, socialism, and the like. But, like the fundamental doctrines of astronomy, physics, and geology, evolution has survived all attacks: I believe I am correct in saying that there is now not a single naturalist of any repute, under the age of sixty, who is not also an evolutionist; indeed, with Louis Agassiz and Von Baer, intelligent opposition to the general doctrine of transformism is practically dead.

Even among the non-scientific public, opinion has undergone a wonderful and rapid change. An evolutionist is no longer looked upon as a dangerous visionary; it is no longer thought necessary to hold "that nature's ancient power was lost" when she had to do with living things, and that the power which could form worlds out of a nebula was unable to evolve a horse from a hipparion, or even a speck of living protoplasm from the elements of the primæval sea.

Under these circumstances it would be superfluous, almost impertinent, for me to make any attempt to repeat the arguments which go to show that the animals and plants living on the earth at any period of its history are the lineal descendants of those which existed during the preceding period, and that the origin of any living thing by direct creation is, in the first place, entirely unsupported by evidence, and, in the second place, unthinkable. I proceed, therefore, to the main subject of this lecture—the position which biology should occupy in the curriculum of our schools and of our University; in other words, its place as one of the natural sciences in a rational scheme of education.

Educational subjects may be divided into two classes, the directly educational—those which serve as a true discipline, which train the mind, leading to clear thought, accurate reasoning, and a high intellectual tone; and the indirectly educational, which primarily serve to impart a certain amount of useful information, and only secondarily, by interesting the student and starting him off on a certain track of thought, serve as an actual means of mental culture. Perhaps the best examples of the two classes are furnished by mathematics on the one hand, and on the other by English history as usually taught in schools. A boy who has once grasped the idea that two and two make four and can never by any possibility add up anything else, has made a long stride in his educational career; but the boy who learns that the battle of Hastings was fought in the year 1066, or that Henry VIII. had six wives, has simply gained two comparatively unimportant concrete facts, the possession of thousands of which would never make him anything more than a well-informed person.

According to the theory of education which was almost universal in the last generation—the English public school system—there were two educational subjects, and two only, Greek and Latin, perhaps with "a shadowy third" in the shape of mathematics, but certainly nothing further than that. As a natural reaction against this time-honoured method of trimming down all minds to one dead level of scholarly dulness came the modern private school system, the principle of which is to try and cram into a boy's head a little of all the subjects of which it is supposed he ought to know something when he arrives at man's estate—divinity, Latin and Greek, modern languages, mathematics, natural science, history, geography, drawing, music, and even bookkeeping. The wretched child is "everything by starts and nothing long"; his masters, chosen for knowing something of as many as possible of these subjects, are usually eminently superficial, and he leaves school well informed perhaps, but profoundly and distressingly ill-educated.

The private school system is now, very naturally, producing in certain quarters a counter-reaction towards the exclusively classical and mathematical method of education, the plea being that the modern plan has been tried and found wanting, that neither natural science nor any of the other recent innovations have any direct educational value whatever, and that these subjects should therefore never form more than a very subordinate part of either a school or a university course.

This cry for a return to the old paths has lately found expression in an article by Dr. Karl Hillebrand,[1] who, however, makes certain very important concessions to his opponents. In the first place, what he is fighting against is not so much scientific education—I mean instruction in the natural sciences—as superficial education; and in this every honest teacher of science will be at one with him. Then again he advocates the postponement of the study of Latin grammar—the chief instrument of culture in his eyes—to the age of twelve or thirteen, and the employment of the first three years of high school life to training the powers of "observation, comparison, memory, and all the elementary functions of the understanding." In this also the advocate of science teaching and the opponent of the English public school system in its purity will be altogether in accordance with Dr. Hillebrand. But when he goes on to advocate as the best training for these "elementary functions of the understanding" the learning of texts and dates by rote, and, by way of science, the "simple classifications of zoology and botany," illustrated by the "exhibition" of real animals and plants, one cannot but wish that before printing such crudities he had tried to understand in what the elementary teaching of science really consists, and how far such teaching would supply the training in observation, comparison, memory, and so forth, to which even he would devote the earlier years of school life. To him, as to many, strict teaching means classical and mathematical teaching, and instruction in science is, if educational at all, only indirectly so.

This opinion as to the educational value of natural science arises, I am inclined to think, from an utter misconception as to what is meant by science teaching: by assuming, in fact, that science can be taught by the ordinary educational apparatus of books and lectures. The fallacy of this is only now beginning to be perceived, even by professed teachers of science. It is true that the chemists have long had their laboratories and the human anatomists their dissecting-rooms; but the notion that no course of lectures on physics, biology, or geology is complete without a corresponding course of practical work, is the product of the last few years, and is even now unrecognised in some British universities and in the large majority of schools.

And yet, one would think, nothing could be more obvious. The whole end and aim of science teaching is to bring the student into direct contact with nature; to insure his knowing, as he knows his multiplication table, the main laws upon which natural phenomena depend, and to make him see, without any possibility of mistake, the relation of those laws to the facts of the universe as he is able to observe them. What would be thought of a mathematical teacher who relied entirely on lectures, and never

[1] "Half-Culture in Germany," *Contemporary Review*, August, 1880.

dreamed of insisting that his pupils should apply what he had taught by working out examples for themselves? Or what of a teacher of art who ignored the necessity of making his students draw or paint? Every one sees the necessity of practical, and the uselessness of exclusively theoretical teaching in these instances, yet the fact is generally ignored that the case is precisely the same with scientific subjects, and that a man who lectures to beginners day after day and year after year on, for instance, the intricacies of animal structure and the problems connected therewith, without making his students see, by actual dissection, what an animal is, is in great measure spending his strength for naught.

Until this important fact is recognised and proper provision made for it, natural science never will and never can be a power in education. As Mr. Matthew Arnold puts it, "To say that the fruit of classics, in the boys who study them, is at present greater than the fruit of the natural sciences; to say that the realists have not got their matters of instruction so well adapted to instruction as the humanists have got theirs, comes really to no more than this: that the realists are but newly-admitted labourers in the field of practical instruction, and that while the leading humanists . . . have been also schoolmasters, and have brought their mind and energy to bear upon the school teaching of their own studies, the leaders in the natural sciences . . . have not. When scientific physics have as recognised a place in public instruction as Latin and Greek they will be as well taught."[1]

When these remarks were written (in 1868) they were applicable to science-teaching not only in schools, but also, in great measure, in universities and colleges. But since that time great changes have taken place, and in biology, of which science alone I am competent to speak, the improvement is due, first of all, to my honoured master, Prof. Huxley, and next to his co-worker, Dr. Michael Foster, both of them brilliant examples of the fact that an eminent man of science may be at the same time a laborious practical teacher. The classes begun by Prof. Huxley, with the co-operation of Dr. Foster, at South Kensington, and since continued at the School of Mines by Prof. Huxley and Mr. Thiselton Dyer, at Cambridge by Dr. Foster and his pupils, at Oxford and University College, London, by Prof. Ray Lankester, have now fairly put the teaching of biology upon a sound footing, and may be said already to have proved the value of that science as a true mental discipline, an educational instrument of very high order.

At any rate this is proved as far as University education is concerned. The battle has still to be fought in the secondary schools, and, as every one must see, the circumstances there are so different that victory in the one case is no criterion of victory in the other. It is evident, in fact, that the strict training in observation and experiment, without which, I cannot insist too often, science teaching is valueless as a mental discipline, is very difficult of application in schools, and that the consequences of setting a large class of young boys to make oxygen, or take a specific gravity, or cut up a rabbit each for himself, might prove rather subversive of order than conducive to improvement. But it has been amply proved that there is no difficulty in the case of senior boys taken in comparatively small classes; and even in large classes the practical teaching of elementary botany is quite feasible, as is shown by the experience of our own High School. Botany, indeed, lends itself more than any branch of science to school-teaching, from the simple fact that by its means the pupil can be brought face to face with Nature with comparatively little trouble, with no apparatus beyond a pocket-knife, and perhaps a simple magnifying-glass, and with no mess unremovable by a duster and broom.

For these reasons I am inclined to think that botany should be made the staple science subject for the junior classes in schools. If taught thoroughly, it necessitates the introduction of a good deal of elementary chemistry and physics, since the principles of vegetable physiology, which should on no account be omitted, cannot be explained without reference to the composition of air, earth, and water, the diffusion of gases, capillarity, chemical decomposition, and so on. Theoretically, no doubt, the foundation of a scientific training should be laid with mathematics, physics, and chemistry. As to the first of these there is no difficulty; but unless the two latter can be taught practically, it seems to me that the best thing is to be content with something less than the ideally perfect, and, with mathematics as the necessary introduction to abstract science, to take as our basis for the concrete study of Nature the facts and phenomena of plant-life.[1]

There is one consideration of the first importance, which every science teacher must keep in mind if he wishes his subject to have its proper value as an educational instrument, and that is the absolute necessity for demanding as much and as hard work from his pupils as the classical or the mathematical master. Unless this is done scientific subjects must always hold an inferior position, and the teaching of them can never be followed by adequate results. It behoves every one of us to remember that—

> "Von der Stirne heiss,
> Rinnen muss der Schweiss,
> Soll das Werk den Meister loben,"

and that, if we are satisfied with a minimum of work from our pupils, we must also be content with a minimum of respect for our teaching. As long as in our Matriculation and Junior Scholarship examinations a pupil can pass creditably in a scientific subject by getting up a text-book, while to obtain distinction in classics or mathematics requires prolonged and thoughtful work, so long will science-teaching in schools fail to have any real educational value.

I should like to make it perfectly clear that I am not making the slightest attempt to uphold the absurd notion that science should replace the strict study of language and literature, or of mathematics. All that I plead for is that it should be put on equal terms with them, and should no longer be handicapped by a totally inefficient method of teaching, and then condemned as wanting in the essentials of a strictly educational subject. Those who advocate a return to purely classical instruction because of the acknowledged failure of book-science are comparable to politicians who can see no remedy for the excesses of a revolution save a return to despotism. The whole case as between scientific and literary instruction is so admirably put by Mr. Matthew Arnold that I cannot resist the pleasure of quoting the passage:—"The aim and office of instruction, say many people, is to make a man a good citizen, or a good Christian, or a gentleman; or it is to enable him to do his duty in that state of life to which he is called. It is none of these, and the modern spirit more and more discovers it to be none of these. These are at best secondary and indirect aims of instruction; its primary and direct aim is to enable a man *to know himself and the world*. Such knowledge is the only sure basis for action, and this basis it is the true aim and office of instruction to supply. To know himself a man must know the capabilities and performances of the human spirit; and the value of the humanities, of *Alterthumswissenschaft*, the science of antiquity, is that it affords for this purpose an unsurpassed source of light and stimulus. . . . But it is also a vital and formative knowledge to know the world, the laws which govern Nature, and man as a part of Nature. This the realists have perceived, and the truth of this perception, too, is inexpugnable. Every man is born with aptitudes, which give him access to vital and formative knowledge by one of these roads; either by the road of studying man and his works, or by the road of studying Nature and her works. The business of instruction is to seize and develop these aptitudes." And again: "The grand thing in teaching is to have faith that some aptitudes of this kind every one has. This one's special aptitudes are for knowing men—the study of the humanities; that one's special aptitudes are for knowing the world—the study of Nature. The circle of knowledge comprehends both, and we should all have some notion, at any rate, of the whole circle of knowledge. The rejection of the humanities by the realists, the rejection of the study of Nature by the humanists, are alike ignorant."

Until within the last few years the position of science, and especially of biology, in universities and colleges, was quite as unsatisfactory as in schools. In the days when zoology was taught merely by lectures, and a man to insure success in examinations had only to "cram" his notes or a text-book and perhaps be able to tell a mammal's skull from a bird's, or a bivalve shell from a coral, it was not unnatural for the votaries of the older forms of culture to look upon "science" as a sort of academic Alsatia—a useful-enough refuge for the stupid, the lazy, and the eccentric, but something quite

[1] "Higher Schools and Universities in Germany."

[1] For this reason I cannot but regret that in the regulations for Junior Scholarships approved by the Senate at their recent meeting, biology is only counted as of equal examination value with a single branch of physics; so that while a candidate can take up physics alone of science subjects, he is obliged, if he select biology, to take in addition either chemistry or a branch of physics or mechanics.

beneath the notice of a man with a fair share of intellect and diligence.

And this opinion was quite justified by the facts. In my own University—London—until quite recently, there was no evidence of practical knowledge required in any branch of science except botany, for the degree of Bachelor of Science. A fair amount of mathematics and mathematical physics were demanded; but the chemical standard was miserably low, and the zoology, physiology, botany, and geology were such that no experienced examinee would wish for more than a month's reading for each, with perhaps an extra fortnight in the case of botany to enable him to learn enough of the art of describing plants. But now that a searching practical examination is enforced in these subjects, the degree has a real value—it is evidence that a man has done real work.

The case is very similar at Cambridge. Formerly, the Natural Science Tripos was a bye-word—a sort of back-door to a university degree. Now, thanks in great measure to Dr. Foster, the chances are that a man who takes high honours in that Tripos will be the intellectual equal of a high wrangler or of a high classic.

Considering that this regeneration of biological teaching began only about ten years ago in London and Cambridge, I think New Zealand is distinctly to be congratulated upon the fact that the first professor of biology in the Colony—my predecessor in this Chair, Captain Hutton—was also the first to inaugurate the true method of teaching that science in the Australian Colonies. It is by no means the least important debt which the Colony owes to Prof. Hutton, that he, having made his reputation as a systematic zoologist, voluntarily undertook the labour—no light one—of organising, in connection with his lectures, a class for regular practical instruction in comparative anatomy. I must confess to a slight feeling of disappointment at finding, on my arrival here, that the revolution I had expected to initiate was already well under weigh.

(To be continued.)

THE ELECTRICAL DISCHARGE, ITS FORMS AND ITS FUNCTIONS[1]

I.

If we knew as much about electricity as we know about sound or light, we should be still a long way from having learnt all that we could wish, but we should know far more than we do now.

For instance, in the matter of sound, we know, in most cases, the nature of the air disturbance to which it is due, and the mechanism whereby that disturbance is effected; and we have ascertained the magnitude and character of the aërial waves on which sound is carried. We know, in fact, what it is which is transmitted, and the velocity and direction in which that transmission takes place.

Again, in the matter of light, although we do not know the exact nature of the disturbance to which luminosity is due, nor the mechanical process by which that disturbance is effected; although we are not even certain whether the ætherial waves, to which light is attributed, have an actual existence or not, we nevertheless do know that something which is capable of being represented by wave motion is transmitted along a ray of light; its direction is a matter of simple observation, and we have determined the velocity with which it travels.

But when we come to electricity our knowledge is much more at fault. We know, it is true, how to produce electricity or electrical action, as well as how to transmit it, by means of wires, to a distance; we know also that there is a dissymmetry at the two ends or "terminals" of a battery or machine, or other source of electricity, implying a directional character either in that which is transmitted, or in the mode of its transmission. But we know neither what electricity really is, nor the process whereby it is transmitted. And although, on account of the dissymetry above mentioned, we cannot divest ourselves of the idea of direction, yet we have as yet no certain clue to the actual direction in which the transmission can be said to take place. It has, indeed, been shown, by the late Clerk Maxwell and others, that the mathematical expressions for the properties of a medium, whose vibrations are capable of representing the phenomena of light, are the same as those of a medium whose vibrations are capable of representing those of electro-magnetism; and that, on the supposition that light is an electro-magnetic phenomenon, the velocity of propagation of electro-magnetic disturbances is the same as the velocity of light. But an identity in the mode of mathematical representation does not decide anything about the physical facts in either case, nor does it even prove that the facts are the same in both cases. And lastly, even granting that there is actual motion along the wires, neither the mathematical formulæ nor the experimental facts can as yet decide whether the motion, or "current" of electricity, is to be considered as starting from one terminal and arriving at the other, or as starting from the second and arriving at the first; or, indeed, whether the motion may not be in some sense double, in both directions at once.

In this somewhat unsatisfactory state of ignorance we approach the subject of this evening's discourse. And although I cannot hope in any adequate sense to resolve these difficulties, I propose to explain what progress has been made towards a solution of them, and to indicate the direction which appears to offer the best promise of success in the prosecution of further research.

Into the various modes of producing electricity it is not my intention now to enter. I shall use them indifferently as may be most convenient, explaining only in general terms any differences which may be of consequence for understanding the various experiments shown in illustration of my argument. It will, in fact, be assumed that electricity has been produced by some known means or other, and our object will be to examine it in the course of its passage, with a view of obtaining some information as to its nature and its mode of transmission.

As a matter of fact we have here as our sources of electricity, first, a Holtz machine, or, rather, Prof. Töppler's modification of it), which produces electricity in a condition similar to that given off by the ordinary frictional machines, although it effects this by a different method; secondly, a battery, or arrangement of metallic plates and acid, wherein a flow or "current" of electricity is produced by the action of the acid upon the metal; thirdly, a dynamo-machine, such as those invented by Gramme, Siemens, Brush, or others, which produces a current similar to that from the battery, but by means of the expenditure of mechanical force in moving coils or other closed circuits of wire within the influence of an electro-magnet, or, as it is usually termed, within a magnetic field; fourthly, a magneto-machine by De Meritens, producing, on a principle similar to that involved in the dynamo-machine, a series of currents, but with permanent magnets, and in this case in alternate directions; fifthly, an instrument called an induction-coil, the object of which is to produce from currents of one character currents of another, in a way to be presently described; and, lastly, we have Leyden jars or condensers for accumulating large charges in a manner which will allow of their being discharged all at once.

Now, in the first place, suppose we make use of the battery, or of the dynamo-machine, producing a direct and practically uniform current; then, if the wires carrying the current be closed, no directly visible effect is produced. I say "directly visible" because indirectly we can prove that a wire carrying a current is in a condition different to one not carrying a current. One way in which this may be shown is the following:—If we bring an ordinary piece of copper wire into the neighbourhood of some iron filings, the filings are indifferent to its presence when it is in its natural state; but as soon as the wire is made part of a circuit through which a current is flowing, the filings are attracted by it as if by a magnet. When the circuit is broken, so that the current is interrupted, the filings drop, and the wire resumes its ordinary condition. This property of a wire carrying a current is, however, beside our present purpose, and I mention it only in order to show that the passage of an electric current is not without its effect on a closed circuit, even when no result is directly visible.

The magnetic effect which we have just seen is not, however, the only effect which a current produces in a closed circuit. If in a galvanic circuit, supposed to consist otherwise of copper wire, we interpose a piece of different metal of a kind called refractory on account of its bad conductive power, such as platinum or iron, or a sufficiently thin piece of the same wire, we shall find that when the current is passing, the interposed wire becomes hot; and if we increase the strength of the current, or reduce the thickness of the wire—in other words, if we increase the quantity of electricity flowing through the platinum, or diminish the size of the platinum conductor which has to carry it—we shall find that the temperature is proportionally increased. A similar increased temperature will be produced by

[1] A Lecture delivered before the British Association at York on September 5, 1881, by William Spottiswoode, D.C.L., LL.D., President of the Royal Society.

shortening the wire, although the explanation of the phenomenon is not quite so simple. If the same process be carried further, the platinum will become white-hot, and if it be carried still further, the platinum will be fused. The Swan, the Maxim, the Lane-Fox, and the Edison lamps, in which the light is due to the incandescence of a fine thread of carbon, are beautiful instances of the application of this principle.

The platinum, which does not allow the electricity to pass along it with the same facility as the copper, is said to offer "greater resistance" than the copper of the same thickness to the passage of the current; and if we were to measure by a suitable instrument the quantity of electricity which passed through the circuit when the platinum was interposed, and were to compare it with that which passed without the platinum, we should find that the quantity was diminished by the interposition of the platinum. The energy which, as electricity, disappears in its passage through the platinum is, however, not really lost, but reappears in the form of heat.

Instead, however, of interposing in the circuit a length of resisting metal, we may break the circuit altogether, or (to express the same thing in different words) we may interpose an interval of air. In such a case the electricity will no longer flow freely as it does through copper, or even push its way as it does through platinum, but it will traverse the interval only in a disruptive manner in the form of a flash or spark; and it is to be noted that the interval over which the passage can be made to pass, or length of spark, does not depend, at least in a direct manner, on the quantity of electricity employed or "strength of current," but rather upon the quality of it. This quality is called "tension," and it is measured by the strength of current which it can maintain, or cause to flow, through a given resistance. The force called into play in the process is called "electro-motive force." Without attempting to go fully into the subject, we may illustrate the relation of quantity or strength of current to tension or electro motive force in general terms by reference to the instrumental means requisite for their production. Thus it is usually stated that in a battery the quantity depends upon the size of the plates employed, and the tension upon the number of cells; and similarly, that in a magneto- or a dynamo-machine, the quantity depends mainly on the thickness of the wire used in its construction, and the tension upon the number of convolutions or length of the wire in the coils for a given speed of working, or for a given number of convolutions, upon the speed at which the machine is driven.

In further explanation of this, however, it should be pointed out that the current generated has, independently of the external circuit, to pass through the cells of the battery, or through the wires of the machine, both of which offer resistance. When a strong current is required, this resistance may be diminished by increasing the size of the plates in the case of the battery, or by increasing the diameter of the wires in that of the machine. In the latter case it must be borne in mind this increase in diameter usually involves a diminution of length on account of the necessary limitations in the dimensions of the machine, and consequently also of electro-motive force. This must be compensated either by increasing the speed of the machine or by augmenting the strength of the field magnets.

With the Holtz machine the matter is a little different. The quantity of electricity produced depends on the amount of surface of the revolving plates passing in front of the collectors in a given time, and consequently for a given machine upon the speed at which it is driven. Thus there is nothing either in the construction of the machine nor in its internal working which can alter anything except the quantity of electricity produced, and we must therefore look to the circumstances and mode of discharge for a determination of the tension of the electricity evolved.

The induction-coil is an instrument for producing from currents of large quantity and low tension others of high tension, but of small quantity. It consists mainly of two parts, viz. a primary coil of thick wire and few convolutions, through which intermittent currents are sent from a battery or machine; and a secondary coil outside, but not connected with the former, of fine wire and many convolutions, through which by a kind of sympathetic or "inductive" action temporary currents are set up every time a current begins or ceases in the primary. The tension of the induced currents depends fundamentally upon the length of wire or number of the convolutions in the secondary coil. There are several other parts of the instrument which are important for its working, which, however, it is not necessary for our present purpose to particularise.

From this digression we may now return to our main subject; and taking it up again at the point where we left it, viz. the heating of resisting metals, we may vary the experiment by taking a piece of iron wire, and bringing to bear upon it some of the induced high tension currents from the induction-coil. It will now be found that if the sparks follow one another with sufficient rapidity, the wire will not have time to cool during the interval between two successive sparks, and that it will burn like a match or other combustible substance.

If, however, we use, instead of iron, some metal very difficult of fusion, or "refractory," as it is called, such as iridium, the consumption of material will be extremely small; and in the incandescent terminals we shall have a source of light of considerable power. And further, if the terminals be inclosed in an envelope impervious to air, and either well exhausted or partially filled with suitable gas other than oxygen, nitrogen for example; then the loss by oxidation will be reduced to an insignificant amount. On this principle Mr. Gordon has constructed a lamp, which consequently has, at all events, the scientific interest of occupying a position intermediate between the incandescent and the arc lamps.

Lastly, if we accumulate a large quantity of electricity in a Leyden jar, and discharge it all at once through a thin wire or film of badly conducting metal, we shall cause the metal (in this case a strip of gold leaf) to be not only fused but to be shattered or deflagrated, in the manner which you will immediately see. The image of the gold leaf is now thrown on the screen, the jar is charged by currents from the induction coil, and is discharged through the metal. The gold leaf is now shattered by the passage of a high tension charge, the quantity of which is greater than it can carry; and in the image of its remains we may trace indications of the forces which have been at work in the process of destruction. Observe, in particular, how the particles have been thrown laterally outwards, as if by an explosion from inside the gold leaf. In the alternations of range of the laterally scattered particles Mr. De La Rue traces an analogy to the phenomena of striation described below. And if these alternations are not due to diversities in the conducting power of the wire at various points, but to resistances set up periodically by the discharge itself in its passage, the two phenomena must certainly have something in common.

I do not, however, propose to pursue these forms into greater detail, because the subject to which I wish more particularly to draw your attention, as the most fruitful both in results actually obtained and in promise for the future, is the passage of the discharge through air and other gases. And I have adduced these experiments with metallic substances in order to show that the discharge through them is capable of various modifications, analogous to those which we shall presently see in gaseous media.

Turning then our attention to gases, it will be convenient, for instrumental and other reasons, to invert the order of experiments, so as to begin with the form of discharge which corresponds to the deflagration experiment, and to proceed thence to less violent forms.

We will now make use of the Holtz machine. If, while the instrument is in action, we separate the terminals to any moderate distance, the discharge will take the form of a bright spark extending usually in an irregular line from one terminal to the other. If, instead of discharging the machine or coil in this manner, we charge a Leyden jar, and then discharge it; or if, what is substantially the same thing, we insert a Leyden jar in the circuit, allowing it to become charged and to discharge itself, then the discharge is of a character similar to that above described, except that it is shorter in span, and at the same time more brilliant in illumination. This is due to the greater quantity of electricity discharged at once. It is moreover to be observed that, however great the quantity of electricity passing in this manner, the discharge appears to be absolutely instantaneous. It is moreover a curious circumstance, attested by many experiments, that the form of discharge in which a Leyden jar is used appears to be incompetent of itself to communicate heat to even inflammable bodies. Thus, such a discharge will pierce a card without leaving any signs of charring behind; and it will disperse a heap of gunpowder, through which it passes, like a heap of sand, without exploding it. It may be added that gun-cotton itself, even in a state favourable to explosion, when exposed to a discharge of this kind, is not only not ignited, but

merely shows signs of perforation like the card, without any blackening or indication of combustion. Whether these facts point merely to shortness of duration in the discharge such as to preclude the communication of heat-vibrations to the bodies traversed, or whether they imply some mode of motion with which heat has nothing to do, are questions which have been thrown out by those who have studied the subject.

In favour of the former view it should be stated that the spectrum of the spark proper, whether with or without the jar, shows bright lines, indicating the presence of metallic vapours. These of course imply a high temperature, although not necessarily any great quantity of heat. And if the duration of the spark itself be extremely small compared with that of the interval between two successive sparks, the period of cooling will be extremely long compared with that of heating, and the observed result is exactly what we might expect.

There is, however, one feature of the spark discharge proper which is perhaps especially deserving of remark, namely, the similarity, in appearance at least, of its passage through air with that of a spark through glass or other solid and non-conducting substances. In the latter case we are familiar with the manner in which it rends its way by a shattering and dislocation of the substance in its immediate path, while it leaves the other parts of the substance untouched, very much as does a bullet when shot through a pane of glass. The path, however, if of any considerable length, is never quite straight, and it sometimes divides itself into two branches. The analogy above suggested will be complete, and the phenomenon will be brought into harmony with other known facts, if only we regard the spark as being so rapid, so instantaneous, in its passage that the particles of air have not time to exercise their mobility during the period occupied by the spark in its passage through them. In this view, air itself in the presence of the electric spark is to be regarded as exhibiting a rigidity and brittleness comparable with that of glass itself.

If, the Leyden jar having been removed, the terminals of the

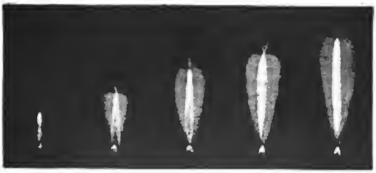


FIG. 1.

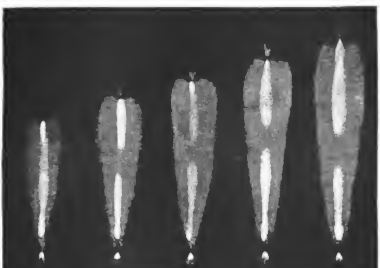


FIG. 2.

FIG. 3.

Holtz machine be separated to a distance greater than that over which the spark will leap, a hissing or crackling noise is heard, indicating a rapid intermittence in the discharge, and a delivery, so to speak, of small quantities of electricity at a time. A minute examination of the phenomena occurring with terminals of different forms, and at different distances, has led to a classification of types of discharge under four main heads :—

1. The glow discharge: presenting a glow on the positive terminal, and a pencil of light issuing from the negative, and consisting of two portions with a dark space between them.

2. The brush discharge: consisting of a brush, viz., a stem and branches at the positive terminal, a pencil of light at the negative, and a dark space as before.

3. The band discharge: consisting of a band of light proceeding from the positive terminal, sometimes stratified, and separated from the negative glow by a dark space.

4. The spark discharge: showing in the spectroscope bright lines at both terminals. Two brushes of metallic vapour, that at the positive terminal being the longer, that at the negative the shorter and thicker. Two dark spaces are to be noticed in this form of discharge.

On the other hand, if the terminals be brought nearer together than they were at first, nearer, that is to say, than is suitable for the production of the spark proper, it will be noticed that the sharp cracking noise is replaced by a sound similar to that heard when they were beyond striking distance. The intermittence of the discharge becomes very rapid, and its colour assumes a reddish hue.

A full explanation of this almost abrupt change in the character of the discharge would probably involve a more profound acquaintance with the nature of electricity than we at present possess. But there is reason to think that something like the following takes place :—The path between the terminals once opened offers for a very short time considerable facility for the

electricity to traverse it again. But the distance between the terminals being very small, the electricity coming from the machine soon attains the requisite quantity and tension, and the discharge is repeated before the facilities due to the preceding discharge are lost. The shortness of the interval between the terminals consequently acts in a double manner to facilitate the discharge, and thus renders the transition from one form to the other more rapid than it would otherwise have been.

Fig. 4.

Fig. 5.

Observations with the spectroscope indicate that heating here takes place, and the revolving mirror shows that either in the discharge itself, or in the heating effects due to it, there is a manifest duration, all of which tend in the direction of the explanation suggested above.

The character of the discharge from the induction-coil both when the terminals are widely separated and when they are near together, is generally similar to that from the machine; but the durational character of the former is very much more marked than that of the latter; so much so, in fact, that with large coils the duration extends over a fraction of a second, perfectly appreciable by the eye without any auxiliary apparatus. This is due to the nature of the instrument, and is dependent both upon the time occupied by the core in losing its magnetism, and also upon the mutual induction of the convolutions of the secondary coil. The flame which accompanies the spark proper is the part of the discharge which persists; and it will have been noticed particularly when the coil is excited by the De Meritens' machine. The discharge produced from the secondary through the instrumentality of this machine is so remarkable, that it has been considered worth a special study. It has also been of great assistance in the examination of the action of a magnet upon a discharge; but the results of the latter experiments have not yet been published.

The form of discharge which we have now reached is substantially that which is known as the "arc," or comparatively quiet and continuous discharge between two terminals near to one another.

Turning to the arc, let us take the form most familiar to our minds, viz. that used in electric lighting. I now project on the screen an image of the arc as used in what are called "arc lamps." The whole consists essentially of two rods of carbon placed end to end, with a short interval between them. The interval is of a length capable of being traversed by the current, at all events after the discharge has been once established. By the passage of the current, which, in fact, constitutes the arc, the carbon becomes heated to a high degree. And it is important to understand that the main source of the light is to be found, not in the arc proper, but in the heated carbons. It will be noticed that, when a machine giving direct currents is used, the two carbons are not equally heated, and that during the combustion they acquire dissimilar configurations. This dissymmetry at the terminals is found to obtain in almost every species of electrical discharge.

With the construction and outcome of the various machines employed for producing the current, and with the mechanical contrivances used for maintaining the arc at its proper length and in its proper position, we are not here concerned. All that need be here mentioned is that the carbon which would be connected with the copper element of a Grove battery, if such were used, and which is called the positive, is the one more rapidly consumed. It becomes hollowed out, and incandescent particles may be seen occasionally traversing the arc, and landing upon the second or negative carbon. In the meantime the arc proper flows steadily between the carbons, the colour being determined by the nature of the terminals, or by that of any substance placed on their ends; and partly also by the nature of the gas in which the discharge takes place.

Let us now regard the terminals merely as parts of our apparatus, subsidiary to the main purpose, and fix our attention almost exclusively on the arc itself. If we had been working in

Fig. 6.

the laboratory, I should have asked you to examine, with the aid of a microscope, the minute structure or anatomy of the arc. As it is, I must beg you to accept as a substitute for the phenomenon itself the following series of photographs, for which we are indebted to the skill and kindness of Mr. De La Rue, who has done so much with his unrivalled battery in this field of research.

Figs. 1 to 5 are, in fact, magnified representations of the discharge through air at different pressures, beginning with that of the atmosphere, and extending in a series of decreasing pressure to about one 30th part of it. In Fig. 6 the pressure has been reduced to about one 2000th part of an atmosphere. In all these instances it will be noticed that there is a tendency on the part of the luminosity to break up into disconnected blocks, and that at an early stage it begins to separate from the negative, and to cling to the positive terminal. Also, that when the pressure is considerably reduced, these blocks are replaced by the beautiful system of flakes or "striæ" delineated in the last figure of the series. At this stage the dyssymmetry on which I have already insisted is complete.

The actual length of the discharges of which you have just seen the representations, varies in a tolerably regular manner with the pressure, from half an inch to ten inches or more. From this we may gather the important fact that in the discharge through gases at low pressures we have a magnified image of the discharge at higher pressures. By this statement it is not of

course intended that every detail that is observable in the former cases can be distinguished in the latter; for the very nature of the gas, its viscosity or other properties, may prevent this. But all the characteristic features which prevail at high pressures are found also at low pressures, on a larger scale and in more marked delineation. From this consideration, as well as from others to be noticed below, we are led to the conclusion that rarefied gases form a promising field for future research into the nature of the electrical discharge.

Proceeding on this basis, I now desire to present to you the actual discharge in two or three tubes from which the air has

FIG. 7.

been exhausted in various degrees. In the first, where the pressure is that of about 3 or 4 mm. of mercury, or ·004, say one twenty-fifth part, of that of the atmosphere, the discharge takes the form of a column of light, slender in breadth and flexible in shape, extending throughout the entire length of the tube. The colour of the discharge depends on the nature of the residual gas. In the present case that gas is air; and the reddish hue is due to its constituent, nitrogen.

In the next tube the exhaustion has been carried further, viz., to a pressure of about 2 mm., or ·0026, say one-fortieth part, of an atmosphere. In this case the luminous column has become thicker; there are traces of a dark interruption towards the negative end; while beyond this break, and about the negative terminal, the light is no longer red, but of a deep blue colour. This strong contrast of colour at first sight appears inexplicable, and as a matter of fact the difficulties of explanation have not yet been altogether surmounted. But these difficulties are much diminished by a spectroscopic examination of the phenomena, from which it appears that, notwithstanding the contrast between the light near the negative terminal and that in other parts of the tube, the spectrum of the former differs from that of the latter, generally, not in its fundamental character, but mainly in the addition of certain strong lines in the blue and violet. Besides this, there is occasionally a weakening of the lines in the less refrangible part of the spectrum seen in the light of the positive column. The extension of a spectrum in the direction of the more refrangible end is known generally to depend upon an increase of temperature; and as there are other grounds for attributing a higher temperature to the region near the negative terminal than to the other parts of the tube, it would seem that we must look to thermal conditions for an explanation of the contrast in question.

But, besides the contrast just described, some tubes show a diversity of colour in the same striated column. Or perhaps, more strictly speaking, there coexist two, or even three, columns (usually pink and blue, with an occasional intervening green) blended together near the positive, but more separated towards the negative end. At the negative end they are in some cases completely separated; in others they are united so as to give the appearance of parti-coloured striæ. In every case, however, the blue striæ are found nearer to the negative end than the green or pink.

In the next tube the pressure is about half a mm., or ·00065, say one 160th part of an atmosphere; and here we find the dark space near the negative terminal, observable in the previous case, greatly increased. But besides this, the whole column is no longer continuous, but is broken up into striæ with dark intervening spaces.

As the exhaustion proceeds the striæ become more and more separated, as well as individually thicker. At first mere flakes of light, they gradually increase in thickness, until they assume the proportions of blocks of light sometimes of larger dimensions in the direction of the axis of the tube than in that of the diameter. At the same time the main dark space between the head of the column and the solitary luminosity about the negative terminal, as well as the dimensions of that luminosity itself, increase in length. A dark space immediately surrounding the negative terminal, and limited by the solitary stria, also begins to show itself, and to increase with the exhaustion. This space has been named after Mr. Crookes, who first made a study of it. As we proceed yet further, the column retreats towards the positive terminal; and at the last stage the solitary luminosity shares the same fate. The Crookes' space occupies the whole tube, and no gaseous illumination whatever remains. To the phenomena which arise in this condition of things I will make allusion at a later stage.

This dependence of the distance between the striæ upon the pressure of the gas may be well illustrated by using a tube fitted at one end with a chamber containing potash. The potash has the property of absorbing gases of almost every kind, of giving them out when it is heated, and of re-absorbing them when it is allowed to cool again. This process may now be seen in actual operation.

The number and disposition of the striæ will naturally depend also on the length of the tube. The effective length may be altered without altering any other conditions of the experiment, by having one terminal attached to the wire leading into the tube by a flexible spiral wire; so that the terminal itself may be shifted. At first sight it might have been supposed that any change due to an alteration in the length would have depended very much upon whether the shifting terminal is the positive or the negative. But whichever be the case, the striæ are seen to drop one after another into the positive terminal; the solitary stria and the adjacent dark space remain unaltered, and no change is apparent beyond a reduction in the number of the striæ.

This is, however, not the only way in which the disposition of the striæ may be made to vary. In some gases at suitable pressures an increase in the strength of the current used, or in the quantity of electricity discharged through the tube, reduces the number of the striæ, and to some extent shortens the column by drawing or driving the striæ one by one into the positive terminal. In such a case it also increases their mutual distances in the same manner as if they were threaded on an elastic string. In other gases the reverse is the case. Thus, in this sulphide of hydrogen tube, the striæ in the column are numerous and crowded while the machine is in rapid motion. As the speed is diminished the striæ recede from one another, until only a few lingering specimens are left, separated by the broad dark and mysterious spaces which you now see.

The long continuance of the discharges from the induction coil afford an opportunity of examining the various phases of striæ during their existence. The details of the instrumental arrangements, as well as other particulars of the observations, have been elsewhere described; but the main features observed may be apprehended by the illustrations subjoined.

Fig. 7 represents the appearance of (in the mirror) a carbonic-acid tube with the slit attached. This tube, viewed by the eye, shows flake-like fluttering striæ, with a slight tendency to flocculency near the head of the column. The commencement of the discharge is at the right hand, and the negative terminal at the top. The drawing fairly represents the appearance of the upper part or head of the column of striæ during one complete coil-discharge. When the battery-surface exposed is small, the whole consists of, first, three or four columns of striæ of decreasing length, and afterwards of an almost unbroken field of striæ. Each of the initial columns is perfectly stratified; and the same disposition of striæ prevails throughout the entire discharge. The striæ which fill the main part of the field present a proper motion, that is a motion along the tube during their period of existence, usually steady and towards the positive. In this case it is nearly uniform, but slightly diminishing towards

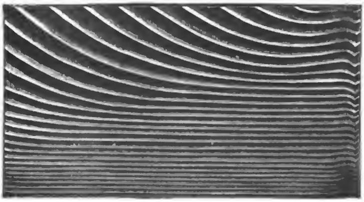


FIG. 8.

the end. These striæ are for the most part unbroken, but are occasionally interrupted at apparently irregular intervals. When the battery-surface is increased, the elementary striæ are more broken, and near the head of the column the interruptions occur as in the figure.

Fig. 8 represents the discharge in a hydrogen-tube of conical form, the diameter of which varied from capillary size to half an inch, the capillary end being at the bottom. The positive terminal is at the top. The principal interest of this tube consists in showing the influence of diameter upon the velocity of proper motion. The wider the tube the freer, it seems, the striæ are to move.

The same fact may be observed by comparing tubes differing in diameter, but in other respects the same; but the conical tube brings out the fact in the most striking manner.

Fig. 9 represents a chloroform-tube, in which a piece of

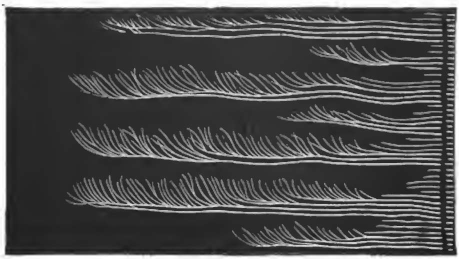


FIG. 9.

cotton-wool had been inserted with a view of ascertaining whether any motion would be communicated to it by the current. This proved to be the case; but I do not attempt here to describe the phenomenon. To the unassisted eye the discharge was extremely brilliant; it passed in a column not quite straight, but in a writhing, snake-like curve, with flaky striæ at intervals through its length. When viewed in the mirror the striæ were seen to spread themselves out with slight, but irregular, proper motion. With an increased battery-surface, or with a greater number of cells, but more notably with the latter, not only were the striæ lengthened, but from several of the long elementary striæ shorter ones were thrown out nearly at right angles to the former. These were of short duration, and had proper motion. The general appearance of these compound striæ was that of branches of fir trees, the twigs of which represented the permanent striæ, and the leaves the secondary.

(To be continued.)

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 19.—M. Wurtz in the chair.—The president gave a welcome to the foreign members of the International Congress of Electricity who were present, including Clausius, Clifton, Du Bois Reymond, Everett, Förster, Helmholtz, Kirchhoff, Melsens, Spottiswoode, Siemens (William and Werner), Smith, Stas, Thomson, Warren De La Rue, and Wartmann.—The following papers were read:—On the relative resistances that should be given, in dynamo-electric machines, to the active bobbins, the inductor electro-magnets, and the interior circuit, by Sir William Thomson.—On experiments made in 1826 on electric currents by lightning far from the place of observation,

and on recent studies of M. René Thury on sounds of telephones during thunderstorms, by M. Colladon. M. Thury stretched a copper wire horizontally between two houses at the height of the roofs, and connected it with the water pipes, and with two telephones. The telephones gave a characteristic sound each time and at the same instant as a flash of lightning was seen, near or far (and even when no thunder was heard). It was like the sound of a Swedish match rubbed on the box. M. Colladon, in 1826, observed deflections in a galvanometer in Paris during a thunderstorm at a distance, while there was no cloud within 30° of the zenith, and M. Peclet describes like inductive effects in his "Traité de Physique" (1832). M. Colladon thinks the sounds will be best heard in the telephone when the air is surcharged with humidity. The telephone affords an easy method of measuring the velocity of transmission of those influences.—Measurement of rotation of the plane of polarisation of light under the magnetic influence of the earth, by M. H. Becquerel. Repeating his experiments under more favourable conditions, he finds that the yellow rays D, traversing horizontally a column of 1 metre of sulphide of carbon at 0°, under the influence of terrestrial magnetism at Paris, and in a direction parallel to the declination needle, undergoes a simple magnetic rotation of 0'·8697 from right to left for an observer supposed to lie with his head towards the magnetic north. In the C.G.S. system of units this leads to the number $1·31 \times 10^{-5}$ as expressing the magnetic rotation of yellow rays through sulphide of carbon between two points of unit distance in a magnetic field equal to unity. (Mr. Gordon's figures, got by different methods, give $1·24 \times 10^{-5}$ for sodium light.)—On the passage of projectiles through resistant media, on the flow of solids and the resistance of air to the motion of projectiles, by M. Melsens. He arranged experiments with a view to catching the air carried in front of a projectile. Lead balls (about 0·017m. in diameter) were shot into a hollow cone in a block of iron, the apex being of steel, and having an opening, smaller than the ball, into a gun-barrel communicating with a bell-jar in a reservoir. The gun, the reservoir, and the bell-jar were filled with water, which was prevented escaping through the cone by a light obstacle of paper or thin brass. Detached fragments of the lead entered the gun-barrel, the bulk of the ball stopping the hole of the cone, and appearing pointed, or with an oblong drop. The effects of the penetrating air are indicated in the cracks and rupture of the gun-barrel, the bell-jar, and the bent tube between them. M. Melsens considers the resistance of the air implies factors of which artillery has not taken sufficient account. This resistance is variable throughout the trajectory, in virtue of the mass of the projectile, the form of the mass of adherent air, the velocity, the thrust of the powder-gases, up to a certain distance from the gun, and, lastly, from the very brief moment when the projectile is equally pressed in all directions by air.—On new sulphurised salts produced with sesquisulphide of phosphorus, by M. Lemoine.—On tungstoboric acid and its salts, by M. Klein.—Determination of phosphoric acid by titrated liquors, by M. Perrot.—On some of the scientific researches contained in the manuscripts of Leonardo da Vinci, by M. Ravaisson. He calls attention to a passage recommending, as a method of hearing distant sounds at sea or on land, inserting one end of a tube in the water or in the earth, and putting one's ear to the other. M. Ravaisson is preparing the manuscript B, one of twelve in the Bibliothèque de l'Institut, for publication (to follow MS. A, published in December last).

September 26.—M. Wurtz in the chair.—The following papers were read:—Researches on the gymnotus in Venezuela, by the late Dr. Sachs, by M. du Bois Reymond. At the instance of Prof. du Bois Reymond, five years ago, Dr. Sachs went out with modern electrophysiological apparatus, to study the gymnotus in the marshy waters of the Llanos of Calabozo. Returning to Berlin in 1877, he set himself to composing a work on his observations in general, and was about to write specially on the gymnotus, when he lost his life by falling down a crevasse in the Alps of the Tyrol. The monograph now presented gives the results of his studies of gymnotus, with further valuable observations by Prof. Fritsch, who has worked out the anatomy of the animal, numerous specimens of which Dr. Sachs had brought home. M. Fritsch has been able to demonstrate in an almost certain manner the development of the electric organs through metamorphosis of striated muscles.—Results obtained in treatment of phylloxerised vines by the use of sulphide of carbon and sulpho-carbonate of potassium, by M. Henneguy. The vines treated with sulphide of carbon retain their greenness longer than those treated with sulpho-carbonate of potassium, but their branches are shorter and bear fewer grapes.—Observations relative to accidents to vines treated in 1881 with sulphide of carbon, by M. Pastre. These accidents have been mostly due to excess of humidity in a compact clayey soil. The sulphide either remains liquid, or evaporates in too little space; and in both cases (the former especially) it destroys the roots. A less frequent cause is too low temperature Among rules M. Pastre lays down are these: To treat only well-dried ground, and vines not too much affected; to multiply the holes and diminish the doses; to manure well; to leave off treatment when the temperature is too low.—On trilinear forms, by M. le Paige.—Photometric comparison of luminous sources of different colours, by M. Crova. He uses a spectrophotometer. With two sources (say an electric light and a standard Carcel lamp), so placed that the mean luminosity of the two contiguous spectra is the same, the ratio the intensities of simple radiations of one light to those of the other (corresponding) is represented by a fraction greater than unity in violet, and less in red, and there is one simple radiation for which the ratio is equal to unity. If this radiation be exactly known, the measure of the ratio of its intensities in the two spectra will give *exactly* the ratio of the total intensities. M. Crova realises this with the aid of two Nicol prisms having a quartz plate between them. The apparatus gives very exact results.—Studies on the chemical action of light, by M. Lemoine. He has compared experimentally, from various points of view, the influence of light with that of heat in chemical reactions; considering, more especially, isomeric transformations, and the influence of dissolution, temperature, organic matters, and colour. *Inter alia*, chloride of silver, so sensitive to light, is unaltered by it when dissolved in ammonia. The rate of chemical transformations often varies extremely with the temperature, for light as well as for heat. Presence of organic matters often accelerates a reaction in light and allows of its commencing at a lower temperature. For various substances which heat alone would decompose at low temperatures, the red end of the spectrum seems much less efficacious than the violet end; but in time both lights seem to produce the same effect.—Researches on tropine, by M. Ladenburg.—On a ureometer, by M. de Thierry. This apparatus, for determining the urea in urine of men and animals, is based on the process of decomposition of urine by hypobromite of soda. It is in two parts, one comprising a tube, with ampulla and stop cock, adapted to a reservoir which communicates through a lateral tube of caoutchouc with the second part. This includes a test-tube, a graduated bell-jar, and a thermometer.—M. Larroque described an instrument for observation of meteors; it is a mirror having the form of a double pyramid.

CONTENTS

THURSDAY, OCTOBER 13, 1881

MR. DARWIN ON THE WORK OF WORMS

The Formation of Vegetable Mould through the Action of Worms, with Observations on their Habits. By Charles Darwin, LL.D., F.R.S. (London: John Murray, 1881.)

IF the world were not already accustomed to the unprecedented fertility of Mr. Darwin's genius, it might well be disposed to marvel at the appearance of yet another work, now added to the magnificent array of those which bear his name. But feelings of wonder at Mr. Darwin's activity have long ago been sated, and most of us have grown to regard his powers of research as belonging to a class *sui generis*, to which the ordinary measures of working capacity do not apply. Be our feelings of wonder, however, what they may, it is most gratifying to find that this latest work from the hand of our illustrious countryman is in every way worthy of its predecessors. Everywhere throughout the book we meet with the distinctive attributes of Mr. Darwin's mind. Beginning with matters of the most common knowledge, which at first sight appear to furnish the most unpromising material, he proceeds by close observation of details and sagacious manipulation of facts to establish general truths of the most far-reaching importance in directions where we should least have expected any such truths to lie.

But to avoid the presumption of seeming to commend the work of so great a master, we shall proceed at once to render an epitome of the work itself. This, as its title is sufficient to denote, is an extension of the celebrated paper "On the Formation of Mould," read before the Geological Society in 1837 (See *Trans.* Geol. Soc. vol. v. p. 505); but the extension is so considerable that the present volume is really a new work. The subject, of course, is the same; but the later observations, while tending to confirm, and in fact to demonstrate, the conclusions based upon the former, have served to swell a short paper into a book of over 300 pages. Alluding to this paper, Mr. Darwin writes:

"It was there shown that small fragments of burnt marl, cinders, &c., which had been thickly strewed over the surface of several meadows, were found after a few years lying at a depth of some inches beneath the turf, but still forming a layer. This apparent sinking of superficial bodies is due, as was first suggested to me by Mr. Wedgwood, of Maer Hall, in Staffordshire, to the large quantity of fine earth continually brought up to the surface by worms in the form of castings. These castings are sooner or later spread out, and cover up any object left on the surface. I was thus led to conclude that all the vegetable mould over the whole country has passed many times through the intestinal canal of worms Hence the term 'animal mould' would be more appropriate than that commonly used of 'vegetable mould.'"

Dealing next with criticisms which from time to time have been made upon his original paper, Mr. Darwin quotes one from Mr. Fish, which we may here re-quote on account of its instructive character. "Considering their weakness and their size, the work they are represented to have accomplished is stupendous." On which Mr. Darwin observes:—"Here we have an instance of that inability to sum up the effects of a continually recurring cause, which has often retarded the progress of science, as formerly in the case of geology, and more recently in that of the principle of evolution." He then adds:—

"Although these several objections seemed to me to have no weight, yet I resolved to make more observations of the same kind as those published, and to attack the problem on another side; namely, to weigh all the castings thrown up within a given time in a measured space, instead of ascertaining the rate at which objects left on the surface were buried by worms. But some of my observations have been rendered almost superfluous by an admirable paper by Von Hensen, already alluded to, which appeared in 1877. Before entering on details with respect to the castings, it will be advisable to give some account of the habits of worms from my own observations and from those of other naturalists."

Of these habits the most interesting are as follows:—

Although earth-worms are properly speaking terrestrial animals, they are still "like the other members of the great class of annelids to which they belong," semi-aquatic. For while dry air is quickly fatal to them, they may live when completely submerged in water for nearly four months. Normally they live in burrows, and generally lie motionless just at the mouth of the latter, so that by looking down into the burrows the heads of the worms can be seen. This habit of lying near the surface leads to their destruction in enormous numbers by birds. For,

"Every morning during certain seasons of the year, the thrushes and blackbirds on all the lawns throughout the country draw out of their holes an astonishing number of worms; and this they could not do unless they lay close to the surface. It is not probable that worms behave in this manner for the sake of breathing fresh air, for we have seen that they can live for a long time under water. I believe that they lie near the surface for the sake of warmth, especially in the morning; and we shall hereafter find that they often coat the mouths of their burrows with leaves, apparently to prevent their bodies from coming into close contact with the cold damp earth. It is said that they completely close their burrows during the winter."

As regards powers of special sense, it has been observed by Hoffmeister that, although destitute of eyes, earthwarms are sensitive to light, time however being required for the summation of the stimulus before it is responded to. It is only the anterior extremity of the body, where the cerebral ganglia are situated, that is thus sensitive to light. These observations have been confirmed by Mr. Darwin. He further found that the colour of the light apparently made no difference in the result, nor did partly filtering out the heat-rays by means of a sheet of glass; while a dull-red heated poker, held at such a distance from the worms as would cause a sensible degree of warmth to the hand, did not disturb them nearly so much as the light from a candle concentrated by a lens. The sensitiveness to light is less when a worm is engaged in eating or in dragging leaves into its burrow—a fact which Mr. Darwin is disposed to consider analogous to what in higher animals we know as the distracting influence of attention. When not engaged in any active operation, the sensitiveness of worms to light is so considerable that "when a worm is suddenly illuminated it dashes like a rabbit into its burrow."

With respect to hearing, all the experiments went to show that worms are totally deaf to all kinds of aërial vibration, although extremely sensitive to the vibration of

any solid object with which they may be in contact, as was shown, among other ways, by placing flower-pots containing worms in their burrows upon a piano; on striking single notes, whether high or low, the worms instantly retreated. In this connection, also, the following may be quoted:—

"It has often been said that if the ground is beaten or otherwise made to tremble, worms believe that they are pursued by a mole, and leave their burrows. I beat the ground in many places where worms abounded, but not one emerged. When, however, the ground is dug with a fork and is violently disturbed beneath a worm, it will often crawl quickly out of its burrow."

Regarding smell, the interesting result was obtained, that the sense is "confined to the perception of certain odours"—namely, those emitted by natural food. For while the animals showed themselves indifferent to tobacco juice, paraffin, &c., held near them, pieces of cabbage-leaf, onions, &c., when buried near an earthworm, were always discovered by the animal.

The presence of taste was proved by the fact that the worm showed a preference for some varieties of cabbage over others; but "of all their senses, that of touch, including in the term the perception of vibration, seems much the most highly developed."

Worms are omnivorous, dragging pieces of meat as well as leaves into their burrows for the purpose of eating them. They smear the leaves so drawn in with a secreted fluid. This fluid is alkaline, and acts both on the starch granules and on the protoplasmic contents of the cells; it thus resembles in nature the pancreatic secretion, and serves partly to digest the leaves before they are taken into the alimentary canal—so constituting the only case of extra-stomachal digestion hitherto recorded in an animal—its nearest analogy being perhaps that of the digestive fluid of Drosera or Dionœa, "for here animal matter is digested and converted into peptone not within a stomach, but on the surface of the leaves."

We now come to one of the most interesting chapters, which deals with the habit of dragging down leaves, &c., into the burrows; for here the experiments elicited some very remarkable evidence of action which is apparently intelligent. These experiments are thus led up to.

"Worms seize leaves and other objects, not only to serve as food, but for plugging up the mouths of their burrows; and this is one of their strongest instincts. Leaves and petioles of many kinds, some flower-peduncles, often decayed twigs of trees, bits of paper, feathers, tufts of wool and horse-hairs are dragged into their burrows for this purpose. . . . When worms cannot obtain leaves, petioles, sticks, &c., with which to plug up the mouths of their burrows, they often protect them by little heaps of stones; and such heaps of smooth rounded pebbles may frequently be seen on gravel-walks. Here there can be no question about food. A lady, who was interested in the habits of worms, removed the little heaps of stones from the mouths of several burrows and cleared the surface of the ground for some inches all round. She went out on the following night with a lantern, and saw the worms with their tails fixed in their burrows, dragging the stones inwards by the aid of their mouths, no doubt by suction. 'After two nights some of the holes had eight or nine small stones over them; after four nights one had about thirty, and another thirty-four stones.' One stone which had been dragged over the gravel-walk to the mouth of a burrow weighed two ounces; and this proves how strong worms are."

The object of this plugging Mr. Darwin surmises to be that of "checking the free ingress of the lowest stratum of air when chilled by radiation at night."

Now, concerning the apparent intelligence displayed in these plugging operations, Mr. Darwin "observed carefully how worms dragged leaves into their burrows; whether by their tips or bases or middle parts. It seemed more especially desirable to do this in the ease of plants not natives to our country; for although the habit of dragging leaves into their burrows is undoubtedly instinctive with worms, yet instinct could not tell them how to act in the case of leaves about which their progenitors knew nothing. If, moreover, worms acted solely through instinct or an unvarying inherited impulse, they would draw all kinds of leaves into their burrows in the same manner. If they have no such definite instinct, we might expect that chance would determine whether the tip, base, or middle was seized. If both these alternatives are excluded, intelligence alone is left; unless the worm in each case first tries many different methods, and follows that alone which proves possible or the most easy; but to act in this manner and to try different methods makes a near approach to intelligence."

A large number of experiments were therefore tried with leaves of various shapes, and both of endemic and exotic species. The results showed unequivocally that the part of the leaf which the worm seized for the purpose of dragging the whole into the burrow was not a matter of chance, but in an overwhelming majority of cases that part of a leaf was seized by the dragging of which the leaf would offer least resistance to being drawn into the burrow. Thus, for instance, "the basal margin of the blade in many kinds of leaves forms a large angle with the foot-stalk; and if such a leaf were drawn in by the foot-stalk, the basal margin would come abruptly into contact with the ground on each side of the burrow, and would render the drawing in of the leaf very difficult Nevertheless worms break through their habit of avoiding the foot-stalk, if this part offers them the most convenient means for drawing leaves into their burrows."

Again, in the case of pine-leaves consisting of two needles joined to a common base, it is almost invariably by this base that the worm draws in the pair of leaves, and it is evident that, as the worm cannot lay hold of the two diverging points at the same time, this is the only part of the leaf by seizing which they would be able to drag the whole into their burrows. Mr. Darwin tried in some leaves tying or cementing the two diverging points together; but the worms still preferred the bases. Still further to test the hypothesis of chance, elongated triangles were cut out of paper and given to the worms instead of leaves. Here "it might certainly have been expected, supposing that worms seized hold of the triangles by chance, that a considerably larger proportion would have been dragged in by the basal than by the apical part"; while, inasmuch as the latter was in a literal sense the thin end of the wedge, it was the part which intelligent action would be most likely to choose. The results of many experiments with these paper triangles showed that "nearly three times as many were drawn in by the apex as by the base. . . . We may therefore conclude that the manner in which the triangles are drawn into the burrows is not a matter of chance, ...

and we may infer—improbable as is the inference—that worms are able by some means to judge which is the best end by which to draw triangles of paper into their burrows."

On the question of defining such action as intelligent or non-intelligent, Mr. Darwin refers to the criterion "that we can safely infer intelligence only when we see an individual profiting by its own individual experienee"; and he adds that "if worms are able to judge, either before or after having drawn an object close to the mouths of their burrows, how best to drag it in, they must acquire some notion of its general shape," and thus guide their actions by the result of individual experience.

Assuredly these observations are most interesting, and it would seem well worth while to try whether, by a series of lessons with similar triangles of paper, an individual worm could be taught to lay hold of the apex in a greater and greater proportional number of cases; if so, there could no longer be any question as to the intelligent nature of the action.

The only other observations with which we are acquainted pointing to the existence of intelligence in annelids are those of Sir E. Tennant ("Natural History of Ceylon," p. 481).

The remaining chapters of the book are occupied with the subject of its title, and in their course many quantitative results are given of the amount of mould which worms are able to cast up. Thus, for instance, a certain field was thickly covered with marl. Twenty-eight years afterwards this layer of marl was found buried by mould to a depth varying between twelve and fourteen inches. Several other similar cases are given, the most interesting being that of a field which adjoins Mr. Darwin's own house. This was last ploughed in 1841, then harrowed, and left to become pasture land. Then

"For several years it was clothed with an extremely scant vegetation, and was so thickly covered with small and large flints (some of them half as large as a child's head) that the field was always called by my sons 'the stony field.' When they ran down the slope the stones clattered together. I remember doubting whether I should live to see these larger flints covered with vegetable mould and turf. But the smaller stones disappeared before many years had elapsed, as did every one of the larger ones after a time; so that after thirty years (1871) a horse could gallop over the compact turf from one end of the field to the other, and not strike a single stone with his shoes. To any one who remembered the appearance of the field in 1842, the transformation was wonderful. This was certainly the work of the worms, for though castings were not frequent for several years, yet some were thrown up month after month, and these gradually increased in numbers as the pasture improved. In the year 1871 a trench was dug on the above slope, and the blades of grass were cut off close to the roots, so that the thickness of the turf and of the vegetable mould could be measured accurately. . . . The average accumulation of the mould during the whole thirty years was only ·083 inch per year; but the rate must have been much slower at first, and afterwards considerably quicker."

Numberless other corroborative cases are given, but we have no further space to enter into their details. Large stones are slowly undermined and sunk by worms, and woodcuts are given to illustrate actual measurements made by Mr. Darwin or his sons of the rate of sinking in particular cases. These measurements show that in the course of two or three centuries large blocks of stone (*e.g.* 67 × 39 × 15 inches) may become completely buried. Thus we are not surprised to learn that old pavements and low walls are subject to the same process, and many instances are given which have been observed by Mr. Darwin or his sons of the remains of Roman houses buried so far beneath the soil that the latter has been ploughed for years without any one having suspected the presence of walls and pavements beneath. In some cases the thickness of the mould or soil above such remains was found to be twenty, thirty, and even forty inches.

The actual weight of worm-castings thrown up in one year was calculated in one case to amount to 18·12 tons per acre.

Such being the work that worms are able by their gradual and cumulative action to accomplish, it becomes evident, as pointed out in Mr. Darwin's paper more than forty years ago, that worms must play an important part in the process of denudation. This topic is therefore treated at length, and it is shown that over and above the mechanical action already described, worms materially assist the process of denudation by the chemical actions incidental to digestion. For

"The combination of any acid with a base is much facilitated by agitation, as fresh surfaces are thus continually brought into contact. This will be thoroughly effected with the particles of stone and earth in the intestines of worms, during the digestive process; and it should be remembered that the entire mass of the mould over every field, passes, in the course of a few years, through their alimentary canals. Moreover as the old burrows slowly collapse, and as fresh castings are continually brought to the surface, the whole superficial layer of mould slowly revolves or circulates; and the friction of the particles one with another will rub off the finest films of disintegrated matter as soon as they are formed. Through these several means minute fragments of rocks of many kinds and mere particles in the soil will be continually exposed to chemical decomposition; and thus the amount of soil will tend to increase."

And,

"The several humus-acids, which appear, as we have just seen, to be generated within the bodies of worms during the digestive process, and their acid salts, play a highly important part, according to the recent observations of Mr. Julien, in the disintegration of various kinds of rocks."

Further,

"The trituration of small particles of stone in the gizzards of worms is of more importance under a geological point of view than may at first appear to be the case; for Mr. Sorby has clearly shown that the ordinary means of disintegration, namely, running water and the waves of the sea, act with less and less power on fragments of rock the smaller they are."

This assistance which worms lend to the process of denudation is of special importance in the case of flat or gently-inclined surfaces, for here it is not improbably the chief agent at work. Castings thrown up during or shortly before rain flow for a short distance down an inclined surface, and the finest earth is washed completely away. Again, during dry weather, the disintegrated castings roll as little pellets, and a strong wind blows all the castings, even on a level field, to leeward.

One other observation must be quoted, which, besides

being of interest in itself, also has reference to the important subject of denudation :—

"Little horizontal ledges, one above another, have been observed on steep grassy slopes in many parts of the world. Their formation has been attributed to animals travelling repeatedly along the slope in the same horizontal lines while grazing, and that they do thus move and use the ledges is certain; but Prof. Henslow (a most careful observer) told Sir J. D. Hooker that he was convinced that this was not the sole cause of their formation."

It is then shown that the initial cause of these ledges is the burrowing of earthworms. For,

"If the little embankments above the Corniche Road, which Dr. King saw in the act of formation by the accumulation of disintegrated and rolled worm-castings, were to become confluent along horizontal lines, ledges would be formed. Each embankment would tend to extend laterally by the lateral extension of the arrested castings; and animals grazing on a steep slope would almost certainly make use of every prominence at nearly the same level, and would indent the turf between them; and such intermediate indentations would again arrest the castings."

Thus, on the whole, it will be seen how important an agency in nature Mr. Darwin has shown the action of worms to be, so that, in his own concluding words, "it may be doubted whether there are many other animals which have played so important part in the history of the world as have these lowly organised creatures."

GEORGE J. ROMANES

OUR BOOK SHELF

The Atlas Geography. By A. H. Macdonell. (London: H. K. Lewis, 1881.)

UNDER this title Mrs. Macdonell has attempted to supply what she believes to be a want long felt in teaching geography to young children. She finds, as every teacher finds, that children prefer the map to the book, and so she provides the means of teaching geography by means of an atlas. The Atlas-Geography consists of nine double maps. First we have in each case a coloured map with the leading names filled in, and facing it a list of the leading features in the map, countries, their divisions, towns, oceans, islands, capes, rivers, &c., which the children learn by heart, fixing at the same time their positions on the maps. Following this is a corresponding uncoloured map, without names, on which the children should be able to point out the features without assistance. Facing this is an interesting and simple descriptive account of the leading characteristics of the continent or country to which the map refers. It will thus be seen that in the hands of a painstaking and judicious parent or teacher the Atlas-Geography ought to prove a most valuable help in interesting children in the subject, and in enabling them to acquire the leading facts. The maps are well executed, clear, and not over-crowded; they are the World, Europe, Asia, Africa, Australia, North America, South America, the British Isles, and Palestine.

Gesammelte Abhandlungen und kleinere Schriften zur Pflanzengeographie. The collected treatises and shorter writings on Phytogeography of the late A. Grisebach, edited by his son, Dr. Ed. Grisebach. 8vo, pp. 628. (Leipzig: Wilhelm Engelmann.)

As the editor states in his preface, the present volume combines for the first time the numerous writings on phytogeography of the late Prof. A. Grisebach, spread over a period of thirty years, and scattered in various journals and publications, several of them very difficult of access. Constant reference is made to many of these writings in the "Vegetation der Erde" (1872); hence their publication in a collected form is a great boon. In addition to those article published previous to the "Vegetation der Erde," this volume contains the author's subsequent reports (1866-76) on the progress in the geography of plants. It also contains a biographical sketch of the late Prof. Grisebach, together with the bibliography of his works. An excellent French translation of the "Vegetation der Erde" appeared in 1874, but no English edition has been published, nor would we recommend the publication of one now, because the data that have been accumulating during the last decade would justify the publication of an original work, treating the subject from a different standpoint.

W. B. H.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Solar Outburst of July 25, 1881

IN the interesting account of a solar outburst on July 25 contained in your last number (p. 508), Mr. Hennessey says that "unhappily the sun remained invisible till July 30." Referring to our sketches of the solar surface, I find that the nearest in time to the date of the outburst are those made on July 21 and 27. On the intervening days clouds prevented all solar work. The sketch on July 21 shows the groups in the [*n p*] quarter of Mr. Hennessey's disk, and that of July 27 gives those in the [*n f*] and [*s f*] portions, and also the two groups in the [*n p*] which were farthest from the centre on the 21st. There was certainly not the slightest trace on the 21st of the remarkable group which burst forth so suddenly on the 25th, and there can be very little doubt that the spots in the [*n p*] quarter on the 27th are identically the same as those in the [*n f*] quarter on the 21st. Drawings of the solar disk are made here on every available day, and the position of each spot is marked with the greatest exactness; but when the sky is cloudy, as on the 27th, it is not always possible to fill in all the details. The exact position of each spot is invariably marked before any details are sketched, and therefore, as the definition on the 27th was good, the group, which suddenly appeared near the centre of the disk on the 25th, must already have completely vanished. I might mention, in conclusion, that our magnetic photograms show no sign of any disturbance synchronous with the solar outburst.

S. J. PERRY

Stonyhurst Observatory, Whalley, September 30

On the Velocity of Light

WITH reference to Lord Rayleigh's article on the Velocity of Light (vol. xxiv. p. 382) I, and possibly others, find it difficult to follow him when he says, in the case of all the methods for determination of the velocity of light except the aberration method, that the velocity arrived at is the "group velocity," and not necessarily the "wave velocity." I, for one, should be glad of further exposition. Does not Foucault's revolving mirror experiment, for instance, measure the velocity of motion of the centre of the disturbance which is transmitted from mirror to mirror? And would it not be the case that, if the waves moved faster than the groups, new groups would be continually formed ahead, the old ones dropping out behind; so that the centre of the disturbance would not remain in any given group? Further, is any credence to be given to the result that blue light travels anything like 1·8 faster than red light, while this is unconfirmed by the colours of Jupiter's satellites?

W. H. MACAULAY

Mountsorrel, August 29

An Aquatic Hymenopterous Insect

THE following circumstance may prove interesting, and probably new, to some of your entomological readers. On Septem-

r 10, in a gathering of pond-water made in this neighbourhood d brought home for microscopical examination, a somewhat gular and unusual object presented itself, but speedily making s way to the sediment at the bottom, it became lost to sight. pon the evening of the 12th, or after the lapse of fully two ys, while holding the bottle to the light, the same object again peared, swimming or flying in mid-water with a peculiar jerky ovement resembling that of some of the Entomostraca, and r one of which I at first mistook it. On removal by means of dipping-tube to a zoophyte trough for fuller examination, it oved, to my great surprise, to be one of the small Hymeno-erous flies of the Proctotrupid family, and here it continued s active movements, now walking and running upon the bottom d sides of the trough, now flying, as it were, through mid-ater by the energetic movements of its wings, but apparently aking no effort to escape. Examination thus proving no easy sk while living, and fearing the loss of a specimen of habits unique, I decided upon securing it permanently as a micro-opic mount. With a minute description I need not now ouble you, but as neither in Westwood nor in any other of the thorities on such subjects at my command I can find any cord of this singular fact having been hitherto observed, either connection with the parasitic Hymenoptera or any other imilar insects in the perfect state adapted apparently to an erial life alone—that they should quit their natural habitat for o lengthened a sojourn in the water—I would make the inquiry s to whether any like occurrence has been noted by any of your ther correspondents. EDWIN BOSTOCK

Stone, October 6

[Our correspondent has had the good fortune to re-discover in his country the little Hymenopterous insect found almost simultaneously by Sir John Lubbock and Mr. Duchess in 1862, to which the former applied the name *Polynema natans* (*Transactions* of Linnean Society, vol. xxiv. part ii. p. 138, 1863). The nsect is parasitic in its larval stage *in the eggs of dragon-flies.* A brief summary of its peculiarities is given in Lubbock's "Origin and Metamorphoses of Insects" (Macmillan and Co., 1874). More recently Prof. Westwood has suggested (*Transactions* of Linnean Society, second series, "Zoology," vol. i. part viii. p. 584, 1879) that the insect is scarcely a true *Polynema*, but rather an *Anaphes*, or the type of a new genus. A true Ichneumon (*Agriotypus armatus*) has long been known to be parasitic in caddis-worms, and therefore also aquatic in its habits.—ED.]

Practical Physics for Boys

IN Prof. Parker's very valuable and interesting paper (vol. xxiv. p. 543) he says: "The consequences of setting large classes of young boys to make oxygen, or to take a specific gravity . . . each for himself, might prove rather subversive of order than conducive to improvement." It may be interesting to some of your readers to know that at Clifton College we have lately tried the experiment of turning some of our ordinary physics classes, numbering from twenty-four to thirty boys, bodily into the physical laboratory, where they work at weighing, measuring, finding specific gravities and such matters, under the control of a single master. The boys work in pairs, each with a little manual of instruction, and each pair with a separate cupboard of cheap apparatus. Two such classes are taken by Mr. Worthington and myself, and we are both agreed that whatever difficulties we may feel, we have none in the matter of discipline. On the contrary, the boys are with scarcely an exception most keen and eager at this work. I understand that similar classes in practical chemistry will shortly be set on foot by Mr. Shenstone in our chemical laboratory. H. B. JUPP

Clifton College

A New Comet

I OBSERVED a telescopic comet in Leo on the mornings of October 4, 5, 6, and 10. The rough positions, as I estimated them, were R.A. 9h. 22m., Dec. 16° N. on the 4th, and R.A. 9h. 36m., Dec. 15° N. on the 10th. The motion is about 30' daily eastwards. When the present bright moonlight is gone the comet will be a fairly bright object in the telescope. At the end of the present week it must be looked for immediately preceding η Leonis.

When I saw it first, on the morning of October 4, it looked like a bright nebula, and I cannot understand how I missed it on the mornings of September 29 and October 1, when I had carefully swept the same region for several hours before sunrise. The inference is that it is getting brighter. W. F. DENNING

Ashley-Down, Bristol, October 10

A Kinematical Theorem

SOME little time ago Mr. Kempe published in NATURE a theorem of interest in kinematics. I subsequently stated in the same pages that this theorem and all theorems of uniplanar kinematics are most simply and properly proved from the consideration that epicycloidal motion is the basis of all uniplanar motion—and that this is also the proper principle on which to base the theory of planimeters. It may not be out of place to occupy a few lines in NATURE with another curious kinematical theorem allied to Kempe's, which I have just found by this method. *If a plane, A, move about in any manner over a fixed plane, B, and return to its original position after any number of revolutions, all those right lines in the plane A which have enveloped glisettes of the same area, are tangents to a conic, and by varying the area of the glisette we obtain a series of confocal conics.* I use the term *glisette* under protest—"line roulette" would be better, as the former name is more applicable to a curve of another sort. GEORGE M. MINCHIN

Royal Indian Engineering College

Integrating Anemometer

MY attention was called to a letter on this subject in your issue of the 29th ult. (vol. xxiv. p. 510), though not in time to enable me to answer it last week. I take this opportunity of stating that the gentleman to whom the idea of the instrument was originally due, and who has defrayed the whole cost of its construction, is the Rev. J. M. Wilson, M.A., head-master of Clifton College (not Dr. Wilson, as mis-stated in the Association *Journal* at York and in your abstract). The objection that the air does not move "parallel to itself," by which I presume is meant in planes parallel to its general direction, does not apply to this any more than to any other cup anemometer. Only the horizontal component of the wind's velocity is sought, and this is given with tolerable accuracy. I have no means of knowing to what extent Mr. Burton's integrator resembles the anemometer in question, but it should be noticed that the two instruments are of a different kind and for a different purpose. Mr. R. Scott was in the chair when the paper was read at York, and joined in the discussion. Prof. Stokes was also present, and has since been in correspondence with me on the matter. Neither of these gentlemen, however, mentioned any other instrument as at all resembling it; indeed upon its being compared to that of Dr. von Oettlinger, Mr. Scott took occasion to point out at least one important difference, viz. the cost. H. S. HELE SHAW

University College, Bristol, October 10

Infusorial Parasites on Stickleback

MR. N. H. POOLE (NATURE, vol. xxiv. p. 485) is apparently right in anticipating that he has discovered either a new habitat for *Trichodina pediculis* or a new representative of that infusorial genus. Although hitherto regarded as a parasite only of the fresh-water polypes, *Hydra vulgaris* and *H. viridis*, I have recently obtained specimens of the type in question living as a parasite, or rather a commensal, on the branchial appendages of the larva of the common newt, *Triton cristatus*. An allied, but marine species, *Trichodina scorpænæ*, has been recently described by Prof. Ch. Robin, that infests in a similar manner the branchia of fishes belonging to the genera *Trigla* and *Scorpæna*, and a further search will no doubt reveal a yet more extensive distribution of the Urceolaridæ, including *Trichodina*, among the Piscine race. Mr. Poole will find full particulars of the data here referred to, together with an account and illustrations of all the forms so far relegated to this somewhat remarkable infusorial group, in Part V., p. 645 *et seq.*, of my "Manual of the Infusoria," just published. W. SAVILLE KENT

The Dark Day in New England

REFERRING to your paragraph in last week's NATURE (p. 540) about the remarkable phenomenon which occurred in New England on September 6, I find in the recently-published "History of Lynn, Massachusetts," the following:—

"1716.—Extraordinary darkness at noonday October 21st; dinner tables lighted."

"1780.—Memorable dark day May 19th; houses lighted as at night." CHARLES W. HARDING

Lynn, October 7

THE EVOLUTION OF THE CRYPTOGAMS[1]

II.

THE direction and many of the gradations through which the highest classes of the vegetable kingdom have been developed from the lower are preserved in the palæontological record. In order to decipher them, however, certain facts must be kept in view: chiefly, that the higher and more complex organisations, are the most susceptible to changes in the external conditions upon which they are dependent, and therefore more readily destroyed, while the simpler the organisation the more yielding or plastic it is, and the greater the chance that it will be able to survive by adapting itself to change. Thus the superb Cryptogams of the Carboniferous succumbed no doubt to great physical changes, but the more humble of them bent to the new conditions, and even found therein an impetus leading to unexpected developments, which eventually carried them far beyond their more advanced brethren.

Tracing back the origin of vegetable life, we see that it consisted nearest its source solely of Algæ. A little later, Cryptogams appeared, and developed their maximum during the Palæozoic period. Next, almost synchronously, Gymnosperms are met with, and after a long time preponderate; and then Angiosperms, obscure and subordinate at first, begin, towards the close of the Secondary period, to take the first rank.

Most of the lowest Algæ, such as Ulva and Conferva, are scarcely of a texture to have left traces of their existence, but eight still existing Diatoms have been discovered in British Coal.

The next group, morphologically, of Algæ—the Siphoneæ—have been shown by M. Munier-Chalmas to be

FIG. 1.—*Bilobites furcifera*, d'Orb. Part of a "Phyllome," with traces of expansions and ramifications; half natural size. Silurian of Bagnols.

abundant in the Trias and Secondary rocks, and to be analogous, or perhaps identical, with the existing Cymopolia and Acetabularia. It is unfortunate that, owing to the texture of most of the Algæ, observation has to be concentrated on the few groups that could be preserved. In the Silurian the remains of these are numerous, and of forms completely differing from existing types.

Following the primordial Palæozoic forms, there appear successively the more highly organised Groups, Characeæ in the Trias, Laminariaceæ in the infra-Lias, and finally Fucaceæ in the Eocene.

The Mosses and Liverworts, which seem to indicate the stages through which Algæ gradually became adapted to terrestrial conditions, are unknown in the older rocks; yet, far from assuming that they did not then exist, we should rather consider how exceedingly unfavourable are the conditions under which marine and estuarine strata are deposited to the chance of their becoming imbedded.

The order Calamareæ, as the authors prefer to call the Equisitaceæ, include such diverse types as Calamites, Annularia, Asterophyllites, and Equisteum, though Camalodendron and a few other forms are excluded. The group is characterised by the arrangement of their organs in whorls, whether these are true leaves or the modified leaves which support the sporangia. The sporangial whorls either occur together and form a terminal fruit, or are placed alternately with whorls of true leaves, and the sporangial bracts are either disunited or coalesce to form a sheath. Modifications of one or

[1] "L'Evolution du Règne Végétatale." Les Cryptogames. Par MM. Saporta et Marion. Bibliothèque Scientifique Internationale, xxxi. (1881.) Continued from p. 75.

other of these characters are the foundations of all the Palæozoic genera yet known. In the extinct Carboniferous forms the fertile or sporangial whorls alternated with, and were protected by, the overlapping whorls of barren leaves, while in Equisetum the sporangial whorls are naked and clustered in a terminal spike, an arrangement considered by Saporta and Marion to be more favourable to the dispersion of the spores. Annularia and Asterophyllites were floating or procumbent plants. Calamites strongly resembled the existing aquatic Equisitaceæ, though ex-

FIG. 2.—*Bilobites Vilanovæ*, Sap. and Mar. Base of a "Phyllome." Silurian of Andalusia.

ceeding them twenty times in size, and surpassing them in development by the possession of spores of two sexes. Their more complex structure and consequent inadaptability to changed conditions, favoured, the authors believe, their early extinction in the Permian. In the Trias, and until the Jurassic, several slightly modified genera coexisted with true Equisetum, and the survival of the latter, one of the genera that have persisted almost unchanged from the Carboniferous, is probably due to their simple

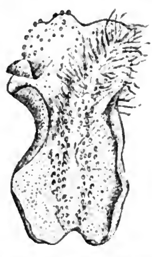


FIG. 3.—Prothallus of an Osmunda aged eight months, slightly magnified to show the double row of archegones down the centre.

organisation, easy dispersion of the spores, and the immense depths to which their rhizomes penetrate.

The structure of ferns, unlike that of Equisitaceæ, lends itself to infinite diversity. The fronds may be simple or multipartite, without their form implying the slightest degree of relationship, and supposed alliances between fossil and recent ferns, such as Ettingshausen has based upon the aspect and venation of the frond, are declared by the authors to be valueless and misleading.

The earliest ferns had simple fronds, and probably resembled in their vegetative organs the Hymenophylleæ, a group already well represented in the Carboniferous. Next in order come the Osmundaceæ, if the relative

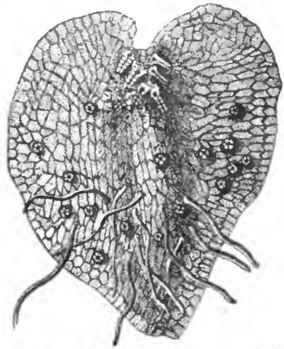


FIG. 4.—Under side of the prothallus of another fern, more magnified, showing the rhizoid radicles, the antherids dispersed over the surface, and the archegones clustered at the terminal notch.

complexity of their prothallus and simplicity of sporangia are accepted as indications of inferiority.

The relative perfection of the sporangium when taken

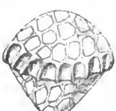


FIG. 5.—Sporangium of Hymenophyllum, girt transversely by the ring of cellules which disrupt the spore-case.

as the essentially important organ, leads to a classification coinciding approximately with the order in which the groups made their appearance:—Hymenophylleæ

FIG. 6.—Sporangia of Osmundaceæ, showing dorsal dehiscence. 1 and 2, sporangia of *Todea Africana*—1, ventral surface; 2, dorsal surface; 3, sporangium of Osmunda seen dorsally, and showing the infra-apical group of cellules which disrupt the spore-case.

Osmundaceæ, Schizeaceæ, Gleicheniaceæ, Marattiaceæ, Cyatheæ, Polypodiaceæ.

From the simplest type of sporangium, two lines of

increasing differentiation in the organ, or its support, can be traced—one leading to the Polypodiaceæ through the Cyatheæ, the other to Schizeaceæ, Gleicheniaceæ, and Marattiaceæ.

The earliest fern of which the fructification is known is the Devonian Palæopteris, Schimper. Its fructification consists of aborted leaflets supporting groups of oblong, ringless sporangia opening into two valves and disposed in threes on pedicles. Rhacopteris, of the same age, and perhaps not generically differing, has fructification which unites in a higher degree the characteristics of Osmunda and Botrydium, and giving birth probably to the Botryopterideæ of the later Carboniferous flora. Another genus, Seftenbergia, is allied by the structure of its sporangium to Angiopteris (Marattiaceæ), though each sporangium is as yet isolated. The Palæozoic ferns did not at this period essentially differ from Osmunda and Todea.

The earliest example of definite grouping in the

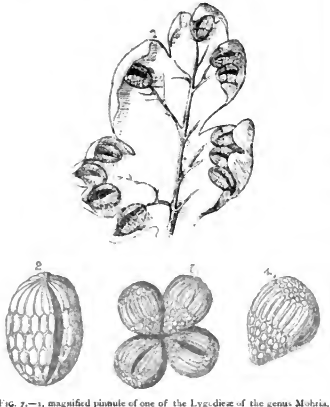


FIG. 7.—1, magnified pinnule of one of the Lygodieæ of the genus Mohria, showing the arrangement of the sporangia on the under side of the frond; 2, sporangium of the same, showing the group of apical cellules which disrupt the spore-case; 3, sorus of Gleichenia, showing the peripheral arrangement of the cellules which disrupt the spore case; 4, sporangium before dehiscence.

sporangia is furnished by Oliogocarpia, in which three to five sporangia are inserted on a point near the ultimate terminations of the venules; but even here, though contiguous, they are distinct, and can be separated. In the later Carboniferous, Marattioid ferns for the first time occur with the sporangia united in a composite organ called a synangium, and soon after the Marattiaceæ reached their maximum development, and commenced, through forms now extinct, to differentiate towards the Gleicheniaceæ. The stages of development of the latter, and of the Schizeaceæ, are more difficult to trace, though both are represented in the Palæozoics by Howlea and Seftenbergia respectively. The actual genus Gleichenia does not appear until the inferior Oolite, and Lygodium until the Cretaceous.

The Cyatheæ are represented in the Carboniferous by Thyrsopteris and in the Jurassic by Dicksonia, while true Polypodieæ cannot be traced farther back than the Rhætic. They seem to have developed suddenly, and among them are a number with their sporangia grouped in sori as in Gleichenia, yet possessing in other respects the structure characteristic of Polypodium.

The Ophioglossaceæ are related to the most ancient ferns by the arrangement and structure of their sporangia, and to Isoëtes and Lycopods by the form of their prothallus. They even present affinities with Sigillaria, and represent, the authors conjecture, an almost unchanged type, older than the differentiation of either ferns, Lycopods, or Rhizocarps.

The Lycopodiaceæ are divided into isosporous and

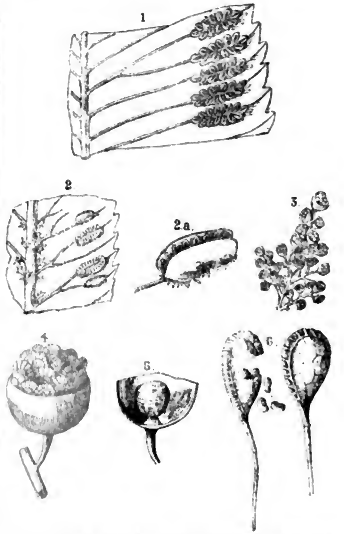


FIG 8.—1, part of pinnule of Angiopteris, with sporangia clustered in groups, but not united; 2, part of pinnule of Marattia with sporangia joined together in a synangium; 2a, a synangium magnified; 3, extremity of the fertile frond of one of the Cyatheæ, *Thyrsopteris elegans*; 4, a magnified receptacle of the same, in form of a pedunculated cup, full of sporangia which are girt with a jointed ring of cellules; 5, section through an empty cup, showing the support to which the sporangia are attached; 6, two highly-magnified sporangia of Polypodiaceæ, one dehiscent, girt vertically by a jointed ring and on pedicles.

heterosporous kinds. The former, comprising Lycopodium and a few tropical genera, have been found fossil in the Old Red of Thurso and the Carboniferous of Saarbruck and Autun, their small size and retiring habits having doubtless caused their relative rarity in stratified rocks. The heterosporous, or more perfected kinds, obtained a magnificent development in the Carboniferous, favoured by the warm and humid climate, free from seasonal changes, which then seems to have prevailed, and only declined when these conditions ceased. They are at present represented by Selaginella, a genus which has scarcely changed since the Carboniferous. The

sporangia are globose, pedunculated, and situated towards the base of the bracts which compose the fruit-

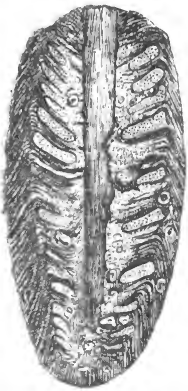


FIG. 9.—Lepidostrobus, the reproductive organ of Lepidodendron. Longitudinal section of sporangiocarp of *Lepidostrobus Dabadianus*, Schimp., showing the lower sporangia containing macrospores and the upper microspores; half natural size.

spikes, and either contain two to eight macrospores, or small and very numerous microspores. In the germination of these spores the approach towards Gymnosperms becomes exceedingly apparent, and is consequently dwelt on at some length, the researches of Sachs, Luersen, and others being largely referred to. An even higher stage was in all probability reached in the Lepidodendrons, the vigorous and splendid growth of which formed the culminating development of the Lycopodiaceæ. The mathematical regularity of their growth, even in the most minute internal structure, is very striking. They formed large trees with acicular or falcate, perhaps deciduous leaves, and bore cones in pairs at the extremities of certain branches, differing exteriorily but little from those of Gymnosperms. The expanded bases of the scales or bracts bore the sporangia, those containing the macrospores being nearest the base. The stem comprised several layers, the centre being of pith formed of elongated prismatic cells. The next layer was woody, and gave off simple vascular threads to each leaflet, these penetrating obliquely the succeeding region of parenchyma and the cortical layers. The bark increases in density towards the exterior, and in some species the interior pith is absorbed in the woody layer.

Lepidodendron, with the greater part of the Palæozoic flora, became extinct during the Permian, leaving as representative the humble Isoëtes. This, however, is not necessarily a degraded type, and may have existed since Palæozoic times, though only known fossil in the later Eocenes, where it in no way differs from existing forms.

The Rhizocarps are beyond doubt the highest existing form of Cryptogam, but though in many respects so nearly approaching to Phanerogams, they are not, as we see them now, in the absolutely direct line of evolution. In all, the sporangia are protected by an enveloping altered leaf, or segment of a leaf, forming a fruit called a sporocarp, which in most cases attains a high degree of complexity. The entire group is aquatic, and stands in the same position towards fossil Rhizocarps that Isoëtes does to the Lepidodendrons. The Carboniferous Sphenophyllum has been shown to correspond to Salvinia, and the Rhætic

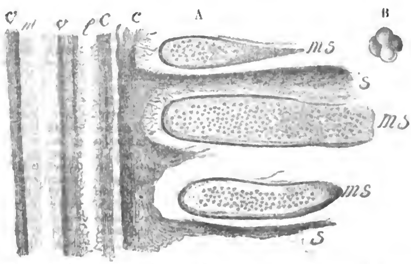


FIG. 10.—Reproductive organs of Lepidodendron with microsporangia and microspores. A, longitudinal section through upper part of sporangiocarp of Lepidostrobus (probably *L. Brownii*, Schimp.), from the neighbourhood of Pézenas (Hérault); M, the central medullary region formed of parenchyma with elongated prismatic cells; V V, the woody layer of fibro-vascular region next the pith, showing large scalariform vessels; L, lacuna, in which delicate and partly disintegrated cells are studded; C C, cortical layer composed of a very dense outer and an inner layer, separated by a loose, nearly destroyed parenchyma; S, "sporangiospores," supporting very elongated microsporangia filled, M S, with microspores aggregated in fours (these are slightly exaggerated in size); B, group of microspores magnified.

Sagenopteris to the Marsiliaceæ. Though the vegetative organs in the extinct forms attained far finer proportions and a higher and more delicate structure, the fructification, in Salvinia especially, appears the more complex. The existing genera have only been met with in the Eocene and Oligocene.

Thus while Angiosperms all present similarity in the reproductive process, Cryptogams preserve many of the stages by which the evolution of the higher forms has been accomplished. They also present every gradation in their vegetative organs, from the simplest and purely cellular plant to the equals of Phanerogams in point of structure. Except the Protophytes, all Cryptogams are impregnated by antherids, and present the antagonistic and alternate asexual and sexual generations, these being in fact their distinguishing characters. The authors' task

—to trace step by step the progressive stages by which the prothalloid phase has been diminished, and the ever, though gradually, increasing approach in the complexity and mode of reproduction to the Phanerogams—has demanded the most patient and prolonged research. The promised second volume will further diminish the hiatus still left between Phanerogams and Cryptogams, and make clear, the authors believe, the precise lines through which the evolution of the one from the other has been accomplished.

J. STARKIE GARDNER

MUSEUMS AND EXHIBITIONS IN JAPAN

THERE is probably no function of Government which the rulers of new Japan have performed so adequately and thoroughly, with such persistence and such unvarying success, as that which consists in the education of the people. It would be impossible in the space at our disposal to describe the course and results of education under the usurpation of the Shôgun; suffice it to say that, though learning of a peculiar kind always received support and encouragement, these were given on no sound or general system. The masses were neglected as beneath consideration, while literary labour of the best kind was always rewarded. No Japanese Horace need ever have lacked a generous Mecænas. But it was not until the restoration of the Mikado and the overthrow of the feudal organisation that a system of universal education which should reach the lowest classes of the people was introduced, and the Government of Japan then looked abroad to those Western lands, to which the eyes of all Japanese were then turned, for models on which to base their new scheme. American teachers of eminence were first brought to the new University established in Tokio; these were soon followed by subordinate instructors for the various schools in the local centres, and in six years after the restoration there were two large educational establishments—the University and the College of Engineering—besides numerous smaller ones in the capital, while every administrative division had its central school—all provided with competent foreign professors or teachers. A large normal college in Tokio trained instructors for the schools in the interior in Western knowledge and Western methods of teaching; and from that time to the present the wise and beneficent system of general education adopted by the Government has gone on extending itself into the remotest parts of the country. As mentioned in a previous article, the number of foreign instructors was gradually reduced, first in the interior, afterwards at the capital, as Japanese trained at home or abroad became competent to take their places. The history of this remarkable spread of education in Japan will be found in the annual reports of the Minister of Education to the Emperor, and in an excellent series of papers published by the Japanese Commissioners to the Philadelphia Exhibition. The spirit in which this work is carried out is well shown in a circular recently issued by the Minister of Education for the guidance of teachers in elementary schools. According to the *Japan Mail* this document contains sixteen clauses, embodying a number of directions for the conduct of school officials. The chief points are (1) "the importance of imparting a sound moral education to the students, both by precept and example, since the condition of a man's heart is of far greater moment than the extent of his knowledge; (2) the necessity of proper hygienic arrangements, which have more effect upon the health of the students than gymnastics or any other physical training; and (3) the value of mental energy in a teacher, for without it he cannot possibly support the fatigue and trouble of really careful tuition." The circular goes on to advise teachers not to constitute themselves advocates of any particular religious or political doctrines, and to take every opportunity of increasing their own stock of knowledge.[1]

But while thus caring for the education of the youth of the country, that of its risen generation has not been neglected. Besides annual and triennial domestic exhibitions, museums have been established in most of the large towns in the country, and it is to these we would more particularly refer in the present article. It should be remarked at the outset that the Japanese are a nation of sightseers; not the vulgar, pushing, noisy mob to which we are too much accustomed in this country, but a quiet, orderly, pleased, and pleasing crowd. They are always anxious to see something new; failing this, they are content with their own temples and ancient festivals. In a visit to any of the numerous museums of Tokio on a Sunday or other holiday, the stranger from the West cannot fail to be struck with the order, good humour, and never-failing interest manifested by the people. The descriptions of the objects are generally very full and clear, so as to bring them within the meanest comprehension; and when these are read out to a group by some one more learned than the rest, the exclamations of wonder, admiration, or delight are incessant, and form a pleasing contrast to languid or imperfect interest frequently taken by our own crowds in their museums.

The first museum in Japan—the *Hakubutsu-kuwan*—was opened, as an experiment, in 1873. A few objects of Western manufacture and some Japanese productions were placed in the Confucian Temple, situated in one of the prettiest suburbs of Tokio. Vast crowds, attracted chiefly no doubt by the foreign exhibits, visited the place daily; and the Government, acting, we believe, on the advice of the governor of the city, determined to enlarge the exhibition considerably and make it permanent. It was accordingly removed to a more central position, the partially-dismantled residence of one of the old nobles being chosen for this purpose. Here the collection was deposited and gradually increased, until at the present time it fills a range of narrow buildings nearly a quarter of a mile in length. This may be called the permanent museum of the capital. A visit to it would strike one accustomed to the museums of Europe with a certain sense of incongruity. Close to the lacquered bullock-carts and chairs of the emperors of a thousand years ago we find English machinery of yesterday: in one compartment we see art treasures of a remote antiquity; in the next, Minton and Wedgwood; a corridor containing a large and valuable collection of the old paper currency brings us, it may be, to a collection of modern glassware. This jogging together of the ancient and modern, of articles familiar in the homes in the West with the priceless art rarities of the East; of the products of the skill and loving care of old Japanese artists with, we may almost say, Brummagem, jars unpleasantly on foreign taste. But it must be remembered that this establishment is founded, not for the educated foreigner or even native, but for the Japanese shopkeeper, farmer, artisan, and labourer, whose interest is not a whit diminished when he passes from a beautiful antique relic to a Bradford loom or a copy of the Milton shield. Indeed, we are not sure that

[1] As an instance of the general spread of elementary education, the present writer may take this opportunity of mentioning what he saw during an examination of some of the principal Japanese prisons in the summer and autumn of last year He found all the children and youths in gaol—in some cases numbering a few hundreds—attending the prison schools for four or six hours each day, while the adults attended in the evenings and on Sundays. He saw in the chief penal settlement in Tokio about three hundred boys learning reading, writing, and the simple rules of arithmetic. In the senior class the boys were learning ciphering *with European figures* from one of their own number. In the large prisons a teacher or teachers form portion of the staff; they are assisted by convicts who act as ushers or monitors. In the smaller ones an inmate—generally a political prisoner—is selected as master, and enjoys in return certain small advantages. The prison system of Japan, theoretical and practical, would well repay examination at the hands of a competent authority on penal discipline. The present Governor of Hongkong, Sir John Pope Hennessy, who has had much experience in the subject in his various governments, has expressed his high appreciation of the excellent condition of Japanese prisons.

the modern exhibits, trumpery as many of them would undoubtedly appear to us, do not attract more attention than the productions of ancient Japanese art industry. We have even heard it suggested that, by placing these various articles under one roof, the Government desired to check in their people an unreasoning admiration of everything foreign, by showing them what Japanese themselves have done in the olden time.

It would be impossible here to describe in detail even the most striking museums of the capital. The Government of the great northern island, Yeso, have established one containing specimens of the flora, fauna, and other productions of that territory near the sacred grounds of Shiba. In the great park at Uyeno, a northern suburb of the city, the Education Department exhibits all the educational appliances of most of the civilised countries of the globe; while in the same neighbourhood is a smaller museum containing a collection of ancient art treasures, to which the Emperor himself has contributed. In all the chief towns throughout the country also—notably in Osaka, Kioto, and Nagoya—museums have been established by the local authorities. Sometimes these contain only specimens of the productions, natural and artificial, of the province in which they are situated; but generally objects of more universal interest are to be seen. These, as we have before remarked, are thronged on holidays by crowds of eager sightseers, and it would be difficult, more especially for a foreign observer, to estimate accurately their beneficial effect on the nation at large, in humanising the people and stimulating healthy competition and production.

The temporary exhibitions have been not less successes than the permanent museums. An annual exhibition of domestic products is held at Kioto, in the old palace grounds, and lasts for 100 days; and a triennial one on a large scale takes place in Tokio. This also is reserved for domestic productions. The second of these has just been closed, an Imperial prince representing the Emperor at the closing ceremonial. His Majesty, having attended at the inauguration and at the distribution of prizes, was able to say (we quote from the report of the *Japan Gazette* newspaper) that there were over 800,000 visitors in 122 days. Each of the speakers on this occasion bore witness to the value of these exhibitions, and noticed the marked improvement in the exhibits now over those of three years ago.

The prospectus of a domestic exhibition of trees and shrubs has just been issued. It is to take place in February next year, and besides specimens of the forests and plantations under Government, private individuals are invited to send exhibits of timber. The exhibition will be under the control of the Department of Commerce and Agriculture, and the result will doubtless be an interchange of knowledge which will be of the utmost value in a country where wood is one of the most universal necessaries of life.

Two years since a most interesting exhibition took place at Nara, the site of an ancient capital of Japan. It was confined wholly to Japanese antiquities, and was under the direct patronage of the Emperor, who contributed many of the most valuable articles. We have referred in a previous paper to the success of an exhibition of the various instruments which have from time to time been employed to test the direction and intensity of earthquake shocks, which was held under the auspices of the Seismological Society of Tokio.

As cognate to the subject of this article we may refer to the public libraries of Japan. Lending libraries have existed in the country from very early times; but it is only recently that the Government have provided large collections of native and foreign works for students. One free library in Tokio, which was founded in 1873, contained a year ago 63,840 volumes of Chinese and Japanese works, 5162 English books, 6547 Dutch, and about 2000 volumes in other European languages. It possesses a large reading-room, provided with many leading foreign journals; admission is wholly free, and permission to borrow books for a certain period is easily obtained. The number of readers is about three thousand a month. Another, containing about 143,000 volumes, including many ancient books and manuscripts, is practically free, an entrance fee of less than a halfpenny being charged. In addition to these many of the leading towns throughout the country are provided with free libraries, which are much used and appreciated by students. The cost of foreign books renders these institutions peculiarly valuable to natives, who, as a rule, cannot afford to pay our heavy prices.

It will thus be seen that the introduction of museums and similar establishments was a happy move on the part of the Japanese Government; they are heartily appreciated by the people, and their educating influence is immense. With the exception of the newspaper press no Western institution has been so rapidly or so successfully acclimatised in Japan.

THE INTERNATIONAL EXHIBITION AND CONGRESS OF ELECTRICITY AT PARIS[1]

III.

THE Congress held its concluding sitting on Wednesday, the 5th inst., and was formally dissolved. Three international Commissions are to be appointed in accordance with the recommendations of the Congress, viz.:—

1. A Commission to determine what length of mercury at zero Centigrade, with a section of a square millimetre, has a resistance equal to the theoretical Ohm, that is, to 10^9 C.G.S. units.

2. A Commission for the following distinct purposes:—To arrange for a general system of observations of atmospheric electricity; to arrange for a general system of observations on earth-currents; to determine the best system of lightning-conductors; to investigate the practicability of a general system of automatic transmission by telegraphic wires of the indications of meteorological instruments. The idea of this last investigation is taken from the apparatus of M. van Rysselbergh, which we described in a previous notice. In fact it is understood that the Committee will report on the advisability of extending to Europe generally the system which already exists in Belgium.

3. An International Commission for fixing upon a standard of luminous intensity, to be used in measurements of electric lights, and for deciding upon the best methods of making such measurements.

The following recommendations have also been made by the Congress:—That the diameters of wires employed in telegraphy be stated in millimetres; that the cultivation of the gutta-percha tree be guarded by suitable regulations, to prevent this important product from becoming scarce; that the Governments of the different countries be requested to legislate on the subject of submarine cables, the present state of the law being insufficient to guarantee the rights of property in such cables.

In illustration of the present state of things Dr. C. W. Siemens mentioned a case where a cable which his firm had laid was wilfully cut by a captain who had caught it with his anchor in deep water, and the law afforded no remedy. It is also understood that regulations are to be made as to the repair of cables which are crossed by other cables belonging to a different company.

A further recommendation, that all countries should adopt for ships engaged in laying cables the same code of signals which is already in use in English ships was withdrawn upon the presentation of indubitable evidence that the code in question was adopted months ago in a note signed by the representatives of all the nations concerned.

[1] Continued from p. 513.

All the proceedings of the Congress have been conducted in French, and it was a novel sensation to most of us to see our English friends mount the tribune and deliver their sentiments in French; a still more novel sensation to those who for the first time ventured upon such an undertaking themselves. You first rise in your place and say, *Je demande la parole*, at the same time holding up your hand to catch the eye of the president. On his replying, *Vous avez la parole*, you walk from your place to the tribune, which is a raised platform in front of the audience, and there, with the eyes of the assembled *savans* of Europe fixed upon you, you must carry out your rash undertaking, with all your imperfections on your head. It is like the sensation of diving for the first time into deep water, where you must swim or drown.

In these international gatherings very wide deviations from the correct standards of grammar and pronunciation are indulgently tolerated, and the English have certainly not appeared to disadvantage as compared with the Germans; though it has been by no means a rare occurrence to see a speaker of either of these nations in sore straits for want of a word. There is one great advantage in conducting a Congress in a foreign tongue, and that is that the difficulty of the situation puts a wholesome check upon any tendency to verbiage on the part of a speaker; he is glad to express his meaning in the simplest manner that he can, and to desist as soon as his laborious task is accomplished; but this advantage is to some extent lost where, as on the present occasion, the language is the native tongue of half the members of the Congress. Some of the later sittings were decidedly dull and unprofitable, being mainly occupied with prolix dissertations of no general interest. The *Salle des Séances*, with its draped walls and high canvas roof, is very stifling to the voice, and much of what was said was insufficiently heard by the bulk of the audience.

The official reports of the proceedings were taken not by shorthand writers, but by young men skilled in science, who wrote abstracts of the speeches in longhand during their delivery; and it must be acknowledged that they did their work exceedingly well. The report thus taken of each meeting was printed and laid before the members at the next meeting, to be adopted before proceeding to any other business. It is called the *procès verbal*, and is treated like the minutes of an English meeting, but it is much fuller than our minutes usually are.

So much of these reports as relates to the discussions on units has been reprinted in the *Révue Scientifique*, No. 13. We have not observed reprints of any other discussions of the Congress.

The jury are now hard at work. They have divided themselves into six groups, which are subdivided into fourteen classes according to the first fourteen classes of the catalogue; and some of the more important of these classes have been still further subdivided; the total number of jurors being about 150, one-half of whom are French. By the help of this division of labour the official inspections of the exhibits have been, we believe, completed; but some days will be devoted to carrying out a series of experimental tests, which have already been commenced; and it is probable that some valuable data relative to electric lights and the machines which furnish their electricity will remain as one definite result of the present Exhibition.

In connection with these experiments a good story is told respecting resistance-coils. An eminent firm sent off several patterns of resistance-boxes to the Exhibition, but being out of one of their favourite types, they supplied its place by an empty box having exactly the outward appearance of the genuine article. As ill-luck would have it, the jury selected this particular box as being precisely what they wanted to assist them in their experiments, and asked for the loan of it. The representative on the spot, being ignorant of the sham, and appreciating the compliment paid to his house, lent the box with the utmost alacrity. The result can be better imagined than described. Application was then made to another eminent firm for a box which occupied a conspicuous position in their case of exhibits; and this also turned out to be a dummy, but the joke was not carried so far this time, as the representative in charge at once declared the fact.

(*To be continued.*)

NOTES

THE subscriptions received for the Rolleston Memorial Fund up to the present date amount to about 530*l*. It is hoped that this sum may shortly be considerably augmented, especially by subscriptions expected to be received from Oxford at the beginning of the present term. All promoters of the movement are requested to make its existence known to others likely to interest themselves in the matter. The treasurer is Mr. E. Chapman, of Frewen Hall, Oxford. A general meeting will shortly be held to determine finally the form which the memorial shall take.

SOON after the death of the late Prof. Rolleston, F.R.S., the delegates of the University Museum at Oxford, acting with the advice of Prof. W. H. Flower, F.R.S., requested Mr. Robertson and Mr. Hatchett Jackson of the Anatomical Department to set in order the collection of Crania in the Museum illustrating the various races of mankind. The compilation of the Catalogue has just been completed by Mr. Hatchett Jackson, and the specimens arranged in the cases by him and Mr. Robertson. The collation of the Catalogue and the numbering of the specimens will shortly be carried out by the latter gentleman. The method of arrangement is that adopted by Prof. Flower in the recently-issued Part I. of the Osteological Catalogue of Vertebrated Animals in the Museum of the Royal College of Surgeons. Students will consequently be enabled to compare with ease the Oxford collection with the collection in the Hunterian Museum. The numbers at Oxford range from 1 to 1053 approximately—a rather larger total than the corresponding section in Prof. Flower's Catalogue. The Oxford collection is peculiarly rich in English specimens of a date prior to the Conquest. There is a unique series of Crania from various Long Barrows; and from the Round Barrows of the Yorkshire Wolds, obtained by Canon Greenwell in his excavations and presented by him to the University, together with other specimens chiefly from cist burials of the late Bronze period. The Roman and Roman-British number 180; the Anglo-Saxon 96. The races of Ancient Egypt, of India with Ceylon, of New Zealand, the American Continent, and the various regions of Australia are well represented. There are five Tasmanian, seven Andamanese crania, and fine specimens of Zulus and Bushmen. There are besides large stores with which at present it has been found impossible to deal. And in the Catalogue as it stands are not included various skeletons and two sets of life-like casts—one set, replicas of those obtained in the voyage of the *Astrolabe* and presented many years ago to Dr. Acland, then Lee's Reader of Anatomy at Christ Church, by Prof. Milne-Edwards the elder; the other set, purchased in 1869, and representing various aboriginal tribes of Australia. It may be added that during the present Long Vacation, Miss Cracroft, niece of the late Lady Franklin, has presented to the Anatomical Department fourteen portraits of Tasmanian aborigines, authenticated with the names of the individuals, their ages, and the districts whence they came, and admirably executed in water colour by Boch.

AT a public meeting of the University College (London) Chemical and Physical Society, to be held on Friday, October 21, at 7 p.m., Prof. Alex. W. Williamson, Ph.D., LL.D., F.R.S., will deliver an address on "An Error in the Commonly-accepted Theory of Chemistry."

WE regret to learn from the *American Naturalist* of the death of Mr. Carlile P. Patterson, Superintendent of the U.S. Coast Survey. It is supposed that Mr. Julius E. Hilgard, for a long time second officer of the Survey, will be promoted to the vacant post.

MR. ETHERIDGE, F.G.S., is, we are informed, leaving the Geological Survey to be Assistant-Keeper of the Geological Department at the British Museum of Natural History, South Kensington.

THE meeting of the Iron and Steel Institute being held in London this week is probably the most interesting and important since the Institute was founded. Representatives of nearly every foreign Government are present, and the muster of foreign members is unusually large. Several of the papers are of great practical and even scientific interest, and are sure to attract much attention and give rise to discussion. On Tuesday visits were paid to Messrs. Siemens' works at Woolwich, and to the Victoria Docks, and in the evening the Lord Mayor entertained the Institute at dinner. Yesterday afternoon a visit was made to Woolwich Arsenal, and in the evening the annual dinner of the Institute was held at Willis's Rooms. To-day the Small-Arms Factory at Enfield is to be visited, and the Carriage Works of the Great Eastern Railway at Stratford, and in the evening a *conversazione* will be held at South Kensington Museum. To-morrow will be devoted to a visit to Newhaven and Brighton.

THE scientific lecturers this winter at the London Institution, Finsbury Circus, will be Mr. Grant Allen ("An English Weed"); Prof. H. E. Armstrong, F.R.S. ("The Economical Use of Coal-gas for Lighting and Heating"); Prof. W. E. Ayrton, F.R.S. ("The Storage of Power"); Prof. R. S. Ball, F.R.S. ("Comets"); Dr. Lionel S. Beale, F.R.S. ("A Living Particle"); Prof. R. Bentley ("Materials used for Paper"); Dr. James Geikie, F.R.S. ("The Ancient Glacier-systems of Europe"); Prof. J. W. Judd, F.R.S. ("Are there Coal-fields under London?"); Prof. E. Ray Lankester, F.R.S. ("Scorpions, Terrestrial and Marine"); Prof. O. J. Lodge ("Electricity *versus* Smoke"); Mr. John Perry ("Spinning-tops"); Dr. W. H. Stone ("Singing, Speaking, and Stammering"); Mr. James Sully ("The Causation and Phænomena of Dreams"); and the Rev. J. G. Wood ("The Horse's Hoof").

A LETTER was read at the recent Social Science meeting at Saratoga from Mr. Charles Darwin to Mrs. Emily Talbot, in response to her inquiries as to the investigation of the mental and bodily development of infants. He specifies points of inquiry which it seems to him possess some scientific interest. "Does the education of the parents, for instance, influence the mental powers of their children at any age, either at a very early or somewhat more advanced stage? This could perhaps be learned by schoolmasters or mistresses, if a large number of children were first classed according to age and their mental attainments, and afterwards in accordance with the education of their parents, as far as this could be discovered. As observation is one of the earliest faculties developed in young children, and as this power would probably be exercised in an equal degree by the children of educated and uneducated persons, it seems not impossible that any transmitted effect from education would be displayed only at a somewhat advanced age. It would be desirable to test statistically, in a similar manner, the truth of the often-repeated statement that coloured children at first learn as quickly as white children, but that they afterwards fall off in progress. If it could be proved that education acts not only on the individual, but by transmission on the race, this would be a great encouragement to all working on this all-important subject. It is well known that children sometimes exhibit at a very early age strong special tastes, for which no cause can be assigned, although occasionally they may be accounted for by reversion to the taste or occupation of some progenitor; and it would be interesting to learn how far such early tastes are persistent and influence the future career of the individual. In some instances such tastes die away without apparently leaving any after effect; but it would be desirable to know how far this is commonly the case, as we should then know whether it were important to direct, as far as this is possible, the early tastes of our children. It may be more beneficial that a child should follow energetically some pursuit, of however trifling a nature, and thus acquire perseverance, than that he should be turned from it, because of no future advantage to him. I will mention one other small point of inquiry in relation to very young children, which may possibly prove important with respect to the origin of language, but it could be investigated only by persons possessing an accurate musical ear: children, even before they can articulate, express some of their feelings and desires by noises uttered in different notes. For instance, they make an interrogative noise, and others of assent and dissent in different tones, and it would, I think, be worth while to ascertain whether there is any uniformity in different children in the pitch of their voices under various frames of mind."

IN a letter to the *Madras Mail* of September 8 on the use of gigantic sea-weed as a protective agent for shores, Capt. J. H. Taylor, the Master-Superintendent of Madras, gives the following interesting "sea-serpent" story:—"A notable incident connected with this sea-weed, is recalled to my recollection, by Dr. Furnell's letter. About fifteen years ago, while I was in my ship at anchor in Table Bay, an enormous monster, as it appeared, was seen drifting, or advancing itself round Green Point, into the Harbour. It was more than one hundred feet in length, and moved with an undulating snake-like motion. Its head was crowned with what appeared to be long hair, and the keen-sighted among the affrighted observers declared they could see its eyes and distinguish its features. The military were called out, and a brisk fire poured into it at a distance of about five hundred yards. It was hit several times, and portions of it knocked off. So serious were its evident injuries, that on its rounding the point it became quite still, and boats went off to examine it and complete its destruction. It was found to be a specimen of the sea-weed above mentioned, and its stillness after the grievous injuries inflicted was due to its having left the ground swell and entered the quiet waters of the Bay."

DR. B. W. RICHARDSON is about to continue the series of lectures delivered by him in the spring at the instance of the "Ladies' Sanitary Association," of Berners Street. The lectures are devoted generally to the subject of "Domestic Sanitation." In the forthcoming series, which will be commenced in the Lower Hall, Exeter Hall, on Saturday, the 22nd inst., the structure and functions of the nervous system, and the physical and mental training of the young, will occupy a prominent place.

THE Phylloxera Congress, to which we have already referred, was opened on Sunday at Bordeaux.

THE Rev. J. Hoskyns-Abrahall writes to the *Times* from Combe, near Woodstock, October 3:—"On October 1, about 8.42 p.m., when I was walking in a north-westerly direction, about three hundred yards north-west of Hanborough Station, which is three-quarters of a mile north-west of Oxford, the eastern sky was suddenly flooded with a light that vied with that of the moon, which shone more than half full in the west. Turning round, I beheld a magnificent meteor, of a pale yellow hue, descending with a slow motion, vertically. It seemed larger than Jupiter. When I first saw it, it had dropped about a third of the distance from the zenith to the horizon; after traversing another third of that space it burst without scattering any sparks."

THE Exhibition of the Photographic Society is now open at the rooms of the Old Water-Colour Society in Pall Mall.

A NEW form of compressed air locomotive engine, the invention of a Mr. Hardie, has been put to a practical, and, it is said, successful test in New York, on the Second Avenue elevated railroad. The compressed air is stored in four tubular tanks connected with each other by pipes so as virtually to form one large reservoir. It is said that a saving of 50 per cent. is effected on the cost of working a locomotive by the use of the new invention.

A TELEGRAM from Geneva last Thursday states that another large rift has opened in the Tschingel, a circumstance which indicates that the mountain is still in movement. The inhabitants of Elm, many of whom had returned, have been again warned to leave their houses.

WE have received a very interesting coloured picture of the moon, reproduced from a telescopic painting by Mr. Henry Harrison of New York. It is the first of a series, and represents the moon at the stage of the three days crescent. The picture is twenty-four inches square, with the moon eighteen inches in diameter, and the execution is excellent. It shows the earth-shine very distinctly on the surface in shadow. As to its accuracy, we notice from a letter by Prof. Harkness that it was tested at the United States Naval Observatory, and the result is stated to have been all that could be desired. This picture is to be followed up by five others representing the moon at various succeeding stages. The London agent for the picture is Mr. William Wesley.

THE June number of the *Journal* of the Straits Branch of the Royal Asiatic Society, published half-yearly (London: Trübner and Co.) contains several useful articles:—Some account of the mining districts of Lower Pérak, by J. Errington de la Croix; The Folklore of the Malays, by W. E. Maxwell; Notes on the Rainfall of Singapore, by J. J. L. Wheatley; Journal of a voyage through the Straits of Malacca on an expedition to the Molucca Islands, by Capt. Walter Caulfield Lennox; a sketch of the career of James Richardson Logan, by J. Turnbull Thomas; and a memorandum on the various tribes inhabiting Penang and Province Wellesley, by J. R. Logan. A journal with such a programme deserves every encouragement, and we hope it will receive it.

THE Society for Promoting Christian Knowledge has issued a series of coloured zoological diagrams representing various typical specimens of animal life, from corals and anemones to mammals. They are accurately and nicely executed after Leutemann's Zoological Atlas for Schools. Why should we still have to go to Germany for such productions?

UNDER the title of "Anglo-Saxon Britain" Mr. Grant Allen has published (through the S.P.C.K.) an interesting little volume on the early history of England. He has taken pains to master all the results of recent research in archæology and ethnology, and therefore the book has a more scientific flavour than usual with such works. While adopting generally the views associated with the names of Freeman and Green, he shows independence of view, and treats his subject in an unusually unconventional manner. Either as a reading-book or as a text-book for the special period, it ought to be useful; it is certainly interesting.

IN "Miscellanies of Animal Life," by Elizabeth Spooner (S.P.C.K.), the authoress has brought together a number of interesting and instructive extracts from various good authorities as to the habits of animals, which ought to prove interesting to children.

THE double balloon ascent which we announced in our last number took place at La Villette gasworks on Wednesday last week, at the appointed time. The weather was splendid, and the two balloons were in view for some length of time; but the noise produced by the crackling of the net and the swinging of the aërostat produced such an effect on the sculler that he desisted from his experiment, and contented himself after a few pulls with an ordinary ascent. The experiment will be tried shortly with more experienced aëronauts.

ON October 18 a great electrical experiment will be made at the Paris Opera to test the effect of electric light on theatrical representations. The principal feature will be the lighting of a large number of Brush lamps by a magneto-electric machine revolving in the Palais de l'Industrie.

THE Ashton-under-Lyne Linnæan Botanical Society held its annual meeting on Sunday, October 2. Its members belong almost exclusively to the artisan class, and they are doing very good work. Under the auspices of the Ashton Biological Society they have undertaken the preparation of a complete flora and fauna of the district. The annual report gives particulars of the winter meetings and summer rambles of the members. It is a remarkable and interesting fact that the science of botany has been steadily and successfully cultivated by the Lancashire artisans for a century, if not longer, and their meetings, which are numerous, are held upon the Sundays.

ALL our lady readers are familiar with the name of Pullar of Perth, whose practical application of science to dyeing seems to meet with general favour. The present representative of that firm, Mr. Robert Pullar, is evidently conscious of how much he owes to science, and has recently been endeavouring to make her some return. The name of the Perthshire Natural History Society is no doubt known to many of our readers; its present president is Dr. James Geikie. At a recent meeting of the Society Mr. Pullar handed over to the Society a handsome and commodious house for their use, with accommodation for a museum, &c., Mr. Pullar himself having been the principal subscriber to the fund. The building will be known as the Moncrieffe Memorial Museum, in memory of the late president of the Society, Sir Thomas Moncrieffe. We trust, under the favourable conditions in which it now finds itself, the Perthshire Natural History Society will do even better work than it has hitherto done, and that its museum will become a model of what a local museum ought to be.

MR. J. HARRIS STONE, M.A., will contribute to the November number of *Good Words* an article upon the Viking Ship which was recently discovered in Norway. The paper will be illustrated with woodcuts made from photographs taken by the author.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mr. Frank Smyth; two Beautiful Parrakeets (*Psephotus pulcherrimus*), an Australian Quail (*Synœcus australis*), a Regent Bird (*Sericulus melinus*), three Modest Grass Finches (*Amadina modesta*) from Australia, two Banded Grass Finches (*Poephila cincta*), two Bicheno's Finches (*Estrelda bichenovii*) from Queensland, a Melodious Finch (*Phonipara canora*) from Cuba, a Blue-beaked Weaver Bird (*Spermospiza hæmatina*) from West Africa, a Black-headed Finch (*Munia malacca*) from India, two Ceylonese Hanging Parrakeets (*Loriculus asiaticus*) from Ceylon, presented by Mr. T. H. Bowyer Bower; two Dunlins (*Tringa cinclus*), a Ringed Plover (*Ægialitis hiaticula*), British, presented by Mr. Edmund A. S. Elliot, M.R.C.S.; a Common Viper (*Vipera berus*, var.), British, presented by Mr. L. A. Sandford; two Axolotls (*Siredon mexicanus*) from Mexico, presented by Dr. Heneage Gibbes, F.Z.S.; a Leopard Tortoise (*Testudo pardalis*) from South Africa, a Radiated Tortoise (*Testudo radiata*) from Madagascar, three Bell's Cinixys (*Cinixys belliana*) from Angola, presented by Sir John Kirk, C.M.Z.S.; a Hog Deer (*Cervus porcinus*), a Hybrid Mesopotamian Fallow

Deer (between *Cervus mesopotamicus* ♂, and *Cervus dama* ♀), a Hybrid Muntjac (between *Cervulus lacrymans* ♂, and *Cervulus muntjac* ♀), born in the Gardens.

GEOGRAPHICAL NOTES

THE U.S. steamer *Corwin*, which has been searching for the missing, and we fear lost, *Jeannette*, has succeeded in reaching Wrangel Land, which has been annexed to the United States. We learn that it is probable that an international effort will be made next year to find traces of the *Jeannette*; our own Government has been moved in the matter, and may very possibly fit out a vessel for the purpose.

THE French Geographical Society has received intelligence of the assassination of a young explorer, M. Henri Dufour, by a tribe of the Ovambos, now at war with the Portuguese. M. Dufour left Omoruru in company with some merchants in December last for the purpose of exploring the basin of the River Cumene, in Eastern Africa. On arriving at this river his companions deemed it expedient to abandon the enterprise, on which M. Dufour courageously resolved to continue his course alone. No tidings of him having reached Omoruru, an inquiry was instituted, which led to the discovery of his untimely end. M. Dufour's papers and effects have been found, but his body has not yet been recovered.

THE current number of the Geographical Society's *Proceedings* is chiefly remarkable for a very long instalment of the report of papers read at the Geographical Section of the British Association, including Sir J. Hooker's address, Sir R. Temple's paper on Asia, and Sir F. J. Evans' on maritime discovery. The paper of this month's number is one by Dr. Bell, of the Geological Survey of Canada, on the commercial importance of Hudson's Bay, with remarks on recent surveys and explorations, which is accompanied by a large and carefully drawn map of the region. The most important of the geographical notes are those respecting Mr. J. M. Schuver's journey in Africa and the proposition of the British Association that the Geographical Society should undertake a scientific expedition to Kilimanndjaro and Mount Kenia, with a subsidy of one hundred pounds. Another note records the presence of the first British traveller at Hami, but seemingly his name and plans are alike a mystery.

WITH reference to the recent census of India the *Pioneer* learns that the census returns show a grand total of population for all India of 252,000,000. Figures amounting to 218,000,000 can be compared with previous censuses, and show an increase of 6·2 per cent. But in some provinces apparent large increases may be due to the inaccuracy of previous enumerations. Provincial totals are—Bengal, 68,800,000; Assam, 4,800,000; Madras, 30,800,000; Bombay, 13,900,000; ditto Native States, 6,900,000; Sind, 2,400,000; North-West Provinces, 32,600,000; ditto Native States, 700,000; Oudh, 11,400,000; British Punjab, 18,700,000; Native ditto, 3,800,000; Central Provinces, 11,500,000; Berar, 2,600,000; British Burmah, 3,700,000; Mysore, 4,100,000; Rajpootana, 11,000,000; Central India, 9,200,000; Hyderabad, 9,100,000. The total makes males 123,000,000, females 118,000,000. The provincial increases per cent. as compared with previous censuses, are as follows:—Bengal, 10; Assam, 19; Sind, 10; North-West, 6; Oudh, 1; Punjab, 7; Central Provinces, 25; Berar, 20; Burmah, 35. The decreases are—Madras, 2·4 per cent.; Bombay, ·3; Mysore, 17.

A SOMEWHAT curious boat has been built and launched at Granton, N.B., for use by the Rev. T. J. Comber, of the Baptist expedition on the Congo. With a view to its being at once portable and durable, this boat has been made of canvas, coated with a mixture of lampblack and tar, and is stretched into shape by malacca canes, while the interior consists of three movable umbrella-shaped structures, which can be tightened at will; it has a partly-covered deck, and weighs only 60 lbs.; further, it can be easily taken to pieces, so as to be carried by two persons, and by a little arrangement will form a tent.

Petermann's Mittheilungen for October is filled up with two articles—one by Mr. W. H. Dall, on the hydrology of Behring Sea and neighbouring waters, and Hofrath A. Regel's account of his expedition to Turfan in 1879.

MESSRS. BLACKWOOD have issued a tenth edition of Page's "Introductory Text-Book of Physical Geography," revised and enlarged by Prof. Lapworth, of the Mason College, Birmingham.

CAPT. POPELIN, of the Belgian station at Karema, Lake Tanganyika, whose death was lately announced, appears to have died when on his way from Ujiji to the Mampara district, in Southern Uguha.

ON SOME APPLICATIONS OF ELECTRIC ENERGY TO HORTICULTURE AND AGRICULTURE[1]

ON the 1st of March, 1880, I communicated to the Royal Society a paper "On the Influence of Electric Light upon Vegetation, &c.," in which I arrived at the conclusion that electric light was capable of producing upon plants effects comparable to those of solar radiation; that chlorophyll was produced by it, and that bloom and fruit, rich in aroma and colour, could be developed by its aid. My experiments also went to prove that plants do not as a rule require a period of rest during the twenty-four hours of the day, but make increased and vigorous progress if subjected (in winter time) to solar light during the day and to electric light during the night.

During the whole of last winter I continued my experiments on an enlarged scale, and it is my present purpose to give a short account of these experiments, and of some further applications of electric energy to farming operations (including the pumping of water, the sawing of timber, and chaff and root-cutting) at various distances, not exceeding half a mile from the source of power, giving useful employment during the daytime to the power-producing machinery, and thus reducing indirectly the cost of the light during the night-time.

The arrangement consists of a high-pressure steam-engine of 6 horse-power nominal, supplied by Messrs. Tangye Brothers, which gives motion to two dynamo-machines (Siemens D), connected separately to two electric lamps, each capable of emitting a light of about 5000 candle-power. One of these lamps was placed inside a glass house of 2318 cubic feet capacity, and the other was suspended at a height of 12 to 14 feet over some sunk greenhouses. The waste steam of the engine was condensed in a heater, whence the greenhouses take their circulating supply of hot water, thus saving the fuel that would otherwise be required to heat the stoves.

The experiments were commenced on October 23, 1880, and were continued till May 7, 1881. The general plan of operation consisted in lighting the electric lights, at first at 6 o'clock, and during the short days at 5 o'clock every evening except Sunday, continuing their action until dawn.

The outside light was protected by a clear glass lantern, whilst the light inside the house was left naked in the earlier experiments, one of my objects being to ascertain the relative effect of the light under these two conditions. The inside light was placed at one side over the entrance into the house, in front of a metallic reflector, to save the rays that would otherwise be lost to the plants within the house.

The house was planted in the first place with peas, French beans, wheat, barley, and oats, as well as with cauliflowers, strawberries, raspberries, peaches, tomatoes, vines, and a variety of flowering plants, including roses, rhododendrons, and azaleas. All these plants being of a comparatively hardy character, the temperature in this house was maintained as nearly as possible a 60° Fahr.

The early effects observed were anything but satisfactory While under the influence of the light suspended in the open ai over the sunk houses the beneficial effects due to the electric light, observed during the previous winter, repeated themselves, the plants in the house with the naked electric light soon manifested a withered appearance. Was this result the effect of the naked light, or was it the effect of the chemical products—nitrogenous compounds and carbonic acid—which are produced in the electric arc?

Proceeding on the first named assumption, and with a view of softening the ray of the electric arc, small jets of steam were introduced into the house through tubes, drawing in atmospheric air with the steam, and producing the effect of clouds interposing themselves in an irregular fashion between the light and the plants. This treatment was decidedly beneficial to the plants, although care had to be taken not to increase the amount of moisture thus intro-

[1] Paper read at the British Association by C. William Siemens, D.C.L., LL.D., F.R.S., M. Inst. C.E.

duced beyond certain limits. As regards the chemical products it was thought that these would prove rather beneficial than otherwise, in furnishing the very ingredients upon which plant-life depends, and further that the constant supply of pure carbonic acid resulting from the gradual combustion of the carbon electrodes might render a diminution in the supply of fresh air possible, and thus lead to economy of fuel. The plants did not, however, take kindly to these innovations in their mode of life, and it was found necessary to put a lantern of clear glass round the light, for the double purpose of discharging the chemical products of the arc, and of interposing an effectual screen between the arc and the plants under its influence.

The effect of interposing a mere thin sheet of clear glass between the plants and the source of electric light was most striking. On placing such a sheet of clear glass so as to intercept the rays of the electric light from a portion only of a plant, for instance a tomato plant, it was observed that in the course of a single night the line of demarcation was most distinctly shown upon the leaves. The portion of the plant under the direct influence of the naked electric light, though at a distance from it of nine to ten feet, was distinctly shrivelled, whereas that portion under cover of the clear glass continued to show a healthy appearance, and this line of demarcation was distinctly visible on individual leaves. Not only the leaves, but the young stems of the plants soon showed signs of destruction when exposed to the naked electric light, and these destructive influences were perceptible, though in a less marked degree, at a distance of twenty feet from the source of light. A question here presents itself that can hardly fail to excite the interest of the physiological botanist. The clear glass does not apparently intercept any of the luminous rays, which cannot therefore be the cause of the destructive action. Prof. Stokes showed, however, in 1853, that the electric arc is particularly rich in highly refrangible invisible rays, and that these are largely absorbed in their passage through clear glass; it therefore appears reasonable to suppose that it is those highly refrangible rays beyond the visible spectrum that work destruction on vegetable cells, thus contrasting with the luminous rays of less refrangibility, which, on the contrary, stimulate their organic action.

Being desirous to follow up this inquiry a little further, I sowed a portion of the ground in the experimental conservatory with mustard and other quick-growing seeds, and divided the field into equal radial portions by means of a framework, excluding diffused light, but admitting light at equal distances from the electric arc. The first section was under the action of the naked light, the second was covered with a pane of clear glass, the third with yellow glass, the fourth with red, and the fifth with blue glass. The relative progress of the plants was noted from day to day, and the differences of effect upon the development of the plants was sufficiently striking to justify the following conclusions:—Under the clear glass the largest amount of and most vigorous growth was induced; the yellow glass came next in order, but the plants, though nearly equal in size, were greatly inferior in colour and thickness of stem to those under the clear glass; the red glass gives rise to lanky growth and yellowish leaf, while the blue glass produces still more lanky growth and sickly leaf. The uncovered compartment showed a stunted growth with a very dark and partly shrivelled leaf. It should be observed that the electric light was kept on from five p.m. till six a.m. every night except Sundays during the experiment, which took place in January, 1881, but that diffused daylight was not excluded during the intervals; also that circulation of air through the dividing framework was provided for.

These results are confirmatory of those obtained by Dr. J. W. Draper [1] in his valuable researches on plant cultivation in the solar spectrum in 1843, which led him to the conclusion, in opposition to the then prevailing opinion, that the yellow ray, and not the violet ray, was most efficacious in promoting the decomposition of carbonic acid in the vegetable cell.

Having, in consequence of these preliminary inquiries, determined to surround the electric arc with a clear glass lantern, more satisfactory results were soon observable. Thus, peas which had been sown at the end of October produced a harvest of ripe fruit on February 16, under the influence, with the exception of Sunday nights, of continuous light. Raspberry stalks put into the house on December 16 produced ripe fruit on March 1, and strawberry plants put in about the same time produced ripe fruit of excellent flavour and colour on February 14. Vines which broke on December 26 produced ripe grapes of stronger flavour than usual on March 10. Wheat, barley, and oats shot up with extraordinary rapidity under the influence of continuous light, but did not arrive at maturity; their growth, having been too rapid for their strength, caused them to fall to the ground, after having attained the height of about twelve inches.

Seeds of wheat, barley, and oats, planted in the open air and grown under the influence of the external electric light, produced, however, more satisfactory results; having been sown in rows on January 6, they germinated with difficulty, on account of frost and snow on the ground, but developed rapidly when milder weather set in, and showed ripe grain by the end of June, having been aided in their growth by the electric light until the beginning of May.

Doubts have been expressed by some botanists whether plants grown and brought to maturity under the influence of continuous light would produce fruit capable of reproduction, and in order to test this question the peas gathered on February 16 from the plants which had been grown under almost continuous light action were replanted on February 18. They vegetated in a few days, showing every appearance of healthy growth.

Further evidence on the same question will be obtained by Dr. Gilbert, F.R.S., who has undertaken to experiment upon the wheat, barley, and oats grown as above stated; but still more evidence will probably be required before all doubt on the subject can be allayed.

I am aware that the great weight of the opinion of Dr. Darwin goes in favour of the view that many plants, if not all of them, require diurnal rest for their normal development. In his great work on "The Movements of Plants" he deals in reality with plant life, as it exists under the alternating influence of solar light and darkness; he investigates with astonishing precision and minuteness their natural movements of circumnutation and nightly or nyctitropic action, but does not extend his inquiries to the conditions resulting from continuous light. He clearly proves that nyctitropic action is instituted to protect the delicate leaf-cells of plants from refrigeration by radiation into space, but it does not follow, I would submit, that this protecting power involves the necessity of the hurtful influence. May it not rather be inferred from Dr. Darwin's investigations that the absence of light during night-time involved a difficulty to plant life that had to be met by special motor organs, which latter would perhaps be gradually dispensed with by plants if exposed to continual light for some years or generations?

It is with great diffidence, and without wishing to generalise, that I feel bound to state as the result of all my experiments, extending now over two winters, that although periodic darkness evidently favours growth in the sense of elongating the stalks of plants, the continuous stimulus of light appears favourable for healthy development at a greatly accelerated pace, through all the stages of the annual life of the plant, from the early leaf to the ripened fruit. The latter is superior in size, in aroma, and in colour to that produced by alternating light, and the resulting seeds are not at any rate devoid of regerminating power.

Further experiments are necessary, I am aware, before it would be safe to generalise, nor does this question of diurnal rest in any way bear upon that of annual or winter rest, which probably most plants, that are not so-called annuals, do require.

The beneficial influence of the electric light has been very manifest upon a banana palm, which at two periods of its existence, viz., during its early growth and at the time of the fruit development, was placed (in February and March of 1880 and 1881) under the night action of one of the electric lights, set behind glass at a distance not exceeding two yards from the plant; the result was a bunch of fruit weighing 75 pounds, each banana being of unusual size, and pronounced by competent judges to be unsurpassed in flavour. Melons also, remarkable for size and aromatic flavour, have been produced under the influence of continuous light in the early spring of 1880 and 1881, and I am confident that still better results may be realised when the best conditions of temperature and of proximity to the electric light have been thoroughly investigated.

My object hitherto has rather been to ascertain the general conditions necessary to promote growth by the aid of electric light than the production of quantitative results; but I am disposed to think that the time is not far distant when the electric light will be found a valuable adjunct to the means at the disposal of the horticulturist, in making him really indepen-

[1] See "Scientific Memoirs" by J. W. Draper, M.D., LL.D. Memoir X.

dent of climate and season, and furnishing him with a power of producing new varieties.

Before electro-horticulture can be entertained as a practical process it would be necessary however to prove its cost, and my experiments of last winter were in part directed towards that object. Where water-power is available, the electric light can be produced at an extremely moderate cost, comprising carbon electrodes, and wear and tear of and interest upon apparatus and machinery employed, which experience elsewhere has already shown to amount to 6*d.* per hour for a light of 5000 candles. The personal current attention requisite in that case consists simply in replacing the carbon electrodes every six or eight hours, which can be done without appreciable expense by the under-gardener in charge of the fires of the greenhouses.

In my case no natural source of power was available, and a steam-engine had to be resorted to. The engine of 6 normal horse-power which I employ to work the two electric lights of 5000 candle-power each, consumes 56 lbs. of coal per hour (the engine being of the ordinary high-pressure type), which, taken at 20*s.* a ton, would amount to 6*d.* or to 3*d.* per light of 5000 candles. But against this expenditure has to be placed the saving of fuel effected in suppressing the stoves for heating the greenhouses, the amount of which I have not been able to ascertain accurately, but it may safely be taken at two-thirds of the cost of coal for the engine, thus reducing the cost of the fuel per light to 1*d.* per hour; the total cost per light of 5000 candles will thus amount to 6*d.* + 1*d.* = 7*d.* per hour.

This calculation would hold good if the electric light and engine power were required during say twelve hours *per diem*, but inasmuch as the light is not required during the daytime, and the firing of the boiler has nevertheless to be kept up in order to supply heat to the greenhouses, it appears that during the daytime an amount of motive-power is lost equal to that employed during the night.

In order to utilise this power I have devised means of working the dynamo-machine also during the daytime, and of transmitting the electric energy thus produced by means of wires to different points of the farm, where such operations as chaff-cutting, swede-slicing, timber-sawing, and water-pumping have to be performed.

These objects are accomplished by means of small dynamo-machines placed at the points where power is required for these various purposes, and which are in metallic connection with the current-generating dynamo-machine near the engine. The connecting wires employed consist each of a naked strand of copper wire supported on wooden poles or on trees without the use of insulators, whilst the return-circuit is effected through the park-railing or wire fencing of the place, which is connected with both transmitting and working machines by means of short pieces of connecting wire. In order to insure the metallic continuity of the wire fencing, care has to be taken wherever there are gates to solder a piece of wire, buried below the gate, to the wire fencing on either side.

As regards pumping the water, a 3-horse-power steam-engine was originally used, working two force-pumps of 3½-inch diameter, making thirty-six double strokes per minute. The same pumps are still employed, being now worked by a dynamo-machine weighing 4 cwt. When the cisterns at the house, the gardens, and the farm require filling, the pumps are started by simply turning the commutator at the engine station, and in like manner the mechanical operations of the farm already referred to are accomplished by one and the same prime mover.

It would be difficult in this instance to state accurately the percentage of power actually received at the distant station, but in trying the same machines under similar circumstances of resistance with the aid of dynamometers, as much as 60 per cent. has been realised.

In conclusion, I have pleasure to state that the working of the electric light and transmission of power for the various operations just named are entirely under the charge of my head-gardener, Mr. Buchanan, assisted by the ordinary staff of under-gardeners and field-labourers, who probably before never heard of the power of electricity.

Electric transmission of power may eventually be applied also to thrashing, reaping, and ploughing. These objects are at the present time accomplished to a large extent by means of portable steam-engines, a class of engine which has attained a high degree of perfection; but the electric motor presents the great advantage of lightness, its weight per horse-power being only 2 cwt., whilst the weight of a portable engine with its boiler filled with water may be taken at 15 cwt. per horse-power. Moreover, the portable engine requires a continuous supply of water and fuel, and involves skilled labour in the field, whilst the electrical engine receives its food through the wire (or a light rail upon which it may be made to move about) from the central station, where power can be produced at a cheaper rate of expenditure for fuel and labour than in the field. The use of secondary batteries may also be resorted to with advantage to store electrical energy when it cannot be utilised.

In thus accomplishing the work of a farm from a central-power station, considerable savings of plant and labour may be effected; the engine-power will be chiefly required for day work, and its night work for the purposes of electro-horticulture will be a secondary utilisation of the establishment, involving little extra expense. At the same time the means are provided of lighting the hall and shrubberies in the most perfect manner, and of producing effects in landscape gardening that are strikingly beautiful.

THE ELECTRICAL DISCHARGE, ITS FORMS AND ITS FUNCTIONS[1]

II.

AMONG the various circumstances which combine to determine the character of the discharge, one of the most important is the size of the negative terminal. And in this respect, as well as in others, the negative differs fundamentally from the positive. If the negative be small, not so much in comparison with the positive as in absolute magnitude, and perhaps also in reference to the diameter of the tube, the tube will offer great "resistance," as it is termed, to the passage of the discharge. On the other hand, if the negative be large, the discharge passes with comparative ease. In the first case, even when the discharge passes, striæ are formed only with difficulty, if at all; in the second they are readily formed. This may easily be shown by using a tube with one small and one large terminal, which can be used alternately as positive and as negative; or by a tube having a negative terminal of variable length.

The same dependence of striation upon the size of the negative may be shown in the case of a tube with a negative terminal of barely sufficient size. In this case, if the tube be touched by the hand (an operation which, as will be hereafter explained, is equivalent to enlarging the negative), striæ will be brought out clear and distinct, while without this assistance they appear only in a confused and irregular manner.

Other characteristic features of the negative terminal would deserve our attention if time permitted. Thus, the well-known phenomena of the so-called "Holtz tube" (or tube divided into compartments by diaphragms furnished with narrow pipes leading from one compartment to the next, and all pointed in one direction), show that a small aperture will serve as a negative, but not as a positive terminal. This property has been generalised by Goldstein, who, using as a negative terminal a cylinder of non-conducting substance pierced with fine holes, reproduces all the phenomena appertaining to an ordinary metallic negative.

And, even apart from the phenomena of vacuum tubes, it would not be difficult to adduce instances showing the importance of the size of the negative terminal in electrical discharges generally. Of these I will now mention only the latest. In making some modifications of Planté's battery M. de Pozzer has found that, if the negative electrode be made of a plate of lead of half a millimetre in thickness, and the positive of one of two-thirds of a millimetre, but the former double the size of the latter, great advantage arises from the greater size of the negative. The discharge from a battery having a negative double as large as the positive lasted, on an average of several experiments, for an hour; while that from a battery, in which the sizes of the electrodes were reversed, lasted only half an hour. The effect of a battery with electrodes of equal size appears to have been intermediate to that of the two others.

From these phenomena, and especially from those of the moving terminal, as well as from other considerations, it appears that the general configuration of the discharge is mainly determined at the negative terminal.

In order, however, to experiment with any hope of progress

[1] A Lecture delivered before the British Association at York on September 5, 1881, by William Spottiswoode, D.C.L., LL.D., President of the Royal Society. Continued from p. 551.

in our knowledge of the nature and *modus operandi* of the discharge, we ought to be in a position to modify the discharge, so as to compare it under different circumstances. The methods hitherto usually employed for this object have been an alteration in the gas used, an alteration of the pressure, and a diversity of figure in the tube or in its terminals. Of the general character of the changes due to such alterations we have already seen something. But, besides these alterations, which are of a structural or instrumental character, it is also desirable to operate on a discharge actually *in transitu*. One of the methods, in fact the only one, employed until lately is that of the magnet, which was used as long ago as the time of Grove, Plücker, and other early experimenters. It is well known that a magnet will displace a movable conductor when carrying a current, according to laws established by Ampère. The same is true, in general terms, with respect to a discharge traversing an exhausted tube.

Thus, in the tube now before you, you will see that when one pole of a magnet is presented to the tube, the discharge is thrown to one side of the tube; and when the other pole is so presented the discharge is thrown to the other side. These two main features, however, very inadequately describe the actions of a magnet, which in fact operates separately not only upon each stria as a unit, but even upon the various parts of a stria in such a way as to deform it as well as to displace it. But they involve the main characteristics of the magnetic action, and must suffice on the present occasion to show that in the magnet we have a powerful instrument for examining the properties and functions of the discharge, even (if the term may be permitted) in the living specimen.

The other principal mode of operating on the discharge consists in reducing it to what has been called the "sensitive state"; *i.e.* to a state in which the position of the luminosity is affected by the approach of a conductor to the tube. For the details of an experimental Investigation into the phenomena of this state, and a discussion of the conclusions that may be drawn therefrom, the reader is referred to the *Philosophical Transactions* of 1879 and 1880. But the following remarks may serve to convey some notion of the method and its issues. Sensitiveness is produced by breaking the circuit with a short interval of air, or, as it is usually described, by interposing an "air-spark" in one branch of the circuit, viz. either that leading from the positive, or in that leading from the negative, terminal of the machine to the tube, or by otherwise rendering the discharge intermittent. The effect of this is to discharge the electricity discontinuously, so that from time to time there passes into the tube a comparatively large quantity of electricity at a higher tension than would otherwise be the case. By this means the gas in the interior of the tube, or perhaps the interior surface of the tube itself, becomes momentarily charged with electricity, thus creating an electric tension, which may be discharged or "relieved" by a displacement in the electricity on a conductor brought near, or in contact with, the tube. This causes or permits a discharge from the interior of the tube itself of the electricity of an opposite kind to that with which the tube itself is charged. If the air spark is on the positive side the charge on the tube is positive, and the relief negative, and *vice versâ*. From this it follows, as might have been expected, that the effect of the relief on the visible discharge is different in the two cases. In each case the part of the inner surface of the tube nearest to the conductor acts as a *quasi* terminal. As a general rule, with a positive air-spark, the relief, being negative, tends to produce a dark space, and thereby gives the appearance of a repulsion of the luminous column. With a negative air-spark the relief, being positive, tends to produce a stria; it thereby causes luminosity, and gives an appearance of attraction of the luminous column.

The appearance of the luminous column when produced under the action of an air-spark is usually amorphous or unstratified, although this is not always the case. In the case of a positive air-spark the column is more or less constricted and confined to the central part of the tube; in the case of a negative air-spark it is more diffused, and usually fills the whole diameter of the tube. This is doubtless due to a gradual discharge from the sides of the tube, and is in accordance with what has been said above.

Under suitable circumstances this relief discharge may be made to bring out artificial striæ from an amorphous discharge, the position of the striæ depending upon the character of the air-spark used. The positions occupied by striæ in the one case will be occupied by dark spaces in the other, and *vice versâ*.

The facts here adduced, together with many others based upon a long series of experiments, all tend to the conclusion that, whatever the number or form of the striæ in a stratified column, each stria is to be regarded as a physical unit, and that in each unit we have represented all the elements of a complete discharge. The form of each stria is in every case determined by that of its immediate predecessor, reckoned from the negative end. This may be verified, among other ways, by observing the form of the successive striæ when distorted by the influence of a magnetic field. The same research has further established the fact that the negative glow and the haze behind it, which terminates in what is usually known as the negative dark space, is a stria turned as it were inside out by the influence, the shape, and the character of the negative terminal. From the mode in which this stria is connected with the negative terminal it has been called the "anchored stria."

The relief effects, may, however, be produced equally well by connecting a point on the surface of the tube with the opposite, or non-air-spark terminal, instead of with earth. By this means we supply a charge of electricity of the opposite name to that with which the tube has been charged, and obtain a result of a similar character to that of ordinary relief.

Alongside of the relief effects above mentioned there is also a system of what we have termed "special effects," which latter are converse to the former, each to each. These are produced by connecting a point on the outside of the tube with the air-spark terminal itself. The special effect with a positive air-spark is equivalent to a relief effect with a negative air-spark, and *vice versâ*.

Lastly, all these effects may be produced by means of impulsive discharges to the outside of the tube from an independent source of electricity, such as a second Holtz machine. And, *mutatis mutandis*, the corresponding effects may be produced by this method even on a non-sensitive discharge. This completes the entire cycle of phenomena due to impulsive action *ab extra*.

The character of these effects being known once for all, this impulsive action may be used as a test of the nature of a discharge (*i.e.* whether positive or negative) passing through a given tube. For example, we may experimentally verify in the case of a coil discharge what might have been anticipated on the principles now established. Such a discharge is in fact equally intermittent from both ends. There is no reason why either terminal should be regarded as the air-spark terminal rather than the other. Hence we might expect that the discharge would be positive through about one-half of the tube, and negative through the remainder, with a neutral zone between them. And such proves to be the case. But more than this, if we attach a small condenser to either terminal of the tube, so as to tone down the impulsiveness of the discharge at that end, we can thereby alter the proportions of the positive and the negative parts of the discharge and shift the position of the neutral zone at will.

The distinctive character which it is thus possible to convey to the whole, or to the two parts of one and the same discharge, naturally leads us to examine whether it be not possible entirely to separate one from the other, and to produce what may be called a unipolar discharge. And this in fact may be done; by connecting the one terminal of the tube through an air-spark to one branch of the circuit, and by leaving the other disconnected, we may produce a discharge which, having plunged blindly into the tube, and finding no response from the other end, returns upon itself, and finds exit by the way by which it came. The unipolar discharge is essentially intermittent, and therefore sensitive; the positive is conical in form and tapering towards its end; the negative is broad, and, so far as it extends, it fills the entire width of the tube. Lastly, two unipolar discharges of the same name can be produced in the same tube; they repel one another, and each returns like a single one.

From these experiments we conclude that the independence of the discharge from each terminal is so complete that we can at will cause discharges from the two terminals to be equal in intensity but opposite in sign (as in the case of the coil), or of any degree of inequality (as in the case of the coil with a small condenser). Or we can cause the discharge to be from one terminal only, the other terminal acting merely receptively (as in the case of the air-spark discharge); or we can cause the discharge to pass from one terminal only and return to it, the other terminal not taking any part in the discharge; or, finally, we can make the two terminals pour forth independent discharges of the same name, each of which passes back through the terminal whence it came.

One of the most important consequences which follows from

these considerations is that the discharges at the two terminals of the tube are so far independent as to be primarily determined each by the conditions at its own terminal, and only in a secondary degree, if at all, by the conditions that exist at the opposite terminal. And since the discharges are not necessarily the same at both terminals, the tube must contain free charges at different times. A tube is therefore not like a conductor, but is an independent electrical system, holding much the same position as the air-vessel in a forcing-pump. All the electricity that goes into it goes out again, but this is true only when we consider the whole discharge from the beginning to the end, and it may not be true even approximately during a small finite time. This independence of the discharges from the two terminals in the passage of electricity through rarefied gases dissipates the error of seeking analogies in metallic conduction; and shows that any obedience to regular laws as to change of potential as we proceed along the tube, resistance, &c., must arise from the fact that the effects measured are really average effects over an interval of time very long compared with the duration of the individual discharges.

The importance which attaches to the negative end of the discharge has led experimenters to examine whether the features appertaining to it could not be still further enlarged. And the only thing requisite to carry the experiments to a limit was an instrumental method of improving the vacuum to the degree required. This was furnished by Mr. Crookes' refinements on the Sprengel pump. In a series of most remarkable experiments he has shown, as mentioned above, not only that the striated column may be reduced to zero, but that the anchored stria itself may be so driven back that the blank space in question may be made to occupy the entire length of the tube.

When an exhaustion such as that described, or an exhaustion nearly equal to it, has been reached, a phenomenon, previously noticed, but not before made the subject of serious inquiry, presents itself. Certain parts of the interior surface of the tube become luminous with phosphorescent light. The colour of this light depends on the nature of the glass, and not in any way on the nature of the residual gas within the tube, nor on the substance of which the terminal is made. With German glass the phosphorescent light is green, with English glass it is blue. But the portion of the glass thus rendered luminous depends upon the form and position of the negative terminal. This phenomenon is supposed to be due to the streams of gaseous particles shot off from the neighbourhood of the negative terminal during the discharge. Although there is reason to think that these streams are an accompaniment rather than an integral part of the discharge, yet the particles would seem to be themselves charged with electricity, inasmuch as their paths are affected by a magnet, just as is a movable conductor carrying a current, or a charged body in rapid motion.

The whole subject of these streams, their power of heating metals and other substances, the shadows cast by bodies interposed in their path, and other properties of them, have been so well and so fully illustrated by Mr. Crookes both in published memoirs and in a lecture before this Association, that it is unnecessary for me now to dwell upon the subject in detail.

Their nature and properties, however, having been thus in the main determined, these streams have proved a valuable auxiliary in an investigation of what have been called the "small time-quantities" involved in the discharge. The discharge is, as has been already shown, a complex phenomenon, the various parts of which, although not entirely separable, may be shown to occupy different periods of time; and the length of these periods may be compared with one another, and with other known electrical phenomena. We cannot, it is true, make any absolute determinations of the time occupied in any of them, but we may still form a table of relative magnitudes of these small time-quantities. And I will now endeavour in a few words to give some idea of the nature of these quantities, and of the method whereby they have been measured.

If we take a tube of such high exhaustion as to cause the discharge to become intermittent, or if we use a positive air-spark of sufficient length with a tube of fair exhaustion, phosphorescent light, due to molecular streams, will be seen on the inner surface of the glass near the negative terminal. If then a patch of tinfoil, connected with earth, be placed on any other part of the tube, it will cause negative relief discharges to take place from the glass immediately within it, producing phosphorescence on the opposite side of the tube.

If any solid object, such as a piece of wire, should be present in the tube below the point of contact, it will cast a shadow on the phosphorescence, precisely as in Crookes' experiments with the streams from the negative terminal. If there be two points of relief contact, the same object will throw two shadows, in directions conformable with radiations from each. To these, other experiments might be added.

A determination of the precise directions in which these molecular streams issue from a relieving surface is not a very simple problem; and we must here content ourselves with showing that, in the case of intermittent discharges at least, the streams do not issue normally. If a strip of tinfoil placed along the tube be used as a relieving surface, the phosphorescence takes the form of a sheet wrapped round the tube; if the strip be wrapped round the tube, the phosphorescence takes the form of a sheet laid along the tube. If contact be made with the finger over a finite surface, or by a ring of wire laid close upon the tube, the phosphorescence takes the form, approximately, of the evolute of an ellipse. In all these cases the illumination is somewhat irregular; but the geometrical elements of which the phosphorescent figure is composed, and the stripes or striations of more intense light, are always formed at right angles to the longer dimension of the contact piece. This being so, suppose that we place on the tube a strip in such a curve that the normal planes to the curve will pass through the tangent at the corresponding point of the image of the curve, *i.e.* the curve on the opposite side of the tube, each point of which is exactly opposite to a point on the tinfoil. In such a case all the striations will lie along the curve formed by the locus of the central patches of phosphorescence, and the result will be a single bright curved line of phosphorescence without any spreading out or striated margin. The curve fulfilling these conditions will be a helix, whose pitch is half a right angle. Experiment confirms the anticipation.

One more step in the study of these molecular streams is necessary for our present purpose, namely, an application to them of the same method which we have used with the electrical discharges themselves; viz. we must examine the effect of an inductive stream produced *ab extra* upon a direct stream due to the discharge inside the tube. These effects may be described generally as the interference of molecular streams.

If the finger be placed upon a highly exhausted tube through which a discharge with a positive air-spark is passing, the phosphorescence due to the molecular streams from the negative terminal is seen to fade away from the place where the finger rests, and from a region lying thence in the direction of the positive terminal. The effect is that of a shadow over that part of the tube; and as this is produced not by any real intervening object, but by an action from outside, we have termed it a "virtual shadow." The phenomenon is due to a beating down of the streams of molecules coming from the negative terminal, by the transverse streams from the side of the tube immediately within the part touched.

The interference of two molecular streams may be further illustrated by a variety of experiments, and in particular by arranging within the tube a conductor of some recognisable form —say skeleton tetrahedron. If the tube be touched at a place opposite to this object a shadow of the latter will be formed in the relief phosphorescence; but if the tube be touched also at a point on which the conductor rests, the shadow will be splayed out in a striking manner. This splaying or bulging of the shadow is due to the interference of the molecular streams issuing from the surface of the conductor, which then acts as a *quasi* negative terminal, with the original relief streams issuing from the first point of contact.

With the help of these properties we are able, by connecting a patch of tinfoil on the tube with earth, or with the negative terminal itself, or with a second patch elsewhere on the tube, to detect the presence or absence of a demand for negative electricity; to localise the main seat of such demand; and even to compare the electrical condition of different parts of the tube at the same time or of the same parts at different times during the very passage of the discharge. In this way we approach the question of the small time-quantities involved in the discharge.

And, in the first place, it must be understood that the whole duration of one of these intermittent discharges is comprised within a period of which the most rapidly revolving mirror has been incompetent to give any account. It may be in the recollection of a few of my audience that when the discharge from my great induction-coil was exhibited at the Royal Institution with tubes on a revolving disk the discharge showed a durational

character as long as the coil alone was used; but as soon as a Leyden jar was introduced, which was in the main equivalent to an air-spark in a continuous current, the durational character disappeared, and nothing was visible but a bright line, the width of which depended, not upon the duration of the discharge, for no velocity of rotation in any way affected it, but only on the width of the slit through which the discharge in the tube was seen. But, notwithstanding the extreme rapidity with which the discharge is effected, our experiments have already shown that the spark or discharge is a complicated phenomenon, the various parts of which take place in a certain order or sequence of time; and that in virtue of this sequence we have succeeded, at the various pressures comprised within our range, in affecting and modifying it *in transitu*. This suggested the idea that, although the subject is surrounded with difficulties, it might still be possible to form some relative estimate, at all events, of the time occupied by the various parts of which the whole phenomenon is composed. And in fulfilment of this the following are some of the conclusions to which we have been led.

The time occupied in the passage of electricity of either name along the tube is greater than that occupied in its passage along an equal length of wire.

This may be shown by connecting metallically a piece of tinfoil near the air-spark terminal with another near the distant terminal; for it is then seen that the former derives as much relief from the latter as if the latter were not on the tube. This shows (1) that at the time when the electric disturbance reached the nearer piece of tinfoil the more distant piece was unaffected, and (2) that the disturbance propagated along the wire reached the second piece before the arrival of the same disturbance propagated within the tube.

The negative discharge occupies a period greater than that required by the particles composing the molecular streams to traverse the length of the tube, but comparable with it.

Proofs of this proposition are to be found in the phenomena of virtual shadows, and in other instances of the interference of molecular streams; but, omitting detailed experiments, the general argument on which the above conclusion is based is as follows: If two molecular streams, one issuing with positive relief from the side of the tube, the other coming from the negative terminal, show signs of interference, it is clear that the former of these, which certainly started first, must have continued to flow, at all events, until the arrival of the latter.

The time occupied by the passage of electricity of either name along the tube is incomparably shorter than that occupied by the emission of the molecular streams, or (what is the same thing) the time occupied by the negative discharge.

In support of this conclusion we have time only to mention a single experiment. If two pieces of tinfoil connected by a wire be placed, one near the negative, the other near the positive end of a tube through which a negative discharge with a rather long air-spark is passing, the former will show relief (positive) effects, the latter special (negative) effects; but no phosphorescence will be caused at the latter, however long the air-spark used. When the second patch is lifted off the tube and placed upon another through which no current is passing, phosphorescence is immediately produced. The explanation of this appears to be as follows: The negative electricity, bursting into the tube, summons all the positive which it can draw from the tinfoil. This is answered so promptly, that the second patch gives up to the first through the medium of the wire all the positive that it can yield, or, which is the same thing, draws off from the first all the negative that it can obtain; and this is done before the advancing negative reaches the distant patch. But so rapidly does the negative advance, that it reaches the distant patch before the molecular streams have had time to flow from the latter in a sufficient stream to produce phosphorescence; and it reaches it in time to revoke the supply of positive to the nearer, and to draw back the supply of negative which would have come to, and with it the molecular streams which would otherwise have flowed from, the further patch. When the second patch is placed on an independent tube, where no such revocation is possible, phosphorescence actually appears, showing that the revocation is no mere supposition, but a real phenomenon.

From the last two laws it follows as a consequence that negative electricity, and therefore also electricity of either name, in the tube outruns the molecular streams.

We may now fairly ask whether the phenomena which we have been studying have any counterpart in the larger operations of nature which are going on around us, and whether the conclusions to which we have been led afford any explanation of observed facts? Many natural phenomena doubtless fundamentally depend upon electricity; how many we hardly yet know. But there are two in particular, namely, lightning and the aurora, which are unquestionably electrical, and whose correspondence with the spark proper, and with the discharge in rarefied gases, respectively has often been noticed. On these I venture to offer a few remarks.

To say that both of these phenomena are dependent on the electrical state of the atmosphere is not saying much; both for other reasons, and especially because we do not know upon what atmospheric electricity itself depends. But it is clear that it is to a knowledge of the distribution of such electricity that we must look for a proximate, as well as an approximate, explanation of the facts.

In a thunder-cloud we have an aggregation of aqueous particles small enough to remain, temporarily at least, suspended in the air. All of these, it would appear, are similarly electrified, and by their mutual repulsion are restrained from further coalescence. By their presence the ground below the cloud becomes inductively electrified in the opposite sense; and as soon as the cloud by its motion comes within sparking distance, or by an increase of its charge attains sufficient tension, a spark discharge takes place, which, as we have seen above, is a flash of lightning. A similar action may naturally take place between two clouds oppositely electrified. The electrical tension required for a flash of lightning is of course enormous. It has been calculated that in order to produce directly from a battery of the most favourable construction a spark of 42 inches, equal to that given from my great induction-coil, from 60,000 to 100,000 cells would be necessary; while for a flash of lightning a mile long not less than 3,500,000 cells would be required.

In some interesting experiments on water flowing from a small orifice in a cistern Lord Rayleigh has found that the breaking of the continuous column into drops is checked by communicating to it a small charge of electricity; but that it is promoted by a large charge. We may imagine with him that something of the same kind takes place in the cloud; and that before the flash the aqueous particles are kept apart by mutual repulsion due to their being all highly charged with electricity of the same name; but that after the flash they are left either without charge or with so slight a charge as to promote their coalescence and their consequent fall in the form of rain. This would be an explanation of the well known downpour which frequently occurs after a flash of lightning.

There is moreover another form of lightning to which the discharge in our vacuum-tubes offers, to say no more of it, considerable analogy, namely, that commonly known as ball lightning. The appearance of ball lightning is described as that of a luminosity or ball of fire moving generally towards the earth, in a direction more or less oblique, and disappearing in most cases before reaching the ground. In some tubes, the exhaustion of which is very moderate, say, having a pressure of several millimetres of mercury, it happens not only that the blocks of light termed entities by Mr. De La Rue are formed, but also that these entities travel along the tube from the immediate neighbourhood of the positive terminal to a finite distance in the direction of the negative, and then disappear. It would seem not unreasonable to suppose that ball lightning is due to conditions not dissimilar to those of such tubes, namely, to a discharge occurring in the upper regions of the air, at an elevation of perhaps twenty miles, more or less, where the pressure is moderate, that is to say, greater than that under which an auroral-like display could take place, and yet less than that which would give rise to a true spark or ordinary flash of lightning. And if further we effect the discharge in the tube by the gradual outpouring of electricity from a charged Leyden battery, or other condenser, through a suitable resistance, or if we use an induction-coil, then the condenser, or coil, will represent the charged cloud, or portion of the atmosphere, from which the phenomenon proceeds; and the analogy will perhaps be considered sufficiently close to render further observations in proof or disproof of the theory desirable.

Let us now turn to the aurora. Sufficient experiments have been made this evening to show that the discharge in rarefied gases differs from that in gases at higher pressures; and that the difference corresponds exactly to that observed between the diffused, gentle, and flickering play of the aurora and the sudden crashing spark of a flash of lightning. It is also abundantly clear that at an elevation of twenty or thirty miles above the

earth's surface the atmospheric rarefaction must be such as to convert what would be lightning at a lower leve into a discharge similar in the main to that in a vacuum tube.

Further, it is an ascertained fact that a difference of electrical condition in different portions of the atmosphere often prevails. We have, therefore, not unfrequently present in regious at moderate elevations, say from twenty to fifty miles, all the conditions necessary for the production of an auroral display.

And not only so, but our experiments enable us to determine, at all events approximately, some limits of elevation within which this phenomenon can occur, and thereby to check the very divergent estimates of those who have observed it. Estimates of the altitude at which the auroral discharge takes place have been made from simultaneous observations at different points, and these have ranged up to fifty or sixty, and even to 281 miles. But even the lowest of these appears to be improbable. The pressure at which the resistance of air is least is a little less than ·4 of a millimetre of mercury; and the corresponding elevation is about thirty-eight miles. A vacuum tube measured by hundred-thousandths of an atmosphere would correspond to an elevation of a little more than eighty-one miles. Through a hydrogen vacuum at this pressure Mr. De La Rue failed to obtain a discharge with 11,000 cells; and he adds that "it may be assumed that at this height the discharge would be considerably less brilliant than at thirty-eight miles, should such occur."

It seems to be a well-ascertained fact that in high latitudes there are fewer thunderstorms and more auroras than in lower latitudes. This fact points to the conclusion that, after a disturbance, the re-distribution of atmospheric electricity is effected by one process or by the other, according to, or rather in consequence of, the meteorological differences between arctic, temperate, and tropical regions. In colder regions, where the air is generally drier, and, consequently, a better insulator than in warmer, there is less liability to a discharge taking place in the lower and denser strata; that is, there is less liability to lightning. But at higher levels the rarefaction may compensate this, and cause an auroral discharge to take place instead.

There are other features in which a comparison may be made between the auroral light and vacuum discharge. These discharges, when free to arrange themselves in a magnetic field, follow the lines of force; the auroral streamers appear to run parallel to the dipping needle. The colour of such discharges varies with the exhaustion; that of the aurora varies, like that of an air-vacuum, from red almost to white; and in the absence of independent observations to the contrary, we may fairly attribute the variety of tint in the aurora partly to a diversity of elevation, and, consequently, of rarefaction in the region where it takes place, but partly also perhaps to the electrical conditions present anterior to the passage of the discharge.

These and other features of the phenomenon of the aurora, as well as the kindred subjects of earth currents, the disturbances of the magnetic needle, and the connection of the whole with solar radiation as a predisposing cause, have been brought together under one theoretic view by Prof. Stokes, to whom I am indebted for much of what I have here said on the subject.

Having thus gone through, so far as circumstances permitted, the experimental and inductive parts of my subject, it might have been very pleasant to have cast aside for a few moments the links which connect strict induction with what may be termed the fixed points of ascertained fact; and, restrained only by the more elastic bonds of scientific imagination, to have indulged in speculations about things still lying on the borders of science and of dreamland. But I must leave each of you to follow out this vein of thought after your own fashion; and, confining myself to a single remark, I will simply indicate the direction in which my own thoughts on the present subject are inclined to turn. The remark is this: If in the search for a solution of the mystery of electricity there be one element more deserving our attention rather than another, it is that of time. We have utilised this element in our experiments with the revolving mirror; and we have touched upon its more subtle influences in our conclusions about the small time quantities in relation to the discharge.

All operations of nature take place in time. It is in the time-sequence of phases, often apparently simultaneous, but in reality successive, that we may hope to strike the origin of many complicated phenomena. Time is the ocean beneath whose waves and whose currents the secret fountains of truth are to be sought. Time is the ocean whose mighty stream encircles our life. Time is the ocean whose "countless smiles" gave birth to Venus and the Nereids and all the infinite forms of beauty and of brightness which play around our youth. Time is the ocean from whence sprang also the steeds of Neptune, typical of the strength of our more mature years. Time is the ocean in whose loving arms we fall asleep, when the sun sinks low on the horizon, and the shades of night are creeping over the heavens, and all things tell us that our course is run.

BIOLOGY AS AN ACADEMICAL STUDY[1]

II.

IT may help to the understanding of what I mean by a sound method of biological teaching if I give a brief outline of the course of study I hope to pursue with my students this session. It is hardly necessary for me to state that this course is derived from Prof. Huxley's by a natural process of descent with modification.

In the first place there will be some four or five lectures on a common flowering plant, giving an account of its ordinary structure as seen by the naked eye, of its microscopic structure, of its physiology, and of the process of its development. After each lecture the students will examine for themselves the plant described, learning not only to dissect it in the ordinary way, but to make preparations for the microscope. By this means they will be familiarised with the use of the microscope, the employment of staining fluids, and other reagents used in the investigation of minute structure, and with the chief processes of manipulation. As the laboratory will be open for nine hours a week, so as to give three hours for working out what has been described in each hour's lecture, it is to be expected that a student of average intelligence will, by the time this part of the course is over, have a very fair notion of what a flowering plant is, of the processes by which its life is carried on, and of the manner in which it originates.

The next few lectures, and the corresponding portion of the practical course, will be occupied with a similar treatment of an animal: the one selected, as on the whole the most convenient and the most instructive, being the common sea crayfish of our markets. In the examination of this organism, the students will learn something of the art of dissection, and will further apply the knowledge of microscopic structure which the study of the plant has given them, to the far more difficult problems of animal histology. The study of the crayfish, and the comparison of it, point by point, with the plant, should give a clear conception of the main points of difference and of likeness between the more highly organised animals and plants—between animals and plants as they are generally known to us.

In dealing with these types in the lectures it will be my aim always to proceed from the known to the unknown; to begin with points which every one who has seen a flowering plant or a crayfish must have noticed, gradually leading up to such points of structure as require minute observation to verify them, and above all never to give a definition or a general statement without first supplying the facts from which it is legitimately deducible.

Next, I propose to take a number of types selected on the one hand from the lowest plants, on the other from the lowest animals: to show how these unicellular organisms agree in structure and in the nature of their physiological processes with the individual cells of which the bodies of the higher plants and animals are made up, and to point out how, in dealing with these lowest members of the two kingdoms of organic nature, the boundary line between the two kingdoms tends to disappear, and it becomes very difficult, sometimes even impossible, to say what is a plant and what an animal. The study of these lowly forms will also lead to the question of the origin of life, and it will be necessary to say something of the attempts which have been made to establish the doctrine of spontaneous generation, and to discuss their value.

The consideration of a few other animal and vegetable types, especially such as, although multicellular, exhibit none of the complex tissues found in the higher animals and plants, will bring the introductory part of the course to a close—the part which deals with the general facts and principles of biology. In it the student should learn how animals and plants agree with and differ from each other, and from inorganic bodies; what are the relations of animals and plants to one another, and to

[1] Inaugural Lecture delivered in the University Library, May 2, 1881, by T. Jeffery Parker, B.Sc., Lond., Professor of Biology in the University of Otago. Continued from p. 546.

inorganic nature; what is meant by differentiation of structure, and by the division of physiological labour exhibited by the higher organisms in contradistinction to the lower. He will also have gained some conception of the all-important truth that the higher organisms begin life as a simple cell, comparable to an entire unicellular organism, and that, of that cell, the animal or plant itself, as well as every element of its fully-formed tissues, is a lineal descendant. And these matters will be impressed upon his mind by actual verification of all the more important points; so that he will, it is hoped, have begun to learn the first duty of the student of science—to take things on trust only so long as he is unable to bring them to the test of observation and experiment.

The whole of this part of the course is a modification, adapted to local requirements, of Prof. Huxley's well-known "Genera Biology" course. It will be seen at once that it serves as an introduction both to botany and zoology, forming a starting-point from which lectures on both these subjects may diverge. I hope to give a few lectures on structural botany on Monday evenings, but the remainder of the ordinary biology course will be purely zoological, dealing chiefly with animal morphology, or comparative anatomy, as opposed to systematic zoology. It is gradually being acknowledged by those most competent to form an opinion, that zoology in this latter sense is a subject of no educational value whatever—I mean as far as the beginner is concerned—since it necessarily follows a course exactly the opposite of that which the scientific novice should pursue. It begins with generalisations, and ends with details; it provides elaborate systems of classification without giving even an elementary knowledge of the totality of organisation of a single animal, and—what is most mischievous for the beginner—it regularly ignores facts not of "classificatory importance," and so tends to offer a premium on superficiality.

The principal groups of animals will therefore be treated partly by the description of "types," selected as exhibiting the chief characteristics of the group, partly by the comparative method—that is, by taking up a particular organ or set of organs and tracing the modifications it presents through a series of groups. The more important fossil members of any division will be considered along with the recent forms, a good deal will be said of the embryology or development of the chief types, and the main facts of their distribution in space and time will be considered, as well as the question of classification and the principles upon which it is conducted.

From time to time the necessity will arise of discussing the relations between these several divisions of the subject and the explanations of them. It will be shown, for instance, that a proximate explanation of the extraordinary changes undergone by an animal in its development from the egg is afforded by the theory that the evolution of the individual is a recapitulation—much abbreviated and distorted—of the evolution of the species. Or, to take another example, it will be pointed out that in the doctrine of evolution we have the only satisfactory explanation of the fact that in tracing back the history in past time of many groups, the boundaries between them tend to disappear, and species are found at last assignable to no existing group, but combining in themselves the character of two or more. As a striking example I may mention the recent discovery of the second known specimen of the fossil called *Archæopteryx*, hitherto supposed to be a true bird, although exhibiting certain approximations towards reptiles. It is now known that Archæopteryx is completely intermediate between reptiles and birds—that it is indeed a feathered reptile; and it cannot be doubted that we have here clearly indicated the line of descent of the group of birds, at the present day so sharply separated from all other vertebrate animals. In the same way the mammalia, when traced back to the earlier tertiaries, are found to be represented by animals which are neither marsupials nor rodents, carnivores nor herbivores, but form a common group of generalised forms, from which the well-marked orders of mammals as we know them to-day are seen gradually to diverge as we trace the fossils from the lower to the upper tertiaries.

While this the greater part of the course is going on, the laboratory work will consist in the dissection of one or more common animals selected as types of each of the chief groups. The Mollusca, for instance, will be illustrated by the cockle or mussel, the slug, and perhaps the octopus; the great group of articulated animals by the crab, sandhopper, beetle, moth, spider, millepede, &c.; the Vertebrata by some common fish, such as the red cod, by a frog if it can be had, by a pigeon and a rabbit. In this way the student will become familiar with the entire organisation of a sufficient number of animal types to enable him to understand the description of other types given him in lectures or in books. Further illustrations of many points of importance will be afforded him by the examination of specimens from the museum, notably in the case of fossils, and in that of the skeleton, which latter, apart from its purely scientific importance, affords an excellent training for the faculties of observation, of comparison, and of memory. Moreover, if the time holds out I hope to be able to let the students see for themselves some of the chief stages in the development of the common fowl—the most convenient starting-point for the study of embryology.

Lastly, from time to time short practical examinations will be held. Subjects will be provided differing from those already seen, and the student will be encouraged to investigate their structure without help, and to compare the results thus obtained with those of the more formal work.

I think no one will doubt that a course of this sort must furnish a true discipline. Whether as a discipline it is superior, *cæteris paribus*, to a classical course—to a study of the grammar and construction of the Latin and Greek languages, and a certain acquaintance with their literature, I must leave to the decision of those who know more of the latter subjects than myself. Certainly a consideration of the faculties the two studies are likely to bring into play, train, and develop, leads one, in the absence of other data, save the sad memories of one's own school work, to assign a distinctly higher value to scientific than to grammatical study as a mental training. But this point has been so often insisted on by men whose words carry weight that no remarks of mine are needed. What I consider it my business to point out is the way in which a course in my own branch of natural science should, and the way in which it should not, be carried on, and I feel convinced that even those who have no knowledge of the subject will see that the training afforded by the course of which I have given a brief outline in observation, in induction and deduction, in the comparative method, and in the true understanding of the relations between cause and effect, is not easily surpassed, to say nothing of the less important, though by no means to be despised, training of the memory, and of the exercise of the imagination provided by theories of molecular structure, and their application to morphological and physiological problems.

As to the effect of these studies upon still higher faculties, I feel that I cannot do better than quote a well-known passage from a lecture of Prof. Huxley's, delivered nearly twenty-seven years ago. He says:—"There is yet another way in which natural history may, I am convinced, take a profound hold upon practical life, and that is by its influence on the finer feelings as the greatest of all sources of that pleasure which is derivable from beauty. I do not pretend that natural history knowledge, as such, can increase our sense of the beautiful in natural objects. I do not suppose that the dead soul of Peter Bell, of whom the great poet of Nature says—

"'A primrose by the river's brim
A yellow primrose was to him,
And it was nothing more,'—

would have been a whit removed from its apathy by the information that the primrose is a dicotyledonous exogen, with a monopetalous corolla and central placentation. But I advocate natural history knowledge from this point of view because it would lead us to *seek* the beauties of natural objects instead of trusting to chance to force them on our attention."

Indeed the elevating effect of science from this point of view is of quite the same nature as that of art, and with the alteration of a word or two the sentence put by Browning into the mouth of Fra Lippo Lippi expresses exactly the same idea as the passage I have just quoted:—

"For don't you mark?—We're made so that we love,
First when we see them painted, things we have passed
Perhaps a hundred times, nor cared to see;
And so they are better painted—better to us,
Which is the same thing. Art was given for that."

One may even go a step further and say, with the Laureate, that he who could know all about one single little flower would know "what God and man is."

I would draw attention to the fact that I have said nothing as to what is often called the practical bearing of scientific instruction. And this purposely; for we who have the charge of higher education in the ordinary sense—as distinguished from professional or technical education—have nothing whatever to do with so-called practical ends. Our business is, as far as in us lies, to train the minds of our students—to teach them to think and to learn for themselves, knowing that whatever career they

may choose, this sort of training will be of primary importance to them—will form indeed the surest foundation for any course of professional training they may afterwards choose to follow.

So far I have been considering only the elementary teaching of biology, devoting special attention to the course I propose to adopt for preparing beginners for the Pass Degree, and with certain additions to the work, for Senior Scholarships. It still remains to say something about the course of study for Honours in the biological sciences.

It is enacted in the regulations of the New Zealand University by what seems to me one of the wisest rules in the calendar,[1] that a candidate for honours in biology must specialise—that is, must choose some special branch of either zoology or botany, and work up that branch as fully as his time and opportunity will allow. He has already, in taking his B.A. degree, proved his general acquaintance with zoology or botany; he now has to show that, of some limited department of one of these sciences, he possesses more than a mere text-book knowledge.

Suppose, for instance, that a student selects the group of fishes as his special subject. It will be my duty to direct him to the more important works on ichthyology in the University and Museum libraries, so that while taking the most recent work on the general subject as his text-book, he may, when desirable, refer to the original sources of information and acquire the habit—most essential for a student of science—of seizing upon the points of real importance in a monograph or *brochure*. While undergoing this course of reading the candidate will dissect as many as possible of the more important New Zealand fishes, making careful notes and drawings of their anatomy, and comparing his results with the statements he finds in books.

But it is further enacted that the candidate for Honours shall send in the results of some original research. In the hypothetical case I have chosen the subject for investigation would most probably be an inquiry into some branch of fish anatomy as far as it could be worked out on New Zealand species—the nervous system, for instance, or the skull, or the digestive organs in one of the groups, or the detailed anatomy of some single species.

It is, I think, from this part of the Honours work that the conscientious student will derive the greatest benefit, and it is in the fostering of research on the part of its members that a university performs its highest duty. Until it assumes that position indeed, it is only a step above the high school, differing from it in degree only, and not in kind. It is only when original work is directly encouraged, and indeed looked upon as the goal of university life rather than the taking of a degree or the gaining of a scholarship—in other words it is only when knowledge is not only communicated, but advanced, that a university takes its true place, not as a mere finishing school, but as a centre of sound learning.

In the case of the advanced student I repeat it is only when his work becomes in some slight degree *original* that he derives the greatest possible benefit from it. "Every man," says Carlyle, "is not only a learner but a doer: he learns with the mind given him what has been; but with the same mind he discovers further; he invents and devises somewhat of his own. Absolutely without originality there is no man." It is impossible to estimate the benefit to a man's whole nature of setting him to puzzle out something that has never been thoroughly worked out before, of putting him upon his mettle to spare no effort in the elucidation of the problem before him, and to "hold it crime to let a truth slip." If a man has anything in him this assuredly will bring it out, more than years of absorbing other men's thoughts and verifying other men's results. The problem he has set himself may seem to others quite insignificant, and its solution a matter of no moment—"the pitifullest infinitesimal fraction of a product"—but to him it is all-important—"an ill-favoured thing, sir, *but mine own*."

This brings me to the last point I have to touch upon. It is to be hoped that a certain proportion of the students who study biology here may be brought to look upon it not as a means of education only, but as a pursuit to be carried on after leaving the University. It is interesting to notice how much scientific work in England has been and is done by what may be called scientific amateurs, men who, while engaged in professional or business pursuits, devote their spare time to the advancement of some branch of natural knowledge. And I think I am justified in saying that New Zealand has hitherto been pre-eminent among the Colonies for following out in this respect the traditions of the Mother Country. To say nothing of botany, many groups of animals have already been thoroughly well worked up, and considerable headway has been made with others; but "there remaineth yet very much land to be possessed," and one may venture to hope that workers from this University will before long begin to swell the *Transactions* of the New Zealand Institute and the publications of the Geological Survey. Upon any who may have this laudable ambition before them I would venture to urge the advisability—I might almost say necessity—of acquiring a sound and exact, although necessarily elementary knowledge of biology as a whole, before beginning to study any special branch. The work of a man who knows his own limited branch of science, and nothing beyond, is quite sure to be imperfect, and will most probably be evanescent. The highest results are only to be obtained by studying a group or a species, not only in and for itself, but in connection with other groups or species, by keeping always in mind the possible connection of one's own results with those of others, by remembering that the objects one is studying are not isolated things like coins or postage-stamps, but are *organisms*, whose special characters have been impressed upon them by forces which have been at work from the beginning of all things.

Finally, it is just possible that some day one of our students may be brought to take up biology as a career. I need hardly say that such a one, besides completing his studies elsewhere, would be probably compelled, unless possessed of private means, to exercise his profession either in Europe or in America, since there is very little chance at present of more than one biological appointment in a decade falling vacant in this Colony. But a man with a love for his subject and not afraid of hard work, who, after learning all he could learn here, availed himself of the best teaching at home—at London, Cambridge, or Heidelberg—would, I feel convinced, have every chance of success. He would never get rich; the present practical applications of biology are not such as insure fortunes. He would have all his life to be satisfied with an "*aurea mediocritas*" in matters of finance, but he could count upon what is even better than a large income—increasing joy and constant development through a thoroughly congenial life-work.

[1] I am sorry to see that the Senate at its recent meeting has adopted a regulation which cannot fail to lower immeasurably the standard of the Honours examination in biology. It is proposed in fact to make the candidate take up a special subject in both botany and zoology. A student, for instance, whose predilections are zoological, and who may never have studied botany at all, is to make a special study of "some one family of the vegetable kingdom," as well as of some group of animals. The inevitable result will be that one or both subjects will be crammed, and Honours will cease to have their legitimate value, and will become nothing more than a step beyond the Pass Degree.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Colleges meet on Saturday, October 15, and the professorial lectures begin the following week. The professors and lecturers in physics have drawn up the following scheme of lectures and classes for the Michaelmas term:—Prof. Clifton lectures on Wednesday and Saturday on terrestrial magnetism, and Prof. Bartholomew Price lectures on Tuesday, Thursday, and Saturday on the dynamics of material systems. Mr. Hayes lectures on electrostatics (treated mathematically). Instruction in practical physics is given daily in the Clarendon Laboratory, under the direction of Prof. Clifton, Mr. Stocker, and Mr. Heaton. Mr. Stocker gives an experimental lecture on elementary mechanics, and Mr. Heaton has a class for problems in elementary mechanics and physics. The above lectures are given in the University Museum. At Queen's College Mr. Elliot gives a course on geometrical and physical objects; at Christ Church Mr. Baynes gives a course on elementary heat and light; and at Balliol Mr. Dixon gives a course on elementary magnetism and electricity.

On Tuesday the Fellows of Wadham College elected Mr. G. E. Thorley to the wardenship of the College, in place of Dr. Griffiths, resigned. It is understood that Dr. Griffiths will continue to reside at Oxford, and will remain a delegate of the University Press and of the Local Examinations.

An examination for Natural Science Scholarships begins on Thursday, October 13, at Exeter and Trinity Colleges. The scholar elected at Exeter will be expected to read for honours in the biological school, and the scholar elected at Trinity will be expected to read for honours in chemistry or physics.

An election to a Brackenbury Natural Science Scholarship at Balliol College will be held in November. Papers will be set in physics, chemistry, and biology. Candidates may offer themselves in two of these subjects, and may also take mathematics or an English essay.

CAMBRIDGE.—Prof. Paget will lecture on Clinical Medicine at the Hospital this term; and Prof. Latham on the Physiological Actions and Therapeutic Uses of Remedies, at Downing College.

Practical Anatomy commenced at the Dissecting Room on October 5; and demonstration-for second year men on October 7. Prof. Humphry's lectures on the Organs of Digestion begin on October 13. A class in anatomy and physiology, preparatory for the second M.B. and the Natural Sciences Tripos, will meet for the first time on October 17.

Prof. Liveing lectures this term on the General Principles of Chemistry, and also on Spectroscopic Analysis, taking limited classes at successive hours on the latter subject; there will be both practical observation with spectroscopes, and explanation of principles and results. Prof. Dewar will lecture three times a week on Physical Chemistry, beginning October 14; and two tutorial lectures weekly will be given in connection with these lectures by Mr. A. Scott, the professor's assistant. Investigations may be carried on in the laboratories, with the approval of the professors. Demonstrations in Volumetric Analysis will be given by one of the demonstrators three times a week.

Mr. F. M. Balfour will give two courses of lectures (elementary and advanced) on Morphology, with practical work, at the New Museums, each course to extend over two terms. Both course will be on the Invertebrata this term.

Dr. Vines commenced his lectures on the Physiology of Plants at Christ's College on October 12.

Prof Stuart lectures on Mechanism three times a week; the workshops and drawing office open on October 14. Mechanical drawing and machine designing will be taught in the drawing office; and the use of tools, the elements of practical engineering and the construction of physical instruments in the workshops.

Prof. Lewis has two courses this term, one on Descriptive Crystallography, and the other on the principal minerals known as rock-constituents.

Lord Rayleigh lectures on Electricity and Magnetism; Prof. Cayley on Abel's Theorem and the Theta-functions, the deputy Plumian Professor on Practical Astronomy.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, September 7.—Mr. H. T. Stainton, F.R.S., president, in the chair.—Rev. A. E. Eaton exhibited a dried specimen of the nymph of a species of *Euthyplocia*, a genus of *Ephemeridæ* previously known only in the adult condition.—Mr. E. A. Fitch exhibited a larva of *Zeuzera æsculi*, infested with a species of *Encyrtus* in extraordinary numbers; specimens of a fly (*Drosophila cellaris*) bred from a bottle of pickles; a series of interesting galls (*Cecidomyidæ*), and some stems of *Equisetum* in which larvæ of *Dolerus eglanteriæ* were feeding.—Mr. T. R. Billups exhibited six new British *Ichneumonidæ*.—Mr. C. O. Waterhouse exhibited a specimen of the common mouse attacked by the larva of an *Œstrus*.—Sir S. S. Saunders exhibited specimens of *Sarcophaga lineata*, Fall., which destroys locusts in the Troad, and of *Chalcis flavipes*, Panz., parasitic on the parasite itself.—The president read a letter from the Colonial Office respecting the report forwarded by the Society on locust parasites.—Mr. C. O. Waterhouse read descriptions of some new *Coleoptera* from Sumatra.—Mr. J. S. Baly communicated descriptions of some new species of *Eumolpidæ*; and Mr. A. G. Butler communicated a list of butterflies collected in Chili by Mr. T. Edmonds.

PARIS

Academy of Sciences, October 3.—M. Wurtz in the chair.—M. Dumas communicated the decisions recently come to by the Congress of Electricians on electrical standards. He also exhibited an ingot of steel produced by Dr. Siemens in the Exhibition, by electric fusion (in fourteen minutes) of a few kilogrammes of steel in a magnesia crucible. The expenditure of fuel to drive the machine was less than that required by direct fusion in a common furnace.—On the secular displacements of the planes of orbits of three planets, by M. Tisserand.—Public experiments on vaccination of symptomatic *charbon*, made at Chaumont (Haute-Marne) on September 26, 1881, by M. Bouley. Symptomatic charbon is proved to be distinct from bacteridian charbon; *inter alia*, the microbe of the former, introduced into the veins, insures future immunity, producing at the time only slight fever. This vaccination of MM. Arloing, Cornevin, and Thomas, differs from that of M. Pasteur in that the natural virus is used in all its energy (not attenuated). Care has to be taken not to let the virus enter cellular tissue, but only the (jugular) vein. The experiments here recorded were made on 25 young cattle, 13 of which had been vaccinated, and the results distinctly vindicate the method. In the second injection the cannula was deeply inserted in muscular tissue.—On a new application of the equation of Lamé, by M. Gyldén.—Observations of the comet *d* 1881 (Encke) and *e* 1881 (Barnard), made at Paris Observatory, by M. Bigourdan.—Application of radiophony to telegraphy; multiple inverse electric teleradiophone, by M. Mercadier. (This was a sealed packet, deposited May 31.) A continuous current traverses a series of radiophonic selenium receivers and telephones at station A, then the line, then another series at B. Before each receiver a wheel with circle of holes rotates regularly, and the passage of the light rays is blocked at will with a Morse key, giving interruptions of the musical notes in the telephone, corresponding to Morse signals. The wheels are arranged to give different notes, and each listener with a telephone concentrates his thought on a particular note. The system may be applied to lines of great length.—On a new electromagnetic pointer designed for experimental researches, by M. Noel. The author sought a means of estimating very quickly and exactly the physiological duration of tendinous reflex phenomena in muscles. A needle is arranged with a friction-coupling of two hollow cones, one of which, when in contact with its concentric cone, causes the needle to traverse a graduated disk at the rate of once in one second; contact of the other cones stops the needle. The motion is determined by currents in a Hughes differential train, *i.e.* two opposite electro-magnets with common armature in equilibrium between. When one current passes through their four coils, the armature is attracted to one magnet, and remains there till an opposite current brings it to the other. These currents flow respectively on applying to the tendon an instrument, which closes the first circuit, and on contraction of the muscle, which opens this circuit and closes the other.—On secondary batteries, by M. Rousse. In one arrangement he uses a palladium plate as negative pole, and lead as positive; the liquid being sulphuric acid solution (one-tenth). Another battery also giving good results is made with sheet-iron, lead, and a solution of sulphate of ammonia (the lead either pure or covered with litharge, or pure oxide or sulphate, or all these mixed). Again, sheet iron, ferro-manganese, and sulphate of ammonia solution.—On a manganese battery, the salts of which are utilised or regenerated, by M. Rousse. Ferro-manganese is substituted for zinc in the Bunsen battery. For weak currents and in apartments, permanganate of potash is used for depolarisation (in other cases nitric acid). The salts produced are sulphate and nitrate of manganese, or sulphate and nitrate of potash. Permanganate of potash, or peroxide of manganese is then obtained by chemical processes.—On levulose, by MM. Jungfleisch and Lefranc.—On an egg of an ancient ostrich, by M. Balland. This was from a subterranean columbarium at Gonzaga. He compares its chemical constitution with that of a recent egg. There is more carbonate and phosphate of lime, and less carbonate of magnesia, &c.

CONTENTS

THURSDAY, OCTOBER 20, 1881

GEOGRAPHY, NATIONAL AND INTERNATIONAL

IT seems impossible to get any full and authentic account of the doings of the recent International Geographical Congress held at Venice, so that at present it is difficult to say how much it did for the promotion of the subject with which it is connected. Congratulatory addresses seem to have been a prominent feature, and much time was devoted to the subject of interoceanic canals, with special reference to those across the isthmuses of Panama and Corinth. If the Congress itself was disappointing, the Exhibition in connection therewith appears to have been a great success. It was a striking illustration of the dimensions which geographical science has now attained. Maps and charts and globes ancient and modern we should of course expect to find; sextants and compasses also, as well as tents and hammocks, and other paraphernalia of the explorer. But besides the exhibits to which geography can lay special claim, nearly every other science was laid under contribution in one way or another. Geology and meteorology, botany and zoology, and ethnology, and even chemistry and physics, have been placed under levy to help in forming the multifarious departments to which geography now lays claim. This wide extension of a subject, which at one time had little claim to be considered scientific, has its advantages and disadvantages. It has reached its widest limits on the Continent, in Germany, where there are chairs of geography, whose professors, to judge from their programmes and their text-books, would require to be almost omniscient. If a student faithfully follows the course thus chalked out, he ought to end by having a fair knowledge of all the sciences. And it comes to be a question whether the same object might not be attained by beginning at the other end. Why, it may be asked, might not the student begin by acquiring a knowledge of the principles and facts of the sciences concerned, and apply them afterwards to the special subject of geography? At the same time, it must be confessed, to have a complete knowledge of the geography of the world, a little of everything is necessary; and the Continental conception of the subject is certainly preferable to the bald and dry idea entertained of it in this country, as exhibited in most of our text-books. Happily better things may be looked for in the future with the use of such text-books as Green's "Geography of the British Isles," and the late Keith Johnston's Geographical Handbook. While geography thus levies tribute on all the sciences, it must be admitted that in return she largely pays back her debt in the multitude of new data brought home by the best of her pioneers. Unfortunately all explorers do not start with that knowledge of the sciences which would greatly increase their observing capacity. Every explorer is not a Livingstone or a Holub, a Prejevalsky or a Maclay; and for such especially, as also for missionaries, a course of geography similar to that which prevails at the German Universities would be a decided advantage. For practical, and especially for school purposes, it is well that some limit should be defined as to the sphere of geography; the happy medium has, we think, been well struck by M. Elisée Réclus in his magnificent "Géographie Universelle," which, when complete, will no doubt form a mine for compilers of text-books.

One of the most valuable recent developments of geography is seen in the scheme conceived by the late Lieut. Weyprecht, for the establishment of a ring of Polar observatories. This is now close upon being an accomplished fact, as will be seen from the account we gave of the recent meeting of the International Polar Congress at St. Petersburg. As our readers are no doubt aware, many Arctic authorities are of opinion that the days of great and expensive national Polar expeditions are past, and that the money thus spent would be put to much better use by being devoted to the carrying on of a continuous series of observations. At various points around the Arctic area observatories will be established as near as practicable to the Pole, where a continuous series of observations will be taken, according to a common pre arranged plan. These observations will be connected with meteorology in all its departments, with terrestrial magnetism, the aurora borealis, atmospheric electricity, the movements of the ice, biology, combined with geographical exploration where practicable. After a year or two of such observations we may then be able to compare and co-ordinate Polar conditions with those which prevail in regions further south. A vast array of data must necessarily be accumulated that cannot but be turned to valuable account by science. Our knowledge of the meteorology of the temperate zone can never be complete until we are well acquainted with Arctic conditions, and thus the work to be done at these observatories will have an important practical bearing. Not only so, but it is maintained that it is only when we have the knowledge which will be collected at these stations that we shall be in a condition to send out an expedition for the Pole itself with anything like scientific assurance of success. We cannot but regret, then, that England has no share in the scheme. The countries forming the International Association are Russia, Germany, Norway and Sweden, Denmark, Austria, the United States, and we believe Canada; France and Switzerland lend it their countenance, and Lieut. Bove's Italian Antarctic expedition is to some extent affiliated to the Association. Stations are to be established on the north coast of Siberia, Novaya Zemlya, Spitzbergen, Jan Mayen Island, the west coast of Greenland, Lady Franklin Bay, and the neighbourhood of Behring Straits. The colony for Lady Franklin Bay, sent out by the United States, has already, we believe, reached its destination, and the others will probably be all at work next year.

While speaking of Arctic matters we must express our surprise at a journal like the *Pall Mall Gazette* talking of Polar exploration as a barren work. This of course depends on what one looks for in the way of results; if an immediate return in *£ s. d.* is looked for, the work is barren enough certainly, as barren as all purely scientific research seems at its first undertaking; though even the *Pall Mall Gazette* must admit that all the difference between the present and the past, materially and intellectually, is due to the ultimate results of this same barren work. And we are glad to see that Capt. Adams, the well-known Dundee whaler, again found time to take part in the

barren work of Arctic research while doing his best to fill his blubber tanks. He succeeded in sailing up Wellington Channel as far as has hitherto been done; visited the scene of the *Fury* and *Hecla* disaster, and brought home some interesting relics of the Franklin Expedition, as well as some additional information. He fell in with an Eskimo who remembers Crozier and his men, and from whom Capt. Adams seems to have obtained some additional information on the fate of the disastrous expedition. It cannot, however, amount to much after what has been done by the late Capt. Hall, and quite recently by Lieut. Schwatka; still, Capt. Adams deserves the greatest credit for attempting to increase our knowledge of the Arctic area at great risk to himself; he is evidently made of the right stuff. From the United States, we learn, an expedition is to be sent out as early as possible to endeavour to find the records of the Franklin expedition, which Capt. Hall always maintained would be found in the clefts of some rocks near the scene of the disaster. The results will be looked for with interest, and will at least tend to the promotion of knowledge. So also will the international search for the missing *Jeannette*, which it is rumoured may be undertaken next summer. The suggestion of Baron Nordenskjöld, deserving as it must be of every consideration, seems improbable; the bodies and the bottle of whisky found at the mouth of the Lena on September 13, 1879, could hardly have belonged to the *Jeannette*, which was seen steering for Wrangel Land on September 2—*i.e.* 1400 or 1500 miles away. Mr. B. J. Jenkins suggests that the *Jeannette* has been fortunate enough to get into open water not far from the Pole, and may turn up next year. There is no harm in hoping on to the last, as we are justified in doing after the experiences of the Austro-Hungarian Expedition.

What is the conception of geography entertained by our Geographical Society may be learned from the very interesting sketch of its history just published by Mr. Markham in connection with the jubilee of its foundation, which took place upwards of a year ago. The Geographical Society was founded on May 24, 1830. Its original objects were "to collect, digest, and publish interesting and useful geographical facts and discoveries; to accumulate a collection of books on geography, voyages, and travels, and of maps and charts; to keep specimens of such instruments as are most serviceable to a traveller, to afford assistance, instruction, and advice to explorers; and to correspond with other bodies or individuals engaged in geographical pursuits." All highly necessary and useful objects in connection with the advancement of knowledge. The Geographical Society absorbed the old African Association and the Palestine Club, and among its founders or first officers we find the names of Murchison, Robert Brown, Sir John Barrow, Admiral Smyth; and in its first list of Fellows some of the leading scientific men of the time. Mr. Markham complains that the Royal Society did so little for the promotion of geography before the Geographical Society came into existence; but it would have been beyond the functions of that Society to deal with the objects referred to above, and Mr. Markham admits that it really did a great deal to promote all that was most distinctly scientific in connection with geography. It must be admitted that the Geographical Society has very faithfully carried out its programme. It soon became popular, and after it recovered from the results of bad management and extravagant expenditure, it rapidly increased in members and income, until now it is probably the most numerous, if not the most wealthy, learned society in the world. Admiral Smyth established its financial prosperity, and, as every one knows, Sir Roderick Murchison made it fashionable. It has now upwards of 3300 Fellows, and its receipts in 1880 amounted to 8600*l.*, while the Society's funded capital was 18,500*l.*, not to mention the value of its fine premises, library, maps, &c., in Savile Row. Notwithstanding all this material prosperity and its weakness for showing off travelling lions, the Geographical Society has really done much for the promotion of exploration and geography. Directly or indirectly it has been connected with all the expeditions of importance that have gone out from England since it was founded; it has encouraged exploration by grants of money amounting in the aggregate to a considerable sum; it has bestowed its medals and other rewards on explorers and geographers of various nationalities, all of them men who had really earned such honours; it has been of much service in instructing explorers in the technicalities of their business, and recently has established a sort of school for topographical observation; it has accumulated a valuable library and collection of maps, which are freely at the service of all who care to use them; and its *Journal* and *Proceedings* contain a vast amount of information, not simply relating to geographical exploration, but many of the papers relate to the more scientific aspects of geography. The Society has itself initiated or materially supported not a few expeditions of importance, one of the most productive being that which Mr. Joseph Thomson recently brought to so successful a conclusion. One of the most important functions undertaken by the Society is the yearly examinations in geography which it holds in connection with schools; and the papers set at these examinations are both comprehensive and scientific; in this direction the Society is doing really good work in the promotion of scientific geography. It may be remembered that the Council recently instituted an annual course of lectures on the more strictly scientific departments of geography, by men of acknowledged eminence in their subjects. Unfortunately the Council did not feel themselves encouraged to continue these lectures; but we venture to think they were too easily discouraged. Let them by all means have their fortnightly popular meetings during the season; but at the same time there is nothing to hinder them having also more esoteric meetings at stated intervals, at which original papers might be read or lectures given of a kind akin to those found so frequently in the proceedings of Continental geographical societies. In this way the Society would do much to encourage scientific geography, and be justified in claiming the rank of a really scientific society, which many of its well-wishers feel that it can hardly claim at present. Why, moreover, should the Council not at least lend their countenance to the great international scheme of Polar observatories, and take some steps to induce our Government to take an active share in the work? They might easily do this without in any way fettering their action in reference to those great Arctic expeditions to which some of them appear to be so partial.

The Society has shown itself remarkably liberal in the distribution of its medals; out of the 109 which have been awarded since its foundation, 37 have been given to foreign explorers and geographers. Mr. Markham gives a brief and interesting sketch of the great advances in geographical knowledge which have been made since the Society was founded, but shows at the same time how much remains to be done, even when we have obtained a rough knowledge of the whole of the earth's surface, while deep-sea research is yet only in its infancy. The little volume also contains an admirably-arranged list of papers in the *Journals* and *Proceedings* of the Society, covering nearly fifty pages; this, we believe, is the work of the librarian, Mr. Rye, and will be of the greatest value for reference.

Altogether it is evident that in recent years geography not only has made immense advances in the knowledge it has acquired of the "world and they that dwell therein," but has acquired a character which entitles it distinctly to be regarded as a department of science.

THE LATE A. H. GARROD'S SCIENTIFIC PAPERS

In Memoriam. The Collected Scientific Papers of the late Alfred Henry Garrod, M.A., F.R.S. (London: R. H. Porter, 1881.)

FEW customs are gaining greater ground at the present day than that of making the death of any man who, by his energy or talents, has raised his name a little above that of the unknown crowd, a reason for opening a subscription and calling upon all his friends and admirers to tax themselves to found a memorial commemorative of his career. It is first decided that there shall be a memorial, and then the question usually arises as to the form that it shall take. It very often happens that some person or some institution has a need at hand. The prosperity of a school, and indirectly of all connected with it, will be promoted if it has scholarships attached to it which will attract needy students. A window is wanted to complete the ornamentation of a church. Those interested in the church or school eagerly seize upon the opportunity which the hand of death has afforded, and suggest a fitting method of bearing testimony to the memory of the departed. Such memorials generally, after a few years, retain wonderfully little personal connection with him they are supposed originally to honour. The name remains, but the person is forgotten, unless preserved in remembrance for other and more cogent reasons.

Personal memorials of really eminent men, of those who have done good service to mankind, are of inestimable value. True records of their lives, their character, their works, their words, even of their features, afford encouragement and example to all who come after. By such memorials the whole world is enriched and its progress ensured. Among such we scarcely know of any more appropriate to its subject than that which has just been carried out by the Garrod Memorial Committee. It is a handsomely printed large octavo volume of 527 pages, containing an excellent portrait, a memoir, and a reproduction of all the important contributions to science made during the short but fruitful career of the extremely talented biologist whose loss we deplored almost exactly two years ago. The work contains, in a most convenient form for reference, a vast number of facts relating chiefly to the anatomy of birds and mammals, together with all the figures with which the several memoirs were originally illustrated, and a copious index. It has been ably edited, evidently as a labour of love, by Prof. Garrod's successor in the post of prosector to the Zoological Society, Mr. W. A. Forbes, with the assistance for the physiological portion of Prof. E. A. Schäfer. Mr. Garrod was all his life favourably circumstanced to a remarkable degree for pursuing biological research. He had from his earliest age the advantage of scientific associations and the best of educations, and was soon placed in an independent position, which enabled him to make the occupation of his life that which almost all others, even those holding most of the existing scientific appointments, can only do in snatches of time saved from the educational or administrative duties connected with their offices. Of all these advantages he fully availed himself; but considering he was only thirty-three years old at the time of his death, the amount of his already-published work when collected together is surprising, and causes the greater regret that he was not spared to continue what he had so well begun, especially as his editor tells us of the immense amount of material in notes and drawings which he had accumulated, besides that which was in a sufficiently finished state to see the light.

In these days, when so much is being said about the encouragement of scientific research, and so many experiments are being tried, both with public and private money, as to the best means of promoting this end, we cannot help making the reflection, before concluding our notice of this volume, on the great results that may follow a small expenditure judiciously and steadily devoted for a series of years to one object. If the Zoological Society had not in 1865 established its prosectorship, we should have seen little of the really solid advances in our knowledge of the anatomy of the two higher classes of vertebrated animals contained in the valuable memoirs of Dr. Murie, those collected in the present volume, and those now in the course of publication by Mr. Garrod's successor in the office.

THE DIAMONDS, COAL, AND GOLD OF INDIA

The Diamonds, Coal, and Gold of India. By V. Ball, F.G.S. 12mo. (London: Trübner, 1881.)

IN this handy little volume the author presents us with a compendium of the facts known concerning the occurrence and distribution of the three principal mineral products of India. The work being so designed that it may be used as a handbook to the detailed accounts published by the Geological Survey of India and by other authorities in numerous scattered publications to which full references are given. In the first chapter the different localities producing diamonds, including both active and abandoned mines, are noticed in some detail. These are grouped into three areas, the most southerly being that to which the name of Golconda is usually applied, although, as the author points out, that town is not actually in a diamond producing district, but was the staple place where the product of the district was bought and sold.

The actual mines are in the southern part of the Madras presidency, in the districts of Kadapah, Karnul, Kistna, and Godaveri. The second great tract, further to the north, lies between the Mahanadi and Godaveri rivers, the chief localities being at Sambalpur and Weiragud, eighty miles south-east from Nagpore, and at a few places in Chota Nagpore. The third great tract is in the vicinity of Panna in Bandelkhand. In addition to these a few small diamonds are reported to have been found near Simla. In all cases the diamonds appear to have been found in sandstones or conglomerates, or in the gravels derived from their alteration. These sandstones are referred in the southern localities to the lowest member of the Karnul formation, which as a whole is considered to be the equivalent of the lower part of the so-called Vindhyan formation of Northern India. An upper group of the latter, the Rewah conglomerate, being the diamond-bearing bed in Bandelkhand. There does not appear to be any authenticated instance of a diamond being found in India in other than sedimentary rocks. One case, however, at p. 49, where the matrix is said to be "a net-work of strings of calc spar inclosing laminæ and small lumps of green clay," suggests the possibility of the material in question being a decomposed basalt or basaltic tuff, and as such comparable with the South African occurrences. What the present total production of the mines may be we are left to guess; as far as can be gathered from the scattered notices collected by the author, the larger number of the mines are of historical interest only.

The second chapter, that on Coal, is mainly an abstract of the communications on this subject made to the Records of the Geological Survey of India by Mr. Theodore Hughes, and is rather behindhand in point of time. The latest information appears to refer to the year 1878-79. The arguments for and against the supposed Mesozoic age of the Indian coal-bearing rocks are given in abstract with great fairness, and the author's conclusion that "floras alone form an unsafe guide to the correlation" of the ages of rocks in different countries is probably the only safe one that can be drawn from the available evidence. As a mere question of stratigraphical position it is probable that these rocks represent the uppermost coal-measures (Permo-Carboniferous) of Europe. Any one acquainted with the smaller coal-basins in the south of Europe cannot but be struck with the numerous analogies between them and the Indian coal-fields, more particularly in the thickness and irregularity of the seams. The author's statement that the Ramgunj coal "may be described as a non-caking bituminous coal," is rather too general. It is true for the larger seams, but besides these are to be found others in which the caking property is as well developed as in any caking coal in the world. The coke produced is not of particularly good quality, which is however due to the large quantity of ash in the coals, but as to their caking capacity, there can be no doubt whatever. The estimation as to the quantity of coal available seems to be rather wild guesses in some cases, and one of these, for which the data are professedly given, is a good specimen of an arithmetical puzzle. They are as follows (p. 69):—

"The coal occurs in three principal seams . . . average total thickness of 16 feet . . . over an area of 8½ square miles. The amount of coal may therefore be estimated at 1,360,000,000 tons, and the available portion of this at 80,000,000."

How the largest of the above figures is obtained, and what its relation to the smaller quantity may be, is certainly not apparent from the author's statement. An allowance of 94 per cent. for faults, waste, and unworkable coal, which the above figures lead to, seems rather large.

The chapter on Gold contains extracts from most of the published details on the occurrence of precious metals in India down to the Reports of Mr. Brough Smyth, and the latest remarks of Indian newspapers, which latter however are dated as far back as May, 1880. An original investigation of the author's as to the distribution of auriferous detritus in areas occupied by rocks of different characters is of interest. He found that the proportion of gold obtained was larger upon crystalline schists than upon gneiss and granite, a result which agrees with that generally obtained in other parts of the world. As this was predicted to him by his native workmen, it is difficult to see how the author arrives at his belief that gold washing in India affords an example of human degradation.

His evidence seems rather to show that the gold washers have a highly skilled and minute knowledge of the distribution of gold-bearing alluvia, but the value of such knowledge is diminished by the circumstance that the amount of such material available has been practically exhausted by the labours of many generations of workers through a period of 2000-3000 years. The great extent of old workings discovered in some of the Wynaad mines also shows that the "old men" were no contemptible workers as vein miners.

In the earlier part of the volume the work done by "amateurs" in Indian geology is somewhat pointedly contrasted with that of the "professionals," whose whole time is devoted to the subject. This is to be regretted, as is also the assumption of an air of finality for the work of the Indian Survey, which the nature of the work certainly does not allow. For instance, we are told in the same paragraph that the rocks of the Vindhyan formation are absolutely azoic, and that they may be of any age, from Lower Silurian to Carboniferous; the real meaning of this expression being that no fossils have as yet been found in them. In this sense the New Red Sandstone might be said to be azoic over a great part of the central plain of England. The results of the Indian Survey are of great value as furnishing a broad outline of the stratigraphical features of the peninsula, but there will be work enough and to spare for both amateur and professional for many years to come before that outline is moderately filled in detail.

H. B.

OUR BOOK SHELF

The Student's Handbook of Acoustics. By John Broadhouse. (London: William Reeves, 1881.)

We are not quite sure what the title of this work is. The title just given is from the lettering on the back. Within the covers appears a second title, "Musical Acoustics," and on the actual title-page appears the triple announcement, "The Student's Helmholtz," "Musical Acoustics," and "The Phenomena of Sound as connected with

Music." The book itself may without unfairness be described as an "arrangement," or rather as a "pot-pourri," inasmuch as it resembles those musical compositions in which some of the fragmentary themes of one or more great masters are dished up for the public in some new or popularised setting, consisting of commonplaces of a more or less florid type. About 80 per cent. of the pages before us consist of clippings and quotations taken *verbatim et literatim* (and in quotation marks be it added) from the works of Helmholtz, Stone, Pole, Tyndall, and Sedley Taylor, interspersed with a connective-tissue woven from the "author's" own brain. We have found this ingenious fabric very remarkable reading, and have gleaned a number of new facts from it. We have learned, for example, that the transmission of verbal messages, prayers, hymns, and sermons through the telegraph wire by the telephone must be held to "prove that air is not the only medium through which sound-impulses can pass." We find our author declaring on p. 80 that the reason why so romantic a name as the "syren" should be applied to so matter-of-fact an instrument *does not appear;* while on p. 98 he seems to have made the discovery that the name is a misnomer, because "Homer's Σειρήνεοs" (*sic*) were not endowed with the power of singing under water as this instrument can. Our author is very unhappy in dealing with equal temperament, and complains that nearly all writers on temperament, with the exception of Mr. Ellis, describe it as dividing the octave into twelve precisely equal semitones, "without explaining that these semitones are not absolutely equal." That the perfect equality of the theoretically equal temperament is never attained *in practice* is indeed true; but why does our author find fault with writers on temperament for stating the exact theory? His accusation against Dr. Stone for palpable misuse of language (on p. 359) is utterly out of place, and only shows that the author has not comprehended the true meaning of a musical interval as defined by a ratio. He appears not to know that if an octave is divided into twelve exactly equal geometrical parts or ratios, the differences between the successive terms of the ratios are not, and cannot be, arithmetically equal to one another. Hence his attack on the perfectly unexceptionable statements of Dr. Pole and Dr. Stone. The diagrams with which the work is interspersed consist principally in pictures of syrens and in copies of wave-forms taken from Mr. Sedley Taylor's "Sound and Music," and spoiled by drawing them as if made up of semicircles pieced together. The wave-form given on p. 266 to illustrate beats does not show the wave-form of the beat *at all:* and though the author gives on p. 102 a wave-form which illustrates a beat admirably, he appears not to know it, as he passes it by as being merely one of a few different forms of tracing which a vibroscope can register. But we have said enough to justify us in having at the outset pronounced "The Student's Helmholtz" to be what we called it—a *pot-pourri*—or, in the plain English tongue, a *hash*.

Afrika im Lichte unserer Tage. Bodengestalt und geologischer Bau. (With a Hypsometrical Map.) By Josef Chavanne. (Vienna: A. Hartleben.)

The conclusions come to by Herr Chavanne we have already referred to. Africa, he finds, is, on the whole, a high plateau or table-land, crossed here and there by mountain-chains or single elevations. The plateau commences in most places at a remarkably short distance from the sea, the slopes south of the equator being particularly steep. North of the equator the land may be looked upon as a very slightly inclined plane, which, like the southern plateau, is also crossed by separate elevations, some of them being very considerable. The presence of numerous, and for the greater part widely-distributed, lakes is unlike the general physiognomy of the other large continents. By far the most important part of the author's work is the excellent hypsometrical map which accompanies the book, and to which we referred a short time ago. Its scale is 1 : 30,000,000. The elevations are marked in eight different tints of brown, showing so many grades and altitudes from zero upwards. Thus at one glance we see the African continent rising as a rule from 0 to 600 metres in the northern half, while, in the southern half, elevations from 900-1200 metres are the rule. The greatest heights—those of 1500-2000 metres and more—are packed close together on the east side, between the southern end of the Red Sea and the Zambesi River, and only occur again in the extreme south-east (Natal) and far up in the north-west (Atlas). The text of the book is well written; the author's descriptions are always attractive, to the point, and free from all superfluous wordiness.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Struggle of Parts in the Organism

The review of Dr. Roux's work on the "Struggle of Parts in the Organism" by Mr. Geo. J. Romanes which appears in your number of September 29 (p. 505) contains some passages which, I venture to think, are hardly consistent with the purpose to which the columns of Nature are devoted. I understand that purpose to be the discussion of scientific facts and scientific laws, properly so called. I should be the last to deny that these facts and these laws may have, and indeed must have, their own ultimate bearing upon theology, whether natural or revealed. But it is not the purpose of a purely scientific journal to enter upon this discussion; it is one which cannot be there pursued without involving controversies alien to the spirit in which physical science ought to be studied and explained.

And if even temperate discussion upon the subject ought to be avoided in a purely scientific journal, still more ought there to be a scrupulous abstention from dogmatic utterances which are hostile to theological opinions, and which are unsupported by even the semblance of argument.

In the passages to which I refer Mr. Romanes asserts that to the whole "argument from design" in nature an "end has come"—as the result of Mr. Darwin's Theory of Evolution—that the "fountains of this great deep have been broken up by the power of one man," and that "never in the history of thought has a change been effected of a comparable magnitude and importance."

As an expression of the opinion of Mr. Romanes that the Darwinian theory ought to put an end to the "argument from design," this assertion may be allowed to pass. But as the assertion of a fact I venture to say that it has no foundation. There are many minds, including some of those most distinguished in science, who not only fail to see any contradiction between evolution and design, but who hold that the doctrine of evolution and the facts on which it is founded have supplied richer illustrations than were ever before accessible of the operations of design in nature.

I should be transgressing my own rule were I to defend this view in your columns. I shall therefore content myself with saying that no possible amount of discovery concerning the physical causes of natural phenomena can affect the argument that the combination and co-ordination of these causes which produce the "apparent" effects of purpose are really and truly what they seem to be—the work of Mind and Will.

Inverary, October 4 ARGYLL

Solar Chemistry

The researches of Mr. Lockyer, and others, summarised by him in recent numbers of Nature, have to a great extent complicated the aspect of this grand problem, which appeared so

simple to Stokes and Thomson in 1852, and to Stewart and Kirchhoff a few years later.

I wish to consider briefly, what are these new and puzzling complications of the solar problem; and whether we may not still preserve our belief in the existence of *essentially different* elementary atoms, which is the basis of the beautiful Vortex Theory. For it seems that to hazard (however *naturally*) such a step as is involved in assumed dissociation of the (so-called) elements, before we make certain that no less serious hypothesis will account for the observed facts, is contrary to the spirit of Newton's *Regulæ Philosophandi.*

The most prominent of these complications seem to be—

(1). The variations of the relative brightness, width, &c., of the lines in the spectrum of a particular substance, in dependence on the source and circumstances of its incandescence.

(2). The so-called "long" and "short" lines. (These, as will be seen, are probably a case of (1).)

(3). The fact that, in the spectra of sun-spots, *some* lines supposed to be due to a particular element indicate rapid motion of the glowing gas; while others, supposed due to the same element, give no such indication.

(4). The (at least apparent) coincidence of lines in the spectra of two or more elementary substances.

To these may be added:—

(5). The remarkable peculiarities of star-spectra; especially the paucity, and the breadth, of the lines in the spectra of *white* stars.

As regards (1), let us consider a sounding body with a large number of different modes of vibration, exposed to impacts either periodic or at least with an average period. The relative intensities of the various notes which it can give will obviously depend upon the period of the impacts. Now this is precisely the case of a particle (I use the word to avoid misconception) of a glowing gas. The average number of blows it receives will depend on (*a*) the number of particles per cubic inch (and also upon *whether there be another gas present or no*, a point of very great importance) and (*b*) the temperature, which is directly connected with the velocity of the particles.

Change the density, the temperature, the admixture with foreign substances, or any two, or all, of these; and the *average period of the battering* to which a particle is subjected may be so altered as to elicit from it *in any required ratios of relative intensity* the various simple rays it can give out.

It will readily be seen that this may account for all of the phenomena of classes (1) and (2) above.

(3) may be accounted for in many ways. I mention only one, as my object is merely to show that we are *not yet* compelled to accept dissociation of so-called elements even in its mildest form. Other modes of escape, though not quite so simple, present themselves.

What is seen in a sun-spot is the integral, as it were, of all that is taking place (as regards both radiation and absorption) in many thousand miles of solar atmosphere, containing the same substance under the most varied conditions. That portions in which certain lines of that substance are prominent over others may be at rest relatively to the observer along the line of sight; while others, in which (from different density, temperature, or admixture, as above explained) other lines are specially prominent, may have large relative velocities, is certain. This would at once account for these singular observations.

As to (5) we must remember that in a star spectrum we have, as it were, a *triple* integral. For we not only integrate through the depth of the atmosphere, but also over the whole surface of the star; spots, hurricanes, and rotation of the whole, included. This is equivalent to the *superposition* of innumerable separate spectra, no two of which may have *any one* individual line in the same place or of the same breadth, &c. Feeble lines may, in fact, entirely disappear under such treatment.

(4) If not due to want of dispersive power in the apparatus, this may be legitimately attributed to inevitable impurities. It is only in "tall talk" (or in advertisements) that any human preparation, elementary or not, can be spoken of as "*chemisch rein.*" And we all know how faint a trace of impurity can be detected by the help of the spectroscope.

Even in the last resort, I see nothing to hinder the existence of exactly equal vibration-periods in two perfectly distinct vortex-atoms:—though their occurrence is extremely improbable.

If we could get an absolutely transparent gas; one, therefore, which could give no radiation under any circumstances; the study of the behaviour of a given quantity of hydrogen mixed with different proportions of it in a vessel of given size, and subjected always to the same conditions of incandescence, would give us invaluable information. G. H.

Replacing Flakes on Palæolithic Implements

THIS wonderful feat was first performed by my friend Mr. F. C. J. Spurrell of Dartford. On first thoughts the thing seems utterly impossible, and it is obvious that no flake can possibly be replaced upon an implement unless one lights on the exact spot where the instrument was made, and finds both implement and flakes in position. Mr. Spurrell so found his material. During the present summer I have discovered another and similar Palæolithic floor, far removed from Mr. Spurrell's, and where implements and flakes are exposed in a stratum perfectly undisturbed since they were gently covered up in Palæolithic times with fine sand containing the shells of such freshwater molluscs as Unio, Cyrenia, and Bythinia. For obvious reasons—the chief one of which is that my work would be totally stopped if I mentioned the locality—I will content myself with stating that the position is nearly a mile from any river, and the *floor* is 41 feet above the level of the nearest stream; above the floor is a thick deposit of fine stratified sand, and above that loam. On this Palæolithic floor I have found several implements and a large number of flakes, and on one of the finest implements, an example 6 inches long, 3½ inches wide, and weighing 1½ lb. I have been able to replace two flakes, one 2½ inches long, the other 2 inches in exact position; the flakes slightly overlap each other on the implement, and both have been struck from the edge of the implement at right angles across its face. The implement and flakes were close together, and with them I found a hammer-stone of flint with a distinctly battered and abraded edge. Mr. Spurrell replaced many flakes round his implement, but the implement itself was a spoilt and poor example. My implement, on the contrary, is an unusually fine one, large, heavy, and perfect. Both the implement and flakes show a little of the original grey crust of the flint from which the instrument was made, and this peculiar *grey* colour led me to attempt the replacement of the flakes with the above-mentioned successful result. One flake has a slightly uneven edge—in some instances considered a proof of use—the second flake is quite sharp. I shall exhibit this implement, with other implements, flakes, &c., from the same place, at an early meeting of the Anthropological Institute. WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

Integrating Anemometer

PERHAPS the following brief description of the integrator devised by me will suffice to establish its near kinship with Mr. Wilson's (NATURE, vol. xxiv. pp. 467 and 557):—A roller with a spherical edge is made to revolve with a velocity proportional to that of the wind as recorded on an anemogram. This roller presses on a plane table carried by two mutually perpendicular pairs of rails in planes parallel to that of the table. The lowest of the pairs of rails is supported by a frame carried on the extremity of a vertical shaft. The point of contact of the roller with the table lies in the prolongation of the axis of the shaft. The table can rotate with the shaft, but not independently. By a simple arrangement the shaft, and consequently the table, are caused to take up positions corresponding from moment to moment with the direction of the wind record on the anemogram. A style concentric with the shaft presses lightly against a compound sheet of tracing and carbonised paper attached to the under side of the table. Arrangements are also made for obtaining the sum of the movements of the table toward each of the four cardinal points. If the roller be moved with a velocity proportional to that of the wind, whether directly by a cup-anemometer or by a mechanical translation of the trace as given by such an instrument, while the table simultaneously assumes orientations corresponding to the direction of movement of the air, the line drawn by the style will be a miniature copy of the path of an imaginary particle animated by the movements actually belonging to the masses of air which successively affect the anemometer at the given station during the selected period, rigorously in accordance with the principle known as Lambert's. But in order that the trace drawn as described should correctly represent the actual movements of the air, it is evident that the whole mass of the atmosphere must be supposed to move "parallel to itself," *i.e.* in such a manner that the straight

line joining any two particles of air shall always be parallel to its original direction, an assumption which is manifestly incorrect. If I rightly understand the description of Mr. Wilson's integrator on p. 467, the trace given by it is precisely that which has just been shown in the case of my own machine to be based on a fallacious assumption. But though the *trace* may be useless, the summation of the movements of the table above described gives results which are representative of physical realities, being in fact the quadrantal components of the wind-movement at the station during the period dealt with by the machine. I trust that the preceding remarks will suffice to justify the statements contained in my last letter. Dr. von Oettingen's remark, referred to in my concluding sentence, related, not to his wind-component-integrator, but to the continual change of form in what may be called the *physical* Lambert's line, and implied the consequent advisability of discarding Lambert's method of treatment.

CHARLES E. BURTON

38, Barclay Road, Walham Green, S.W., October 14

P.S.—On September 21 last I forwarded to Prof. Stokes a description, with drawings, of two forms of wind-component integrator, suitable either for attachment to a cup and vane-anemometer, or for the reduction of existing anemograms of the pattern adopted by the Meteorological Office; and of simpler mechanism than my earlier machine, or Dr. von Oettingen's.

Calabar Bean as a Preservative

As many find such a difficulty in preserving entomological and other natural history specimens it may not be uninteresting to your readers to have a brief note on the use of Calabar Bean as a preservative. About eight years ago, when Aquilla Smith, M.D., Professor of Materia Medica, Trinity College, Dublin, was showing me through the museum that he has rendered so famous, I was struck by the perfect manner in which the specimens were preserved; the little brown beetle that is generally such a pest in similar collections being entirely absent. Dr. Smith told me that he treated the specimens with tincture of Calabar Bean, and very kindly gave me a bottle of the tincture. I used the tincture freely in my cabinet of Lepidoptera, and, although the collection has been wofully neglected since, it has remained quite free from mites. Dr. Smith tells me that the tincture was prepared by Mr. Squire of 277, Oxford Street, London, its strength being one part of the bean to eight of (rectified?) spirit. I might mention that Mr. Fetherstonhaugh used some of the tincture which I gave him in his cabinet, and was delighted with its action. A drop of the tincture is placed on the body of the insect. I found it a good plan to do this whilst the insect was on the drying board, as otherwise, in newly set insects, the damping with spirit caused the wings to spring.

E. MACDOWEL COSGRAVE

A Correction

I FIND that the term "glissette" is not used precisely in the sense which I had supposed. A reference to Mr. Besant's "Notes on Roulettes and Glissettes" (which I had not before me last week) shows that the envelopes of the moving lines, to which the theorem in my last letter refers, would be properly described as *roulettes*. It is obvious, however, that glissettes are in general also roulettes.

GEORGE M. MINCHIN

Royal Indian Engineering College.

Effect of Green in Painted Windows

I NOTICED to-day a curious effect in the east windows of Old Upton Church which may interest artists among your readers, and of which I should be glad to see any explanation. The pattern is in small regular pieces in which a strong red is prevalent, especially in the ribbon round the edge. Green is perhaps the least represented in area. At all events, generally, red largely prevails over green. The latter is not over brilliant. At a distance of ten feet the general effect is red. At that distance I see the pattern sharply, and green is not at all obtrusive. At the length of the church, say fifty feet off, I cannot distinguish the pattern, and the whole window looks a thin watery green haze; the bright red margin is inappreciable.

Richmond, October 12 W. J. HERSCHEL

THE AUTUMN MEETING OF THE IRON AND STEEL INSTITUTE

AT the meeting of the above Institution, which has just taken place, several papers of scientific and practical interest were read and discussed. They may be broadly divided into two classes, viz. 1st, those relating to the production of iron and steel, from the ore, and the qualities of the material when produced; and 2nd, the various applications to which steel has been put in recent times. The latter class of papers, at the recent meeting, dealt principally with the use of steel in the manufacture of ordnance, small arms, projectiles, and gun-carriages, and the papers, some of which were of great interest, will be reserved for consideration in a separate notice. Amongst the papers dealing with the manufacture of steel we may notice specially a memoir by Herr Paul Kupelweiser of Witkowitz, in Austria, on recent progress attained in the use of the basic process at the works with which he is connected. This process, which has been frequently referred to in NATURE, seems—probably on account of the quality of the ores met with—to have been adopted more frequently in Continental steel works than in our own country, for according to Herr Kupelweiser's summary, no less than thirty works in France, Belgium, Germany, Austria, and Russia, have acquired licences under the Thomas patents, the greater number of these being already at work; while the remainder are adapting their old plant, or erecting new works with the view to its immediate introduction. The weak point of the process hitherto has undoubtedly been the want of durability in the refractory linings of the converters, and on this point the author states that, in spite of numerous trials with other materials, the works with which he is acquainted still use the materials originally proposed by Mr. Thomas, viz. either the basic bricks or the shrunk lime and tar mixture. At Witkowitz, however, a new material has been used containing a comparatively small percentage of silica, and the quality of the bricks manufactured from this has been found to be materially improved. Ground brick mixed with 5 to 10 per cent. of tar is also used at many works for lining as well as for repairs. Basic tuyères have been tried in many places, but are not commonly used; but the author states that magnesia obtained by precipitation from chloride of magnesia by milk of lime appears, from experiments made on a small scale, to be a promising material for making tuyères. As regards the quality of the steel he makes the following remarkable statement:—"The basic process, as regards the quality of its products, is not only completely equal to the acid process, but even, in my opinion, superior to the latter." As a specimen of the excellent quality of the mild steel manufactured at Witkowitz the author exhibited a locomotive boiler tube made of this material, which had been expanded cold by means of a tube expander from 9 to 17 millimetres, on an original diameter of 48 millimetres, equal to an extension of from 20 to 36 per cent. on the periphery of the material, without even splitting at the line of weld.

Another paper of great interest to foreign manufacturers was Prof. Tünner's memoir "On the Use of Lignite or Brown Coal in the Blast Furnace." It is well known that the Austro-Hungarian Empire contains immense deposits of this fuel. It would be difficult to over-estimate the benefit which would accrue to the iron industry of Austria if this abundant and inexpensive fuel could be used successfully in the blast-furnace. All the experiments made in this direction till last year were of a more or less isolated and unsatisfactory character. In June, 1880, however, the "Mining and Metallurgical Association of Styria and Carinthia" appointed a committee to investigate the subject afresh. This committee has not yet reported, or indeed concluded its labours, but it is satisfactory to learn that it is fully acknowledged that there is no theoretical difficulty in the way of smelting

iron with raw or coked brown coal, and that the practical difficulties have been partly solved; for we learn that at Kalan a blast-furnace was for a time worked with a mixture of from 25 to 75 per cent. of brown coal, and 25 to 75 per cent. of coke. The great difficulty in the utilisation of this fuel lies in the fact that, owing to the high percentage of contained water, the raw coal is liable, when heated, to splinter up into small pieces, somewhat similarly to anthracite, and the coke formed of it is also very small and tender. It is, however, satisfactory to learn from Prof. Tünner that these difficulties may be in a measure obviated by the use of a strong blast, and especially constructed furnaces. The chief difficulty arises in continuing the operation when sponge-iron is produced; but it is suggested that the reduction might be completed from this stage in a small furnace, such as a Siemens furnace with coked fuel.

The results of the further labours of the Committee will be awaited with great interest.

Mr. G. J. Snelus of Workington contributes a paper on the Distribution of the Elements in Steel Ingots. It was till quite lately taken for granted that the steel plates, &c., produced from ingots were not only mechanically, but chemically homogeneous. When the disastrous failure of the boiler plates of the *Livadia* took place, this subject, amongst many others, was minutely investigated, and samples of different portions of the plates were submitted to chemical analysis, with the startling result that the proportions of carbon, manganese, phosphorus, and sulphur were found to vary greatly. At the spring meeting of the Institute Mr. Stubbs announced, during a discussion, that he had discovered that during the solidification of the ingots a redistribution of the elements took place, the carbon, sulphur, and phosphorus going to that part which remained fluid the longest. Mr. Snelus has now by experiment confirmed this statement so far as large ingots are concerned. This fact is brought out most clearly in the following table, which gives the analysis of carbon, sulphur, and phosphorus, of six samples taken from a slice 21 inches below the top of an ingot, measuring 7 feet × 19″ × 19″, and a similar number from a slice 4 inches above the bottom; the number in each case being taken from the outside, number 6 from the centre, and the remaining numbers from intermediate positions:—

No.	Top.			No.	Bottom.		
	C. carb.	Sulphur.	Phosph.		C. carb.	Sulphur.	Phosph.
1.	·44	·032	·044	1.	·44	·048	·060
2.	·54	·048	·060	2.	·42	·056	·062
3.	·57	·080	·086	3.	·41	·048	·054
4.	·61	·096	·097	4.	·40	·048	·054
5.	·68	·120	·111	5.	·38	·048	·058
6.	·77	·187	·142	6	·37	·044	·052

In examining smaller ingots, however, Mr. Snelus found that the metal was practically homogeneous, and consequently the want of uniformity in the *Livadia's* boiler plates cannot be accounted for in this way, seeing that they were produced from relatively small ingots.

Mr. Edward Richards gave an account of a series of experiments on the strength of samples of mild steel. The specimens were tested both for ordinary tensile strength, and also for the tensile strength after the samples had been submitted to long-continued tensile and compressive strains approaching the elastic limit to torsional strains, and to long-continued strains below the elastic limit. He also made experiments on the strength of samples of plates which had been perforated. The results are of great interest, and will well repay careful study, though they are too voluminous to be reprinted here. We may however notice that in one sense these experiments go to support the much-combated opinion held by Dr. Siemens, that any mechanical treatment to which mild steel is subjected, has invariably the effect of increase of strength.

THE "QUARTERLY REVIEW" ON EARTHQUAKES

THE pages of the *Quarterly Review* constitute perhaps the very last place in which one would look for a new theory on an important scientific question, and the perusal of an article in the July number of that journal on "Earthquakes: their Cause and Origin," has left us in grave doubt as to whether the author of it is writing seriously or is perpetrating a gigantic practical joke.

The article professes to be a review of the well-known and valuable works of Schmidt, Heim, and Mallet on Earthquakes; but added to this list of books for review we have "Scepticism in Geology, and the Reasons for it, by Verifier"! When we find that a considerable portion of the article is occupied with passages quoted from this last-mentioned book, in which the most absurd misconceptions and misconstructions of the writings of Lyell, Darwin, Huxley, and others are embodied, we can scarcely forbear from leaving the task of framing an hypothesis concerning earthquakes, in order to indulge in conjectures as to the relations which may possibly exist between "Verifier" and the author of the article in question.

Ignoring the whole body of facts which have been accumulated by seismologists concerning the amplitude, direction, and velocity of earthquake-waves, the author denies that the earthquake movements are waves at all; and in his reasoning (if such it can be called) he hopelessly confuses the vibration with the shock which has produced it. Dismissing with contempt the views of others on the subject, he proceeds to offer his own conjecture as to the cause of earthquakes. It is no other than our old friend electricity, written with capital letters. Some well-known examples of electrical discharges taking place from portions of the earth's surface are adduced, and it is then naïvely assumed that such discharges of terrestrial electricity would produce the effect of an earthquake. The undulatory movements are supposed to be the result of a struggle of the electricity to break through cushions composed of soft, non-conducting materials, and the cracks and chasms opened in the soil to the power of the "electric jet" to rip asunder the surface.

The facts on which this extraordinary theory (or "conjecture," as the author very properly terms it) appears to be based are of two kinds. In the first place it is noticed that peculiar atmospheric and electrical disturbances have occurred at the same time as earthquakes. In the second place Dr. Schmidt is quoted to prove that the earthquake shocks which he had studied in Greece had very commonly a course from north-east to south-west. The author adds to this the fact that an earthquake-wave occurring in the United States in the year 1870 took the same direction. He then asks triumphantly, "Is not this the line of path habitually followed by electric currents?"

Excited beyond all bounds by this supposed discovery of the true cause of earthquakes, our author then proceeds to make a number of suggestions which are certainly rather sensational than practical. To the Society of Telegraphic Engineers he appeals to invent a conductor which shall ward off the electric currents and divert earthquakes from their habitual haunts. Medical men are requested to examine the bodies of people killed during earthquakes in order to discover "lightning-scars." And lastly, Sir William Thomson and other eminent electricians are asked to "direct their attention to that storehouse of unlimited energy already filled within the bosom of the earth," and to utilise it for useful purposes.

This curious article may at least serve one useful purpose. Its author is evidently a man of some general knowledge and considerable culture, and the absurd errors into which he has fallen are manifestly the result

of his never having received any proper training in the rudiments of science. If the appearance of this article serve to call the attention of the managers of our public schools, and others interested in education, to the painful consequences which may result from the want of a preliminary grounding in the facts of science and the principles of scientific reasoning—then we think it will not have been written in vain.

THE STORM OF FRIDAY, OCTOBER 14

THIS great storm, which appeared so suddenly, sped its course over North-Western Europe so rapidly, and involved so wide a region in its destructive violence, will be long remembered for the well-nigh unparalleled loss of human life which it has occasioned among our fishing population between the Forth and the Tweed. For some days previously atmospheric pressure had been low to the north of the British Isles and high to the south, the difference from north-west to Land's End being about an inch, thus giving steep gradients, and resulting strong west and north-west winds, and stormy seas along the west coast; and as the area of low pressure moved very slowly eastwards the weather conditions continued with some persistence substantially the same. At length on Thursday morning the daily weather charts showed that a change had just begun in the extreme south-west of Ireland, at Valentia, where, and where only, with a barometer beginning to fall, the wind had changed to a southerly direction, but everywhere else in the British Islands it remained north-westerly; whilst at the same time the area of high pressure to the south was advancing from France to Spain, indicating that the path of the coming storm would take a more southerly course. By 2 p.m. the area of a falling barometer had spread eastwards, and the wind changed to south-west as far as Holyhead; and by 6 p.m. observations showed the continued rapid easterly advance of the storm, the wind being now southerly or south-westerly at all the telegraphic stations except Nairn, where it was west-north-west, showing that Nairn was still within the influence of the slow-moving depression to the north.

High winds and very heavy rains occurred during the night over the northern half of Great Britain, and on Friday morning the weather charts showed that North-Western Europe was involved in a storm of great intensity, the centre of which had now advanced as far as Midlothian. Gradients were steep all round the low centre of pressure, and consequently gales and storms of wind prevailed in all parts and in all directions over the British Islands, being west over France and the south of England, south-west and south over the north of England and the North Sea, north-east in the northern half of Scotland, and north-west in Ireland. From the barometric readings published in the *Times* it is seen that the lowest reading occurred in London about 8 a.m., and, in accordance with the isobars on the Weather Chart, the lowest reading occurred in Edinburgh at the same hour. In London, which was some distance from the centre of the storm, the lowest barometer was only 29·086 inches, but in Edinburgh, over which the centre passed, pressure fell to 28·425 inches, which was an inch lower than it was twelve hours before. After 8 a.m. a rapid recovery of pressure set in; the most rapid rise of the barometer in London was 0·214 inch in the two hours from 4 to 6 p.m., and 0·163 inch in the two hours immediately following. In Edinburgh the increase proceeded at a much more rapid rate, beginning with 0·018 inch, from 8 to 9 a.m., and increasing gradually to 0·166 inch from noon to 1 p.m., and 0·150 inch from 1 to 2 p.m., after which it rose less rapidly, and continued to do so at a steady, though greatly diminished, rate for two days till Sunday at 10·30 a.m., when the barometer stood at 30·370 inches, having thus risen nearly two inches in little more than forty-eight hours.

On Saturday morning the centre of the storm had advanced fully 600 miles to eastward, being at the high daily average of 25 miles an hour, and was now near the south-west angle of Lake Wener in Sweden. Here the lowest barometer was about 28·600 inches, whilst at the same time to westward at Valentia pressure had risen to 30·220 inches, thus giving for the southern shores of the North Sea steep gradients for north-west winds, which, with the high seas they raised, proved very destructive to those coasts.

The anticyclone indicated by the high barometer following in the wake of the storm was accompanied with temperatures unusually low for the season during the night of Saturday–Sunday, when temperature fell to 27°·0 at Parsonstown and Nottingham; 29°·0 at Ardrossan; and 32°·0 at Leith, Shields, Cambridge, Oxford, and Mullaghmore. Snowfalls of some depth occurred in many districts, doing no little damage to green crops, and in later districts to grain crops still standing in the fields.

The lamentable destruction to fishing-vessels off the coast of Berwickshire was doubtless to no inconsiderable extent due to the deceptive character of the weather on Friday morning in cases where the barometer either is not consulted, or such a fall as an inch during the twelve hours immediately preceding, is discredited as a precautionary warning. In Midlothian, shortly after eight o'clock, the clouds broke up and the sun shone in a sky rapidly clearing of clouds. Soon, however, a change commenced, and within an hour, behind a low bank of darkish-looking clouds in the northern horizon, a long bank of ashy, leaden-hued, ominous clouds began to appear, and rose higher in the sky. In a brief space of time the whole of the sky was overcast, and a darkness quickly followed so great as to render gas necessary in reading the morning newspaper. It was remarked at the time that the darkness lasted three or four times longer than is usually the case with the darkness which is observed immediately to be followed by a complete change of wind. When it passed away, the wind had changed from south-west to north-north-east and the temperature fallen, and thereafter the wind gradually rose to a gale. On the other hand, off the Berwickshire coast the darkness was denser and more threatening, and almost simultaneously with its approach a hurricane broke out with a devouring energy which bore everything before it, and, explosively as it were, instantly rose to a height which, judging from actual facts related by the fishermen who escaped and the spectators on the shore, can perhaps only be paralleled in this country in recent years by the Edinburgh hurricane of January 24, 1868. On land many lives were lost in London and elsewhere, and in all parts of the country chimney-stacks, roofs, and walls were blown down, telegraph lines were wrecked, and tens of thousands of the finest trees were snapped asunder and levelled with the ground. When there has been time to collect the records of this storm, it will be found to have been one of the most destructive to life and property in these islands in the memory of the present generation.

THE INTERNATIONAL EXHIBITION AND CONGRESS OF ELECTRICITY AT PARIS[1]

IV.

AS we believe our readers will be interested in a fuller description of the arrangements for the telephonic hearing of the Opera than we have yet given, we extract the following from Nos. 50 and 51 of the new and popular French electrical journal, *La Lumière Électrique*, edited by Count Du Moncel. It is from the pen of the Count

[1] Continued from p. 564

himself, and is, we believe, the best account yet published. Our own additions are in square brackets.

One of the greatest successes of the Exhibition of Electricity is certainly the arrangement for the telephonic hearing of theatrical performances which has been organised by the Société Générale des Téléphones. By means of an electrical connection which has been established between the Opera, the Théâtre Français, and the Exhibition Palace, it has become possible to hear in the most complete manner the pieces played on our two principal dramatic stages. The singing of the Opera especially has a fairy-like effect, and all who have been so fortunate as to penetrate into the sanctuaries reserved for these hearings go away astonished and enchanted as if they had come from a dream of the Thousand and One Nights. The effect is in fact captivating; for the singing of the Opera is positively better heard in this way than in the Opera House itself; the words are more distinct, and the delicate turns of sound are better rendered, in consequence doubtless of the fact that the telephonic transmitters, being interposed between the actors and the orchestra, the instrumental music is to some extent sacrificed to the singing. It would scarcely be an exaggeration

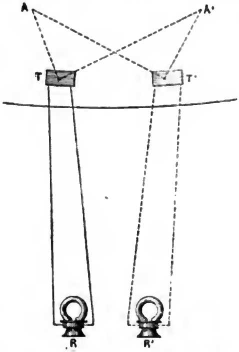


Fig. 1.—Stereoscopic combination of two transmitters.

to say that we hear too well, for the words of the prompter are heard with a distinctness which is somewhat distracting. In short the success is complete, and the Société des Téléphones, as well as M. Ader, who has superintended the arrangements, have a large claim on public gratitude.

The theatrical performances are to be heard not only in the four rooms allotted to the public, but also in a little boudoir off the so-called Salon de l'Impératrice, which is at some distance from the others. Here there are no external noises to distract, and it is possible to enjoy the charming melodies at one's ease.

The four public rooms, one of which is represented in Fig. 3, are covered on all sides with drapery to deaden external sounds, and the telephones are hung in pairs from wooden panels, of which there are twenty in each room. Chandeliers supporting lamps of Swan, Maxim, and Lane-Fox light each of these rooms, and the entrance to each is through a kind of pen which holds twenty persons in single file. The auditor has only to put the two telephones to his ears to hear the theatrical performance. On the table in the centre of the room there is a telephone for the use of the officials.

The connecting wires pass across the northern portion of the galleries of the Exhibition, and thence through sewer pipes to the Opera House and Théâtre Français, where they abut on the stage of each of these theatres, and terminate in transmitters, which are in fact microphones with multiple contacts. Those which are employed are of the Ader system, and are the same as those which are used for the Telephonic Exchange of Paris, but with one slight modification to meet the exigencies of this special purpose. As is well known, the acting portion in these transmitters consists of a sort of gridiron composed of two fixed parallel bars of carbon loosely connected by means of six smaller cross-bars of the same material, whose ends are supported by resting in holes in the sides of the large bars, and are free to rattle about in them. This frame of carbons is attached in a horizontal position to the under side of a thin board of pine, which is the vibrating plate. [Sometimes the gridiron is doubled, so as to consist of three fixed bars, the middle one being connected to each of the others by five small bars (ten small bars in all). The terminals of the battery are in both cases connected with the two outside bars, and the current divides itself between the five small bars as it passes from each large bar to the next. An ordinary

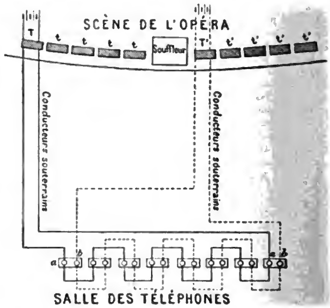


Fig. 2.—Connections between Opera and hearing-room.

five-barred gate, without the sloping tie, gives a good idea of the general form. It is one of the many forms which have been given to Crossley's microphonic transmitter, and is believed to be about on a par with many other forms.]

On account of the multiplicity and variety of sounds which it is capable of transmitting, it has been fixed on a leaden socket standing on four india-rubber feet, to deaden the effect of the vibrations of the floor of the stage. The necessity for such an arrangement is obvious in a theatre where the floor is continually being shaken by dancing.

At the Opera House there are ten transmitters of this kind disposed on both sides of the prompter's box along the edge of the stage. The arrangements suitable for any theatre vary according to the position and magnitude of its stage; and M. Ader informs us that he intends to double the number of transmitters at the Opera House, so as to render the sounds still more intense.

The receivers in the telephonic rooms of the Exhibition are the *téléphones à surexcitation* of M. Ader, which we have described more than once in this journal, and the arrangement of the batteries which work these multiple systems possesses no special feature. The batteries are

placed wherever there is room for them, generally underneath the stage; but as they would become too highly polarised if left in circuit during a whole representation, it is necessary to renew them every quarter of an hour, and for that purpose a commutator has been provided which allows the change to be made in a moment. This comutator consists of a board furnished with as many spring-plates as there are transmitters, and serves to keep up the connection between the transmitters and the batteries.

The greatest difficulty that has been met with has been how to render the transmitter more sensitive to the voices of singers than to the loud sounds emitted from the instruments of the orchestra, which would otherwise predominate. M. Ader has had to make a number of acoustic studies and trials on this point, which we cannot explain without going too much into detail, and has completely vanquished the difficulty; we will, however, explain the means which M. Ader has employed to enable the auditor to follow to some extent the movements of the actors on the stage.

Everybody knows the stereoscope, which enables a person, by means of the superposed visual impressions of the two eyes, to see the stereoscopic images with their natural relief. M. Ader applies the same principle to the perception of sounds.

Suppose two microphonic transmitters, placed on the stage at T and T′ (Fig. 1). Let one of them be connected by wires with the receiver R, and the other with the receiver R′, these receivers being applied to the two ears of the auditor, and suppose an actor to stand first at A and then at A′. In the former position, as he is nearer to the transmitter T than to T′, his singing will be heard loudest with the left ear; but when he is at A′ he is nearer to T′ than to T, and the right ear will receive the strongest impression. Thus as he goes from A to A′ the definite sensation which the auditor will receive will be that of a diminution of loudness in the left and an increase of loudness in the right ear, which is the same as the sensation which we experience when a person who is speaking walks from our left to our right. The same principle will apply to a number of actors crossing one another on the stage. Fig. 2 explains how this idea has been carried out at the Opera House.

FIG. 3.—One of the hearing-rooms in the Exhibition.

We have already stated that there are five transmitters on each side of the prompter's box [marked *souffleur* in the figure] along the edge of the stage. Each of these transmitters has its own separate circuit, and consequently its own separate underground cable. On arriving at the hearing room, the cables terminate each in eight receivers, but always in such a manner that to each auditor the effects are very different for his two ears. Fig. 2 shows the course of the circuits for two trans-

mitters, and the circuits for the others are arranged on the same plan. A little study of this figure is enough to show that in each pair of telephones in the receiving room the left one corresponds to the transmitters on the left side of the stage, and the right one to the transmitters on the right side. All the left-hand telephones are in one circuit, and all the right-hand ones in the other. This arrangement, which is clearly shown in the figure, is very ingenious, and though a simpler arrangement with a smaller number of circuits could have been employed, the additional expense involved in having so many circuits was not allowed to stand in the way, as it was necessary in order to make forty-two pairs of telephones work properly; and even this number is not sufficient to satisfy the public curiosity.

These brilliant experiments show that there was no exaggeration in the statements which were published soon after the invention of the telephone, to the effect that concerts and sermons had been heard at great distances. It cannot now be said that we were too credulous when we announced in 1878 that the opera

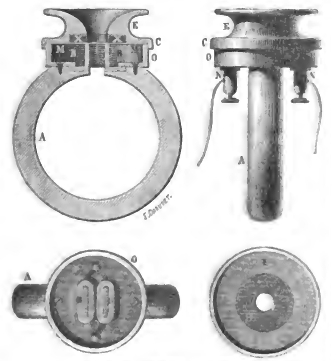


FIG. 4.—Ader's telephone *à surexcitation*.

"Don Pasquale" had been very well heard in the telephone at Bellinzona, and that none of the fine turns of this charming music had been lost. We believe indeed that the results then obtained were far inferior to those which we have now the opportunity of enjoying; and still more marvellous results in telephony have, we are informed, been quite recently obtained.

Fig. 3 represents one of the hearing rooms, namely, that which is lighted with Lane-Fox lamps. A mahogany wainscotting is carried round the walls at about the height of the ear, and on it are fixed twenty small wooden panels furnished with hooks to hang the telephones on. The telephones are connected with the underground conductors by means of flexible wire cords which come out of the wainscotting, so that nothing is easier than for the auditors to put the telephones to their ears. As the telephones are connected, eight in each series, with one and the same pair of microphonic transmitters, and the different pairs of transmitters occupy different positions on the stage, the effects are not the same for all the telephones. Those which are in connection with transmitters at the extreme right or left are more affected by the sounds of the loud instruments in the orchestra than those which are connected with the transmitters nearest the prompter; but these latter, on the other hand, are more affected by the prompter's voice.

To make the effects as equal as possible, M. Ader has so arranged the connections that the two transmitters which form one pair are in precisely opposite conditions; for instance, the transmitter at the extreme left is paired with the first to the right hand of the prompter, the second from the extreme left with the second to the right of the prompter, and so on. The best effect is obtained from the pair which occupy the middle places in the two sets. These differences give an obvious explanation of the different accounts given by different persons of the predominating sounds which they have heard, and explain why many of them, having heard in different parts of the same room, have not received the same impressions. Naturally enough they attribute the difference to the quality of the telephones, but though it is possible that some of these may be better than others, it is to the positions of the transmitters on the stage that the differences are chiefly attributable.

[The receiving instrument, which, as above stated, is the *telephone à surexcitation* of M. Ader (pronounced like the English name Adair), is very similar to the Gower-Bell receiver, having like it a steel horseshoe magnet, which forms nearly a complete ring, and which, being coated with nickel, serves as the handle. Round the two soft iron pole-pieces of this magnet coils of very fine wire are wound, which are in circuit with the line wires, so that the currents from the transmitter at the sending-station pass through them. A thin circular plate of iron, fastened by its edges, is fixed at a very small distance from the pole-pieces, and serves as the vibrating diaphragm. The peculiarity of the Ader telephone is that a flat ring of soft iron is fixed at a little distance behind this vibrating plate; that is to say, on the side remote from the magnet, its office being to concentrate and intensify the force of the magnet upon the diaphragm. This is what is meant by *surexcitation*. The plate, in fact, is more strongly attracted by the magnet than it would be if this ring were absent. In Fig. 4 (which consists of three sections and one elevation of this telephone) A is the steel magnet, B B are the coils, M M is the vibrating diaphragm, X X the flat ring for intensifying the force of the magnet upon the diaphragm, O the resonance chamber, and E the trumpet-shaped opening which is applied to the ear.]

The following account of Rysselberghe's meteorograph was accidentally omitted from a previous article:—

One of the neatest specimens of electrical mechanism is the meteorograph of M. Van Rysselberghe, exhibited by the Royal Observatory of Brussels. It gives its records not only at the place of observation, but at one or more distant stations, and is now giving every night at Paris a record of the indications of the instruments at Brussels. Once every ten minutes it comes into action and registers one after the other the six following elements:—(1) temperature; (2) humidity; (3) water in rain-gauge; (4) direction of wind; (5) barometer; (6) velocity of wind. It also makes a mark about every half second due to the action of clockwork at the sending-station.

The registration is made by a diamond point on a thin plate of zinc which is bent round the surface of a revolving cylinder, and which is covered with lamp-black to make the marks more visible. This plate serves afterwards for printing any number of copies. There may be several of these cylinders at as many different stations, all receiving simultaneously the indications furnished by any one station. The mode of action is as follows:—

Let us take for example the case of one of the thermometers. The thermometer-tube is vertical and open at the top. A long metallic probe smaller than the tube of the thermometer descends once in ten minutes with a

slow motion produced by clockwork. The probe, by touching the mercury, completes a circuit, through which a current is instantly transmitted from a local battery. The line-wire is included in this circuit, and a corresponding movement is produced in the diamond point of the receiving instrument. A local electro-magnet is also made by this current, and the arrangements are such that the current is thus diverted from the mercury at the instant after the probe has touched it, and there is consequently no spark when the probe leaves the mercury. The instantaneous current which thus passes is always from the probe to the mercury; in other words the mercury is the negative and the probe the positive terminal. If any moisture be present its oxygen goes to the probe (which is of platinum) and the hydrogen to the mercury, which thus, instead of oxidising, is kept always bright. Evidently the higher the mercury stands in the tube, the sooner will the contact be made, and thus the scale of equal parts before-mentioned gives the height of the mercury.

The diamond point makes a succession of short marks which (in virtue of a mechanical interruption) form a regular series up to the moment when the probe touches the mercury, after which they cease for several seconds. The cylinder revolves once in ten minutes, and the diamond point has at the same time a slow longitudinal motion (being mounted on a screw axle), so that the successive indications of the same thermometer form a nearly continuous curve (traced by points).

Thus by one line wire and one diamond point the curves for all the six instruments are drawn at a station which may be 200 or 300 miles distant. The value of such an instrument for furnishing the director of a central station with accurate data on which to base his weather-predictions speaks for itself; and as regards expense, all the expenses of photography and of reducing and engraving photographic traces are saved. It has been worked in Belgium over a wire of the length of 750 miles.

(*To be continued.*)

NOTES

The Royal Institution Session will commence with a course of six lectures on astronomy, adapted to a juvenile audience, by Prof. R. S. Ball, F.R.S., Astronomer-Royal in Ireland. Dr. W. Huggins will give a discourse on Comets at the first Friday evening meeting, January 20, 1882.

The International Commission for the next transit of Venus, established in Paris under the presidency of M. Dumas, has accomplished its work and published a series of instructions, which will appear in the next number of the *Comptes rendus* of the Academy of Sciences, and be sent to all astronomers and observatories. A complete scheme for international co-operation has been adopted.

As No. 12 of the Bibliographical Contributions, edited by Mr. Justin Winsor of the Harvard University Library, we have a List of the Publications of Harvard University and its Officers, 1870–80. It contains, for example, the publications of the Astronomical Observatory, the Bussey Institution, the Museum of Comparative Zoology, &c., followed by an alphabetical list of the officers (professors, &c.) of the University with their publications, and including such names as those of Agassiz, father and son, J. A. Allen, the ornithologist, J. P. Cooke, professor of chemistry, Asa Gray, H. A. Hagen, professor of entomology, E. C. Pickering, professor of astronomy, the late Benjamin Peirce, S. H. Scudder, N. S. Shaler, J. Trowbridge, professor of physics, and others.

The experiments made at the Paris Opera in electric lighting have been successful for regulators. Not less than thirty-six Brush lamps illuminated the celebrated monumental staircase, with Werdermann in the circular gallery, and Jaspar in the buffet. Sixty-four Jablockhoff lights were disposed on the ceiling round the chandelier with success in spite of the numerous changes of colour. The incandescent light exhibitors—Swan, Maxim, and Edison—were not ready to act their part, and the opportunity was lost for them; a second will be given to-day.

A rumour has been spread by the *Journal Officiel* that the Electrical Exhibition will be closed on the 1st of November. The impending resignation of M. Cochéry is stated to be at the bottom of this semi-official attempt. But it is certain no alteration will be made in the original date of closing, except to extend the time granted up to December 1.

The death is announced, at the age of eighty-four years, of M. Dubrunfaut, a well-known French industrial chemist.

It is stated that M. Hervé-Mangon, director of the Paris Conservatoire des Arts et Métiers, has decided to resign his post in order to devote himself more entirely to politics, he having been elected recently as *député* for the department of La Manche. Probably he will be succeeded by Col. Laussedat of the Polytechnic School.

Prof. Haeckel has arrived at Vienna on his way to Ceylon.

In connection with the Museum and Library, Queen's Road, Bristol, the following syllabus of a course of nine lectures, on literary and scientific subjects, to be delivered during the winter, 1881–82, has been issued:—October 31, 1881, Clements R. Markham, C.B., F.R.S., Sec. R.G.S., the Basque Provinces of Spain; November 14, Prof. W. J. Solas, M.A., F.R.S.E., F.G.S., the Natural History of Volcanoes; November 28, Prof. S. P. Thompson, B.A., D.Sc., F.R.A.S., Electric Storage and Lighting; December 12, Prof. William Ramsay, Ph.D., F.C.S., Improvements in Iron and Steel Manufacture; January 23, 1882, Prof. Bentley, F.L.S., Epiphytic and Parasitic Plants, with some observations on the Life of other Plants; February 6, Ven. Archdeacon Norris, B.D., Canon of Bristol, Redcliffe Church: its Architecture and History; February 20, J. E. H. Gordon, B.A., the Leyden Jar; March 6, W. Saville Kent, Infusoria; March 20, Rev. A. H. Sayce, M.A., the Land of the Phœnicians.

Lieut. Friedrich Will will shortly undertake a thorough zoological-entomological investigation of the provinces of Bahia, Pernambuco, and Pianhy; he is sent by the Entomological Society of Stettin, the president of which is Dr. C. A. Dohrn.

We have received parts 1 and 2 of the first volume of the *Transactions* of the Seismological Society of Japan, containing an address on Seismic Science by Prof. Milne, together with papers by Messrs. Ewing, Wagner, and Gray, on various seismometric and seismographic instruments, and by Mr. Mendenhall on a determination of the Acceleration of Gravity at Tokio. The Society is to be congratulated on the numerous proofs of activity which it has already shown, and on the very valuable scientific work it is doing in this rather neglected branch of study.

A useful paper by Mr. W. J. Harrison, Science Demonstrator for the Birmingham School Board, on the Teaching of Science in Public Elementary Schools, has been issued by him in a separate form. He resumes all the reasons for science-teaching in schools in a clear and forcible manner, and gives some hints that might be of service to science teachers. In Birmingham, we believe, they are now endeavouring to obtain money for science scholarships, by which boys of merit will pass from the Board Schools to the great Foundation School there (King Edward's Grammar School), then to the Mason College,

and perhaps subsequently to some university. There are now 2000 children and 200 pupil teachers under science instruction in Birmingham, and the results so far have been most encouraging.

MAJOR-GENERAL MAITLAND, writing to the *Times* in connection with the Bordeaux Phylloxera Congress, makes a suggestion which appears quite worthy of attention. He believes that all the remedies hitherto applied or proposed are open to the reproach to which all empiric treatment of disease is obnoxious—viz. the attacking of a symptom instead of the essential root of the disease, and thus betraying a want of right apprehension of its true origin. "This, in my humble view," General Maitland says, "is to be attributed to exhaustion of the vitality of the plant, induced by unduly and unnaturally overtasking its productive powers. In this respect the phylloxera of the French vineyards bears a close analogy to the red spider of the Indian tea garden, to the leaf-worm of the Indian, American, and other cotton fields, and, in short, to parasitic growth wherever proving fatally destructive throughout the vegetable kingdom. The mode in which this law of nature, as it may be termed, operates, may be nnderstood by reference to the physiological paradox, 'Life dies; death lives.' Wherever the vitality of a plant is abnormally diminished by over-plucking, over-pruning, and unceasing inexorable demands to produce more, more, when nature demands rest and repose to recruit exhaustion, the sap, the plant's life-blood, becomes poor, sluggish, and enfeebled. Parasitic life is then evolved, and preys upon the little remaining life that injudicious culture has left the plant. If the above view in regard to the origin of phylloxera be accepted as an approximation to the truth, the remedy would seem to be self-indicated—repose. Give the vineyards rest."

AN extraordinary report of four large expeditions for Africa being organised in Brussels, was lately given in the *Pall Mall Gazette*, and has this week been reproduced by the *Daily News*. There is, however, absolutely no foundation for the statement.

THE *Colonies and India* states that the unusual spectacle of snow was seen on Table Mountain on August 16. Such an occurrence has been recorded only once since 1813, viz. in 1878.

THE first list of the honorary council of the International Electric Exhibition which is to be held at the Crystal Palace, comprises the following names of well-known men of science:—Mr. James Abernethy, President Institute Civil Engineers; Prof. W. G. Adams, F.R.S., Sir James Anderson, Prof. Ayrton, F.R.S., Sir Henry Cole, K.C.B., Mr. William Crookes, F.R.S., Capt. Douglas Galton, C.B., F.R.S., Dr. Gladstone, F.R.S., Col. Gouraud, Sir John Hawkshaw, C.E., F.R.S., Dr. J. Hopkinson, F.R.S., Prof. Fleeming Jenkin, F.R.S., Sir E. J. Reed, C.B., M.P., Mr. B. Samuelson, M.P., Dr. C. W. Siemens, F.R.S., Mr. W. Spottiswoode, President Royal Society. The following gentlemen will be the chief officers for the Exhibition: Manager, Major S. Flood Page; secretary, Mr. W. Gardiner; superintendent, Mr. P. L. Simmonds; assistant engineer for Exhibition, Mr. R. Applegarth, C.E.; clerk of works, Mr. W. Carr.

THE Programme of the Technological Examinations of the City and Guilds Institute for 1881-2 contains several new subjects and arrangements—improvements on previous programmes. The examination papers set for 1881 are interesting.

WE notice in the Russian journal, *Old and New Russia*, an interesting paper on M. Tyaghin's wintering at Novaya Zemlya, on hunting in that land, together with a good sketch of the bird life in the neighbourhood of the wintering place.

DR. GOBI, who has investigated during many years the flora of the White Sea, has published his researches in a separate work in Russian.

WE notice in a paper published in the *Annals* of the Spanish Society of Natural History (vol. x. 1881), that Don Fr. Quiroga observes that the numerous implements in Spanish museums which are usually described as nephrite are mostly made of fibrolite, this name having been given by Count de Bournon to a variety of sillimanite. Out of 115 hatchets which were considered as nephrite, and were found mostly during the geological survey of the provinces of Guadalajara and Cuenca, only one was of nephrite, whilst 111 were of fibrolite and three of jadite. The fibrolite is often found among the mica-slates of the provinces of Madrid and Guadalajara.

THE same volume of the *Annals* contains a paper, by Don S. Calderon of Arana, on the evolution of the earth.

A STRIKING instance of the activity of man in destroying forests may be shown by the following figures, which we find in M. Olshevsky's paper in the last issue of the *Isvestia* of the Rnssian Geographical Society. After having taken into consideration the surveys which were made in the province of Ufa before 1841, and the recent distribution of forests in that province, M. Olshevsky shows that the area of forests, which formerly was about 17,577,000 acres, has now diminished by at least 3,500,000 acres; although the population is still very sparse, that is, less than three souls per square mile, and it was yet less some time ago.

THE well-known publishing firm of A. Hartleben (Vienna, Pesth, and Leipzig) have recently published a little work by Heinrich von Littrow, "Carl Weyprecht, der österreichische Nordpolfahrer." It contains many characteristic reminiscences as well as letters of the late discoverer of Franz-Josef Land. It is a fitting and touching literary monument to a brave, energetic, highly-cultivated, kind, and modest man of science, whose useful career was unfortunately cut short so prematurely.

Auf der Höhe is the title of a new international review, edited by Leopold v. Sacher-Masoch, and published at Leipzig by Gressner and Schramm (London: Dulau). The first number (October) contains several interesting articles, though none of them scientific; among the list of contributors, however, we notice the names of several Continental men of science.

DR. KING's report on the Government Cinchona Plantation in British Sikkim for the year ending March last, shows a continued and highly satisfactory progress—a progress that has been made not only in the extended cultivation of well-known and established species, but also in the propagation of valuable and rarer kinds. Most satisfactory results are recorded of the species known as *Cinchona Ledgeriana*, one of the varieties of *Calisaya* which, as Dr. King says, is surpassingly rich in quinine, and which has derived its name from Mr. Ledger, a collector who brought the seed from South America. Regarding another valuable kind, namely, the plant yielding the Carthagena or Columbian bark, which is largely imported to this country from the northern part of South America, and of which four plants were sent to the Government Plantations from Kew in January, 1880, Dr. King says, "They arrived in good condition and during the year they were increased largely by cuttings. Propagation went on most favourably for some time, but later on in the year the young plants were severely attacked by the pest only too well known to gardeners as 'thrips.' The usual treatment was applied with vigonr, but in spite of this, when the year ended the six original plants had been increased only to sixty rooted plants and ninety partially rooted cuttings." Dr. King, however, further says that "every effort will continue to be made to increase the stock of this interesting species." Both the general condition of the plantation and the financial results are reported as satisfactory, and the results as gathered from the quinologist's report, which is appended, are also satisfactory, inasmuch as they show an increased manufacture of febrifuge and also an increased demand. Dr. King and his co-workers

are to be congratulated on the continued successful results of their labours.

DR. OBST, the director of the Ethnographical Museum at Leipzig, after attending the Archæological Congress at Tiflis, intended to make an exploring tour in the Caucasus, Armenia, and Asia Minor, and then to return to Saxony *viâ* Constantinople and Athens.

A STRANGE phenomenon was recently observed at Emerson, near Lake Winnipeg. A dark cloud formed of myriads of winged black ants passed over the place from east to west. When it descended the ground over a large area was covered an inch deep with the insects.

MAUNA LOA (Hawaii) is again active, and the lava threatens the port of Hilo, situated on the east side of the island.

IN a letter which we have received from Mr. G. H. Kinahan he disavows the suggestion imputed to him (NATURE, vol. xxiv. p. 471) that Laurentian rocks occur in Co. Tyrone.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀), a Bonnet Monkey (*Macacus radiatus* ♀) from India, presented by Mr. G. E. Jarvis; a Vervet Monkey (*Cercopithecus lalandii* ♂) from South Africa, presented by Mrs. Brassey; two Leopards (*Felis pardus*) from Ceylon, presented by Lieut.-Col. J. S. Armitage, F.Z.S.; a Mesopotamian Fallow Deer (*Cervus mesopotamicus* ♀), two Beatrix Antelopes (*Oryx beatrix* ♀ ♀), two Arabian Gazelles (*Gazella arabica* ♂ ♀) from Muscat, presented by the Lord Lilford, F.Z.S.; a Naked-footed Owlet (*Athene noctua*), European, presented by Mr. R. J. Marlton; a Common Kestrel (*Tinnunculus alaudarius*), a Common Hare (*Lepus europæus*), European, presented by Mr. W. K. Stanley; a Paradise Whydah Bird (*Vidua paradisea*) from West Africa, presented by Mr. Bowyer Bower; two Bonnet Monkeys (*Macacus radiatus*) from India, a Bell's Cinixys (*Cinixys belliana*) from East Africa, deposited; an Osprey (*Pandion haliaetus*), European, purchased; a Hardwicke's Hemigale (*Hemigalea hardwickii*) from Borneo, received on approval.

OUR ASTRONOMICAL COLUMN

THE SATELLITE OF NEPTUNE.—We subjoin such a table as was suggested by Prof. Newcomb for indicating with little trouble the approximate position of the satellite of Neptune, at any time about the approaching opposition. The argument *u* has the same significance as in Newcomb's Tables:—

Argument *u*.		Angle of Position.		Distance.
°	°	°	°	″
0	180	73·3	253·3	11·4
10	190	65·7	245·7	13·1
20	200	59·7	239·7	14·6
30	210	54·8	234·8	15·8
40	220	50·4	230·4	16·6
50	230	46·3	226·3	16·9
60	240	42·3	222·3	16·9
70	250	38·2	218·2	16·4
80	260	33·8	213·8	15·5
90	270	28·6	208·6	14·3
100	280	22·2	202·2	12·7
110	290	14·0	194·0	11·0
120	300	2·7	182·7	9·2
130	310	346·4	166·4	7·7
140	320	324·2	144·2	6·8
150	330	299·4	119·4	7·0
160	340	278·6	98·6	8·0
170	350	263·7	83·7	9·6
180	360	253·3	73·3	11·4

Values of *u* at Greenwich noon

Oct. 28	187·55	Nov. 27	225·26	Dec. 27	262·95
Nov. 7	80·12	Dec. 7	117·83	Jan. 6	155·51
17	332·69	17	10·39		

Motion of *u* in

Days.	°	Hours.	°
1	61·26	1	2·55
2	122·51	2	5·11
3	183·77	3	7·66
4	245·03	4	10·21
5	306·28	5	12·76
		6	15·31
		12	30·63

From which figures *u* may be interpolated for any hour required. When *u* is found in the second column of the table, the angle of position is to be taken from the second column.

COMET 1881 *f* (DENNING, OCTOBER 3).—The comet discovered by Mr. W. F. Denning of Bristol during the night of the 3rd inst. has been observed at Marseilles by M. Coggia, and at Lord Crawford's Observatory at Dunecht. Elements calculated by Dr. Copeland and Mr. Lohse upon Dunecht observations on October 9, 10, and 12, are as follows:—

Perihelion passage 1881, September 12·0943, Greenwich M.T.

	° ′ ″	
Longitude of perihelion	22 6 9	M. Eq. 1881·0
Ascending node	72 47 45	
Inclination	7 45 12	
Log. perihelion distance	9·859822	

Motion—direct.

Hence it is found that this comet, like that discovered by Mr. Barnard on September 19, is receding both from the sun and the earth. As remarked in Lord Crawford's Circular, No. 33, the elements bear some resemblance to those of the fourth comet of 1819, detected by Blanpain at Marseilles, which was certainly moving in an elliptical orbit of very limited dimensions. This circumstance alone attaches a particular interest to Mr. Denning's comet, and makes it of importance that it should be accurately observed for position as long as practicable.

CERASKI'S VARIABLE—U CEPHEI.—The following Greenwich times of minima depend upon Mr. Knott's observation on the 2nd inst. with the period 2d·49280:—

	h. m.		h. m.		h. m.
Oct. 22	10 24	Nov. 11	9 1	Dec. 1	7 39
27	10 4	16	8 41	6	7 18
Nov. 1	9 43	21	8 20	11	6 57
6	9 22	26	7 59	16	6 37

BIOLOGICAL NOTES

THE HYPOPHYSIS IN ASCIDIANS.—In a second paper to the Belgian Academy on this subject (*Bull.* No. 6) M. Julin describes the quite special arrangement of the "hypophysary gland" in *Phallusia mamillata*. Besides the principal excretory duct existing in all Ascidians, and here considerably reduced, there are a large number of orifices by which the glandular tubes pour their product of secretion into the peribranchial cavity, of which the cloaca forms the median part, which receives all the products and residues of the organism, to be cast out. Hence the products of the hypophysis in this species are probably also excrementitial, and the gland is physiologically the kidney of the animal. If it be so with *P. mamillata* it is likely to be the same with the other Tunicata; and though, in most, the hypophysis opens into the mouth, one cannot infer that the product is to be utilised in the alimentary canal. From the morphological point of view it is noteworthy that in glands properly so-called, arising from an epidermic or epithelial invagination, the product of secretion is generally eliminated by a single orifice, and that the only exceptions occur in the category of urinary apparatus (Cestodes, Trematodes, &c.).

THE CORALS OF SINGAPORE.—We learn from a paper (*Proc.* of Berne Nat. History Society) by Prof. Studer, on the Corals of Singapore, that there are no less than 122 species known from this locality. Of these fifty-one species are special to the locality, whilst the others inhabit the seas of New Guinea, of the New Britannic Archipelago, of the Solomon Islands, and reach as far as Fiji, some few extending as far as Tahiti. At the same time the Singapore corals yield very few species in common with the Red Sea, the Seychelles, and Mauritius, and these are Fungidæ, but no Madreporaceæ. Thus it may be established that the coral fauna of the Indian Ocean must be divided into two distinct regions—a western and an eastern, the latter extending far to the east into the Pacific. These two

regions are divided by deep sea and by coast-lines, which, as the eastern and southern coasts of India, do not afford the necessary conditions for the development of corals, whilst the extension to the east is much facilitated by low grounds and favourable coast-lines. Nevertheless, however different as to the species which inhabit them, both regions have a close likeness as to certain species, and both might be considered as having formed a single region, probably at the time when the great plateau of the Sunda Islands was a continuation of the continent, and when Madagascar and Ceylon were in close connection. As to the inhabitants of greater depths and of colder water—as the Gorgonids, the Anthozoæ, and the Primnoids—the same species are widely spread throughout the Pacific and the Indian Ocean, showing thus that the differentiation of shallow-water forms goes on more rapidly than that of the deep-water ones.

A Chemical Difference between Living and Dead Protoplasm.—From various experiments (chiefly with protoplasm of plants, also with Infusoria) Herren Loew and Bokorny find (*Pflüger's Arch.*) that living protoplasm possesses in an eminent degree the property of reducing the noble metals from solutions, and that this property is lost when death occurs. "It may well be inferred," say the authors, "that the mysterious phenomenon denoted by the name of 'Life' depends essentially on these reducing atom-groups. In the present state of science we explain these 'groups in motion,' these springs of life phenomena, as aldehyde groups, but would by no means exclude some different and better mode of explanation."

Rattlesnake Poison.—Dr. Lacerda Filho has published the results of his experiments on the poison of the rattlesnake (*Crotalus horridus*) in the *Archivos do Museu nacional do Rio de Janeiro*, iii. 1. The poison of *Crotalus horridus* acts upon the blood by destroying the red-blood corpuscles, and by changing the physical and chemical quality of the plasma. 2. The poison contains some mobile bodies similar to the micrococcus of putrefaction. 3. The blood of an animal killed by the snake's bite, when inoculated to another animal of the same size and species, causes the death of the latter within a few hours, under the same symptoms and the same changes of the blood. 4. The poison can be dried and preserved for a long time without losing its specific quality. 5. Alcohol is the best antidote to the poison of *Crotalus horridus* known until now.

The Spermogonia of Aecidiomycetes.—According to recent observations by Prof. Rathay (Vienna Acad. *Anz.*) the spermogonia of Uredineæ or Aecidiomycetes may discharge their contents without the action of external moisture, of rain or dew (the only way, as apparently supposed by A. de Bary). The process may occur in dry and hot sunny weather, and as follows:—These spermogonia produce in their interior not only mucilage and spermatia, but also sugar. In virtue of the latter they separate water by "osmotic action," and this water causes the inclosed mucilage to swell, and thereby afford exit from the cavity. The author's observations were made upon the spermogonia of *Gymnosporangium conicum* and *Puccinia suaveolens*.

Pelagic Fauna of Gulf Stream.—Alexander Agassiz gives an interesting account of his explorations of the floating fauna of the Gulf Stream in the vicinity of the Tortugas. The party remained at this station for some five weeks, being allowed to select quarters at Fort Jefferson. Unfortunately during the greater part of their stay the strong northerly winds interfered greatly with the surface fauna. Had the south-easterly winds prevailed the fauna would have been driven against the Tortugas. The few favourable days showed, however, a wealth of pelagic animals which had been hardly anticipated, and which proved how excellent a station this would be to investigate the fauna from. It also has the immense advantage of supplying the naturalist, and at his very door, with not only the common species of reef-building corals, but with the varied invertebrate fauna to be found in such places. Leaving a full enumeration of the species for another occasion, in the letter we now notice (*Bulletin* of the Mus. Comp. Zoology, vol. ix. No. 3) A. Agassiz mentions in a general way the presence of a couple of species of Firoloidea, of Phyllirhöe, of several Appendiculariæ, of a small Pyrosoma, of a Doliolum, two species of Salpa, and half a dozen species of Pteropods. The number of pelagic foraminifera was greatly disappointing; not once was a species of Globigerina met with, and the Radiolarians appear to have also been scanty. A list of the Ctenophoræ, Discophoræ, Siphonophoræ, and Hydroids met with is appended by Mr. Fewkes. Many of the species are indicated as new.

Retarded Development in Insects.—In a paper by Prof. C. V. Riley, at the recent meeting of the American Association, the author records several interesting cases of retarded development in insects, whether as summer coma or dormancy of a certain portion of a given brood of caterpillars, the belated issuing of certain imagines from the pupa, or the deferred hatching of eggs. One of the most remarkable cases of this last to which he calls attention is the hatching this year of the eggs of the Rocky Mountain Locust or Western Grasshopper (*Caloptenus spretus*) that were laid in 1876 around the Agricultural College at Manhattan, Kans. These eggs were buried some ten inches below the surface in the fall of 1876 in grading the ground around the chemical laboratory, the superincumbent material being clay, old mortar, and bits of stone, and a plank side-walk being laid above this. In removing and regrading the soil last spring Mr. J. D. Graham noticed that the eggs looked sound and fresh, and they readily hatched upon exposure to normal influences, the species being determined by Prof. Riley from specimens submitted by Mr. Graham. Remarkable as the facts are, there can be no question as to their accuracy, so that the eggs actually remained unhatched during nearly four years and a half, or four years longer than is their wont; and this suggests the significant question, How much longer the eggs of this species could, under favouring conditions of dryness and reduced temperature, retain their vitality and power of hatching? Putting all the facts together, Prof. Riley concludes that we are as yet absolutely incapable of offering any satisfactory explanation of the causes which induce exceptional retardation in development among insects. The eggs of Crustaceans, as those of *Apus* and *Cypris*, are known to have the power of resisting drouth for six, ten, or more years without losing vitality, while in some cases they seem actually to require a certain amount of desiccation before they will hatch. Yet the fact remains that different species act differently in this respect. In short, nothing is more patent to the observing naturalist than that species, and even individuals of the same species, or the progeny of one and the same individual, act very differently under like external conditions of existence: in other words, that temperature, moisture, food, &c., influence them differently. Hence, as has been shown by Semper to be the case with other animals, so it is with insects, changes in the external conditions of existence will not affect the fauna as a whole equally, but will act on individuals. We can understand how this great latitude in susceptibility to like conditions may and does, in the case of exceptional seasons, prove beneficial to the species by preserving the exceptional individuals that display the power to resist the unusual change; but we shall find ourselves baffled when we come to seek an explanation of the cause or causes of such retardation, unless we accept certain principles of evolution. In the innate property of organisms to vary, and in the complex phenomena of heredity, we may find a partial explanation of the facts, for the exceptional tendency in the present may be looked upon as a manifestation through atavism of traits which in the past had been more commonly possessed and more essential to the species.

PHYSICAL NOTES

A singular case of the production of sound by natural causes is recorded by M. Reuleaux (*Proc.* of the Nat. Hist. Soc. of Prussian Rhineland and Westphalia). He observed it while hunting in the Röderbacherthal, near the highest point of the Rhine province. The ground is, in the main, gently undulating and densely wooded. The valley, spacious on the eastern side, narrows rapidly at one part to a sort of pass, through which, for about one kilometre, the Röderbach flows westwards. A south-west wind was blowing, and M. Reuleaux, coming along the hillside from the east, heard what appeared to be the strokes of a fine deep-toned bell in rapid succession. There was no such bell in the neighbourhood, and some other sounds soon heard satisfied him that the effects were of natural origin. Tones were heard growing in force to a maximum, then dying away; they were like those of organ pipes at first, but their "clang" came to resemble that of a harp or violin. At the mouth of the pass, whence the sounds seemed to radiate, there was a strange agitation in the air, and mixture of sounds, some of which abruptly stopped. M. Reuleaux supposes bodies of air in vortical motion (*trombes*) to have been carried along from the pass, and the sound to have been due to conflict between the outer and the inner air at the mouth of such *trombes*, producing oscillations. There was a marked difference of temperature between

the higher and the lower parts of the valley, and this is regarded as an important factor in the case; the cold air above pressing on the warm below, and closing the pass to a sort of tube. The wind seemed to be active only in the lower parts.

WITH the aid of delicate apparatus of recent invention Herr Grunmach (*Wied. Ann.* No. 9) has investigated the electromagnetic rotation of the plane of polarisation of radiant heat in solid and liquid substances (flint glass, plate glass, sulphide of carbon, oil of turpentine, distilled water, and alcohol). His finding is as follows:—1. In solid as well as in liquid diathermanous bodies there is such rotation, and always in the direction in which the current flows through the spiral or circulates round the magnetic core. 2. The amount of this rotation is, *cæteris paribus*, very different for different substances; the rotation is greater the greater the index of refraction of the substance. 3. With direct action of a galvanic current conducted round the diathermanous body, the amount of the rotation is proportional to the intensity of the current. 4. In a diathermanous body placed between the poles of an electromagnet, the amount of rotation is proportional to the magnetic force acting on the body. 5. The amount of rotation increases with the length of the substance traversed by the rays; but the relation between these two quantities could not be numerically determined.

EXPERIMENTS on heat-conduction have been lately made by M. Christiansen of Copenhagen (*Wied. Ann.* No. 9), by the following simple method:—Three round copper plates are placed one above another, separated by small pieces of glass. A hole is bored radially into each plate, and a thermometer bulb inserted in each hole. The lowest plate rests on a brass vessel, through which cold water is conducted, and on the top plate rests a brass vessel with circulation of warm water. Through holes in the two upper plates (supplied with copper stoppers) the intervals between the plates may be filled with liquid. M. Christiansen experimented first with air, and he proves that its heat-conduction increases with the temperature. The ratio of the conductivity of air to those of several liquids was next studied, the liquid being placed in the lower interval. The results agree well with Weber's figures for absolute conductivity. Some experiments were also made with plate glass (dry and wet) and marble. The method may be adapted (the author points out) to measurement of electric resistances, the potential being measured instead of the temperature.

AMONG some interesting experiments with liquid films, described by M. Plateau to the Belgian Academy, is one in which fine iron wire is first bent to represent a six-petalled flower in outline; the circular centre being supported on a small fork stuck in a piece of wood. The wire is slightly oxidised with nitric acid. The flower is dipped in glyceric solution, and is then put under a bell jar near a window, so that the sky is reflected in the films. A pretty play of bright colours is soon observed, and it continues for hours. Again, with regard to explosion of soap bubbles, one is apt to think the whole of the film is converted simultaneously into minute spherules. M. Plateau has formerly shown that it is not so, and has analysed the course of the phenomenon. An experiment proving the contraction of the bubble during its quick destruction is as follows:—A bubble of glyceric liquid about 11 centimetres in diameter is blown with tobacco smoke, and placed on a ring. Having waited till the top appears blue, you break it there with a metallic wire, whereupon the mass of smoke is shot vertically upwards a dozen centimetres, and then spreads out horizontally, in umbrella shape. It then rises more slowly, and is diffused.

PROF. EXNER of Vienna has lately proved that galvanic elements formed of three elementary substances, one of which is bromine or iodine, give perfectly constant action, and that the electromotive forces exactly correspond to the heat values of the chemical processes. There is no trace of polarisation. Bromine and iodine are also shown to be the worst conductors of electricity at present known. Both bromine and iodine conduct entirely without polarisation, (the latter in solid as well as in liquid condition.) The conductivity rises rapidly with the temperature.

CAREFUL experiments by Herr v. Wroblewski on diffusion of liquids (three chloride of sodium solutions and water) are described in *Wied. Ann.* (No. 8), and yield the result that the constant of diffusion (so far as those experiments go) decreases with decrease of the amount of salt, according to a law of simple proportion. The author further tried a photometric method of measuring diffusion, where the proportion of salt is extremely small; using Hüfner's spectrophotometer and (as colouring-matter in water) nigrosin. He cannot claim great exactness for the results, but the constant is at least one place of decimals smaller than the smallest constant of a salt hitherto known.

DR. KALISCHER, who has been experimenting on selenium cells for the photophone, confirms the observations of Adams and Day that light may in certain cases set up in these cells a photo-electromotive force; the cell becoming its own battery. The same experimenter draws attention to a curious point, namely, that the sensitiveness of selenium cells to light is often greater in cells of high resistance than in those in which, by annealing, the resistance has been greatly reduced. A single cell kept for some months gradually lessened in resistance, while becoming less sensitive to light. These anomalies Dr. Kalischer attributes to the allotropic modifications through which the substance passes, the want of homogeneity accounting also for the photo-electromotive forces observed.

AN excellent paper by M. Gariel has appeared in our contemporary, *L'Électricien*, in which the formulæ for the grouping of cells in a voltaic battery, as deducible from Ohm's law, are discussed and represented in graphic diagrams. M. Gariel has thus arrived at a kind of abacus by which the various problems that arise may be geometrically solved by simple inspection.

PROF. LOVERING of Harvard has lately unearthed from the *Memoirs* of the American Academy a paper by Dr. Nathaniel Bowditch of Salem, Mass., communicated in 1815, in which he investigates the figures made by a double pendulum which compounded two vibrations at right angles to one another. This research, which was illustrated by several plates of figures, therefore antedates that of Lissajous, to whom the discovery of these figures is usually accredited, which was published in 1857. Bowditch investigated the cases of the ratios representing unison, the octave, the twelfth, and the double octave. Bowditch was himself inspired to this investigation by a paper written by Prof. Dean of Burlington, Vermont, in which a compound pendulum, identical with that known as Blackburn's pendulum, was used to illustrate the motions of the earth as viewed from the moon. Blackburn's pendulum dates from 1844. Sang, in 1832, used vibrating wires to compound rectangular vibrations; and Wheatstone's kaleidophone dates from 1827.

SELF-LUMINOUS photographs capable of shining in the dark can be made, as Eder has shown, by laying a transparent "positive" upon a sheet of Balmain's luminous paint, and then exposing the latter to sunlight. The photograph thus produced is a "positive" also. It lasts, of course, only for a limited time.

DR. MÜLLER-ERZBACH, who has just made an exhaustive examination of the desiccating powers of different substances, states that there is no perceptible difference between the power of concentrated oil of vitriol, glacial phosphoric acid, and solid caustic potash in this respect, and that caustic soda and chloride of calcium are only slightly inferior, the difference in tension of aqueous vapour between phosphoric anhydride and anhydrous chloride of calcium being a fraction of a millimetre in the barometric column. He also states that caustic soda is absolutely dehydrated by being shut up in a desiccator with caustic potash.

SOLAR PHYSICS[1]

LIEUT.-COL. DONNELLY, R.E., made the following introductory remarks to Prof. Stokes' first Lecture, which was the first of the series:—

I greatly regret both for your sake and my own that I should have to detain you for a few minutes from the lecture which we have all come to hear. It has, however, been considered desirable that some explanation should be given of what has led to the formation of this Committee on Solar Physics, and what has led to the giving of these Lectures. I am glad to say that in engaging your attention for a few minutes I shall not seriously curtail the time that Prof. Stokes will have at his disposal, for he has been good enough to undertake to lecture on Friday in place of General Strachey, who unfortunately cannot give the lecture which has been announced for him.

[1] Introductory Lecture by Prof. Stokes, Sec.R.S., in the South Kensington Museum Theatre on Wednesday, April 6, 1881.

Our history commences in the year 1875, when the Royal Commission on Scientific Instruction and the Advancement of Science made their eighth and final Report, strongly recommending the establishment, by the State, of an Observatory for Solar Physics. They say that their opinion is confirmed by the action which has been taken by foreign countries in this matter, observatories for astronomical physics having been already established in various parts of Italy, while their immediate erection had been determined on at Berlin and in Paris. The Royal Commission further hoped that similar institutions might be established in various parts of the British Empire, and they particularly called attention to the great advantages that India, at certain high-level stations, affords for continuous observations, which are so important in this matter. In 1876 a very large and influential deputation from the British Association had an interview with the then Lord President of the Council, the Duke of Richmond and Gordon, with the view of urging on the Government the necessity of taking action on this and other recommendations of the Royal Commission. In replying to that deputation the Duke of Richmond pointed out that in a certain small way he had already done something in the matter, for Mr. Lockyer had been transferred from the War Office to the Science and Art Department, and facilities were being afforded him for carrying forward that portion of the researches upon this subject which he had been engaged on for several years.

The representation by the Council of the British Association was followed by a memorial from a number of eminent men of science. I need only mention, among others, the names of Adams, Andrews, Broun, Joule, Clerk Maxwell, Roscoe, and William Thomson, to show you how influential a memorial it was. They based their appeal for the formation of an observatory for astronomical physics on the fact that in the opinion of a considerable number of scientific men there was a more or less intimate connection between the state of the sun's surface and the meteorology of the earth, and they called attention to the fact that recent investigations on the part of several independent men had led them to the conclusion that there was a similarity between the sun-spot period, periods of famine in India, and cyclones in the Indian Ocean. They conclude by saying, "We remind your Lordships that this important and practical scientific question cannot be set definitely at rest without the aid of some such institution as that the establishment of which we now urge." It was under those circumstances that the Lords of the Committee of the Council of Education referred the question to Prof. Stokes, Prof. Balfour Stewart, and General Strachey for their opinion as to whether a start could not be made, and most of what is required by the memorialists in the way of daily accurate observation be accomplished by utilising the advantages offered by the chemical and physical laboratories at South Kensington, with the aid of the detachment of Royal Engineers stationed there. I need not trouble you with the terms of the reference. They are given in Lord Sandon's letter of August 13, 1877, which is printed in a Parliamentary paper as a return to an address of the House of Commons moved by Lord Lindsay on March 20, 1879. I may, however, quote one sentence from it: "Although we are not at present in a position to consider the establishment of an official observatory on a comprehensive scale, we believe that some advantage can be gained if a new class of observations can be made with the means at command; since the best method of conducting a physical laboratory may thus be worked out experimentally, and an outlay eventually avoided which, without such experience, might have been considered necessary." I should also mention that Lord Sandon in his letter suggested that the Astronomer-Royal should be consulted on the subject, and he stated that "We propose to ask General Strachey to act with you especially with a view to advising us as to how far any arrangements made at South Kensington may be worked with, or form part of the system of observations which, we are informed, are in contemplation for India." Just at that particular time the Indian Government had made arrangements for having daily photographs taken of the sun's disk at Dehra-Doon in the North-West Provinces, by Mr. Meins, who, while he was a sapper in the Royal Engineers, had been trained by Mr. Lockyer. The Committee to which I have already referred reported at the end of 1877, and they state what in their opinion may be done at once and without entailing any serious cost. This report is also given in the Parliamentary paper to which I have alluded. Nothing however was done at that time, and in November, 1878, the Duke of Devonshire, as Chairman of the Royal Commission on Scientific Instruction and the Advancement of Science, wrote again calling attention to the subject, and strongly urging that the Report of this Committee should be acted upon. In 1879 a small sum, 500*l.*, was taken in the estimates for the expenses of the Committee on Solar Physics. And this has been continued ever since. As soon as that vote had been put into the estimates with the sanction of the Treasury, a Committee was formed consisting of the gentlemen whom I have already mentioned, namely: Prof. Stokes, Prof. Balfour Stewart, and General Strachey, to whom were added Mr. Norman Lockyer, Capt. Abney, and myself. The object of this Committee is to make trial of methods of observation, to collect observed results, to find out what is being done in foreign countries, and so far as possible to collect and bring together all information on this subject, and finally to reduce the Indian observations which have been made since the time that Mr. Meins was sent to India. The Committee made a preliminary report last year, which was presented to both Houses of Parliament, and has been published. I therefore need not trouble you with any of the information contained in it. You will there see what the Committee has been doing, and what arrangements have been made for carrying on the Indian observations since Mr. Meins' death.

While the Committee has been thus acting in its corporate capacity, certain of its members have been carrying on independent researches of their own on different branches of the subject. The results of those researches have been published in the *Proceedings* of the Royal Society, but from the necessarily fragmentary manner of publication, it has no doubt been very difficult, even for men of science, to follow what was being done. Acting, therefore, on a suggestion made by the President of the Royal Society, the Lords of the Council asked the Members of the Committee to give a course of lectures which should bring in a more or less popular manner the results of their researches before the public. It is to that suggestion that this course of lectures is due. So much for the Committee. But I trust you will excuse me if I touch upon one other subject. It is now just within ten days of eighty years since Dr. William Herschel read a paper before the Royal Society which was headed "Observations tending to Investigate the Nature of the Sun in order to find the Causes or Symptoms of its Varying Emission of Light and Heat," and so on. But for the time I have already occupied, I should like to have read to you some portions of this paper, which are very striking even at the present moment. I will however only say now that he followed this paper by another one on May 14, 1801, on "Additional Observations tending to Investigate the Nature of the Sun in order to find the Causes or Symptoms . . . . and a Few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper." In those papers Dr., or, as he was afterwards, Sir William, Herschel very strongly and forcibly urges the importance of a continued observation of the sun's surface and of the sun-spots. He investigates the connection of sun-spots as far as the periods were then known, with cyclones, with the prices of wheat, and other terrestrial phenomena, and he points out of what great advantage continuous observations upon this subject were likely to be. My colleagues can tell you better than I can, and no doubt will in their lectures, what has been done in this matter since the days of Sir William Herschel. I am afraid it is not very much—I mean of course in the way of continuous observations—and yet during the interval a step has been made in the instruments of research almost, if not altogether, as great as that made in astronomy by the discovery of the telescope. I refer to the use of the spectroscope. Now, the use of this instrument, the spectroscope, so far as solar and stellar chemistry is concerned, is no doubt due to a magnificent research by Kirchhoff published in 1859. But I think I may be allowed to call your attention to a statement made by Sir William Thomson in his address to the British Association in 1871. He there says that some time prior to the summer of 1852 he had been taught by a certain distinguished professor at Cambridge the fundamental principles upon which this process of investigation proceeded. I need scarcely, I hope, tell you that I am not endeavouring to introduce parochialism into what should be the cosmopolitan regions of science; still less am I claiming priority for one who I am sure would be the first to repudiate such a claim. But I think you will agree with me that it is rather a striking example of the fitness of things that it is the distinguished physicist to whom Sir William Thomson referred who will give the introductory lecture of this course.

PROF. G. G. STOKES, F.R.S., then delivered the following lecture:—

Some of my colleagues have applied themselves with industry and with remarkable success to various questions connected with the physics of the sun. I am not in that happy condition. I have however been requested to open this course of lectures on Solar Physics. In doing so I will touch but lightly on the labours of my colleagues, because they are going to lecture themselves, and they will be far better able than I should be to expound their own researches. As to the subject of the lecture I have pretty nearly a *carte blanche* before me, and I may choose my own ground. I propose to refer briefly to what is known on the subject and what speculations were made respecting the physical constitution of the sun some considerable time ago, and then to indicate how our notions gradually came to be changed.

Now I need not dwell on the importance of the sun to man. The savage knows how important it is, how man is dependent upon the sun for light and heat; but the man of science knows that, to a far greater extent than the savage can imagine, man is dependent upon that great central body of our system for almost his whole supply of light and heat. For if we want light at night, what do we do but light a candle, or whatever else it may be? If we want more heat than we get directly from the sun we light a fire; but whence comes that fire? In England we commonly use coal; and whence came this coal? An examination of the products of the coal-fields shows that they are the remains of extinct vegetation; and if we may assume that vegetation went on in past geological ages according to the same laws that we observe at the present day, the supply of the carbon, upon which we are mainly dependent for the heat given out in the combustion of the coal, was derived from the air. But in the air it existed in the state of carbonic acid, to which we reduce it in the process of burning; and it was under the influence of light that, by some process the details of which we cannot explain, the carbonic acid was decomposed and the carbon appropriated. So again as regards our supply of light: if we light a candle we make use of what is derived from the fat of animals; they are unable to decompose carbonic acid, and are dependent on vegetables for their food; so that directly or indirectly we come to the agency of the sun. We see therefore how important the sun is to man. But independently of its great importance, it presents us with features of extreme interest, which are calculated to excite the liveliest curiosity in the man of science.

The question arises, first, Is the sun always in precisely the same condition? For more than two centuries it has been known that there is a change in its appearance which has been observed from time to time. I allude to the dark spots which appear on its surface. Those spots are seen to move over the disk of the sun, not with a uniform angular motion, as if there were some body interposed between us and the sun, and circulating around it, but nearly as if they belonged to a solid globe rotating on its axis. I say nearly, but not quite in the same way, because it is now well established by the labours of the late Mr. Carrington, that if we attempt to determine the time of rotation of that body on the supposition that the spots were stuck to it, we obtain different results according to the place of the spot on the sun's disk. As I have said, taken as a whole the spots move nearly as they would do if they belonged to a solid globe to which they were stuck, and in that way we may determine approximately at least the direction of the sun's axis of revolution, or equator. Now Mr. Carrington found that the spots which are situated a short distance north and south of the equator, taken by themselves alone, would indicate a more rapid period of rotation of this body than those which are situated nearer the poles. (They are never found for some considerable distance round either pole.) Associated with those spots there is another appearance called faculæ, which are ridges of extra brightness on the surface of the sun, and which have an evident relation to the spots. They are ordinarily in the neighbourhood of the spots, and moreover—and this is a point worthy of consideration with reference to any theory as to the formation of the spots—it is found that sometimes faculæ will break out at the surface of the sun where there is no spot, but there is certain to be an outbreak of a spot or spots not long later. Besides this outward appearance, which can be seen with even moderately good telescopes, fine telescopes show that the whole of the surface of the sun has a mottled appearance, consisting of portions, some more, some less, bright. It is dotted over with small specks, having the general character of minute specks of bright light. [Photographs of the sun's surface, including a large-scale one, by Janssen, of a small portion, were here exhibited.]

These dark spots are constantly in a state of change, which goes on from day to day, and the finer mottlings change with very great rapidity indeed, so that M. Janssen found that two consecutive photographs taken quickly one after the other did not show the mottling identical; two photographs taken at the same instant did.

Now what notion can we form as to the nature of these spots? One important matter to know with respect to any speculation about their nature is, whether they are elevations or depressions. Mr. Wilson showed even in the last century, by observations of them as they changed their position on the sun's disk by the sun's rotation, that they were below, and not above, the general surface; and to the telescope they give the idea of a hole in a luminous envelope, through which you look down upon something dark beneath; and so the older astronomers adopted the notion that the sun was surrounded with a luminous envelope which they called a photosphere, and that the body of the sun itself was, not absolutely, it may be, but at any rate comparatively speaking, dark. Indeed, Sir William Herschel went so far as to speculate on the possibility of the sun being a habitable globe. How this great luminosity could possibly be kept up around a vast globe like the sun, generally dark and accordingly at a comparatively low temperature, they did not explain, and in fact you must suppose, on this hypothesis, that the true state of things at the surface of the sun is quite unlike what we have at the surface of the earth. Now we must endeavour to make our theories as to the nature of the phenomena which present themselves rest upon known laws as far as we can. Sir John Herschel, indeed, conjectured that possibly the body of the sun might be defended from the heat of the envelope which, as we know on earth, radiates so fiercely into space, by a perfectly reflective canopy. But where are we to get a perfectly reflective canopy? The only example we know of perfect reflection is that of total internal reflection, where rays of light or heat, as it may be, fall with sufficient obliquity on the surface of separation between a denser and a rarer medium, the rays being in the denser medium.

The nearest approach we know to total reflection, leaving that case out of consideration, is that of polished silver; but polished silver, although it reflects by far the largest quantity of the light falling incident upon it, by no means reflects the whole. If a globe like the sun with an envelope of polished silver were surrounded by an intensely glowing body, the globe would not remain cold, at least if we are to rest upon the experiments which we can make in the laboratory. Yet this idea of a dark solid body remained in the mind of astronomers for a long time. I will read a passage from Sir John Herschel's "Outlines of Astronomy" about what the spots are:—"Many fanciful notions have been broached on this subject, but only one seems to have any degree of physical probability, viz. that they are the dark, or at least comparatively dark, solid body of the sun itself, laid bare to our view by those immense fluctuations in the luminous regions of its atmosphere, to which it appears to be subject." This sentence remained unaltered even in the edition of Sir John Herschel's work published as late as 1858.

It was, I think, in 1854, that Sir William Thomson—whom I am happy to see before me—threw out another speculation as to the nature of the heat of the sun. First I should say, perhaps, what it was not supposed to be. If we abandon the idea of a body remaining cool within an intensely glowing envelope surrounding it on all sides, and suppose that the sun is really exceedingly hot, where are we to suppose the source of that heat to be; in fact, what origin are we to attribute to the source of the heat which we know as a fact to radiate from the sun, wherever it may come from? The most natural supposition would be that of primitive heat. Take the sun, that is to say, existing as it was ages ago; starting from that point, then, you may imagine it to be sending out heat all these ages and gradually cooling itself down. Now there would be one very strong objection to that theory if you supposed that the sun was a solid body. It might be glowing, but unless the conducting power were enormously greater than anything we have reason to suspect from experiments we can make on earth, the surface would very quickly cool down and become comparatively dark. The notion of a solid body must be given up if we suppose that primitive heat is the source. It must be at least liquid, and that liquid must be in a state of constant agitation.

Objections, however, occurred to Sir William Thomson's

mind to such a view, and they led him to adopt another, that the heat was due to the impact of meteoric bodies falling into the sun. The surroundings of the sun may be considered to consist of a vast number of meteoric bodies similar to the shooting stars which we see when they come across the earth's atmosphere. An assemblage of such bodies reflecting in a measure the light of the sun may possibly constitute the zodiacal light. Now if these bodies are continually falling into the sun their impact will produce an enormous quantity of heat. I should mention that this idea had been thrown out previously by Waterton, but Sir William Thomson made an important change in it by supposing that instead of being dependent on meteoric bodies casually falling into the sun from the stellar spaces, there is a supply of such bodies circulating round the sun and gradually falling into it. He showed that the heat produced by such impacts would enormously surpass the heat of combustion of the most combustible substances we know on earth. This theory attributes the heat of the sun to something outside itself; what I may call, in contradistinction to that, primitive heat, attributes it to what is inside the sun, to the body itself. According to the meteoric theory the seat of the most intense action is at the surface of the sun itself. The old theory of a comparatively cool nucleus is here given up, and the sun is allowed to be a glowing body, molten, doubtless; but still the most intense action is supposed to take place on the surface of the sun. With regard to the spots I think the idea of Sir William Thomson at that time was that there were great whirlwinds at the surface of the sun from time to time which blew away these meteors, and consequently caused, where they existed, a less intense succession of impacts, and consequently less heat, and that a portion became comparatively dark. I just mention this historically. I will not at present say anything about the very important information which the spectroscope gives us respecting the sun, but will reserve that to a later period.

A different theory was thrown out by M. Faye in 1865. According to this the interior of the sun is intensely hot, and for that very reason, as M. Faye supposed, comparatively speaking non-luminous. He conceived, in fact, that the interior was so hot that bodies were there in a state of dissociation; and as we know that many a glowing gas gives out plenty of heat, but comparatively little light, so it was supposed that the interior of the sun, by virtue of its intense heat, radiated only comparatively little light, and that it was not until the substances of which the sun was composed came to the outside that they became cool enough to enter into chemical combinations, and to supply us with substances which were capable of emitting an abundance of light. Now here there is one feature in common with the old views, namely, that the source of the light is supposed to be a photosphere surrounding a solid body which is, comparatively speaking, dark; but the reason why this body is supposed to be dark is precisely the reverse of that which was supposed in the older views. In the older views the body of the sun was supposed to be comparatively cool: here it is supposed to be so intensely hot that the substances of which it is composed have not yet got into a state in which they can emit much light. According to this theory the spots are places where the photosphere is, so to speak, blown away, and you see down into the intensely hot body of the sun, which is comparatively feebly emissive of light. This view seemed to receive some support from a remarkable discovery made by Mr. Huggins in 1858 with reference to the constitution of the planetary nebulæ. On applying the spectroscope to these planetary nebulæ he made a remarkable discovery, that the spectrum which they emit consists exclusively of bright lines, such as the spectrum we know to be produced by an incandescent gas. Many of these nebulæ have a somewhat stellar nucleus, which seems to exhibit a spectrum of a more ordinary character. Now at first sight this condition of things appeared to be just what the theory of M. Fay required, and to give an explanation of the phenomena according to that view. These planetary nebulæ give out a feeble light compared with the stars; and so, when seen through an aperture in the photosphere, we may suppose that the interior gaseous portions of the sun are too hot to glow with more than this feeble light.

Now that supposition is in contradiction to a very important extension of Prevost's theory of exchanges which was made independently by Prof. Balfour Stewart—who is here present, I am happy to see—and by Prof. Kirchhoff. According to Prevost, if you have a body contained within a heated envelope, and everything has come to its final state, and this envelope is opaque, then all the bodies within it will be of the same temperature. They will receive as much heat from the walls of the envelope as they give out by radiation, and there will be a perfect balance between the radiation and the absorption. If one of those bodies is comparatively transparent, letting through a good part of the heat which it receives from the envelope, it will give out itself comparatively little heat, otherwise it would gradually become cooler. Now the extension I have mentioned is that this is true not merely of the sum total of the heat given out or absorbed, but of each particular kind of heat or light of which that total consists; so that if we take light or heat of any degree of refrangibility, there is a balance between what is absorbed and what is given out.

Now this extension of Prevost's theory militates against M. Faye's theory of the constitution of the sun as regards the constitution of the spots. For, take the interior of the sun. If we take light of any particular degree of refrangibility, the body, that is, this supposed gas which constitutes the bulk of the sun, will be either opaque as regards that kind of light, or transparent, or partially transparent. If it is opaque it is certain to emit light of the same refrangibility. If it is transparent, then the spot would not be dark, because, as regards any kind of light for which this interior gas was wholly transparent, we ought to see the opposite side of the photosphere shining through; just as in the planetary nebulæ we do see what we have every reason to suppose to be a nucleus of the nebula shining right through its enormous semi-diameter. The stars subtend no appreciable angle, but the planetary nebulæ subtend a very appreciable angle, which can be measured, and in all probability, judging by the distance of the planetary nebulæ from us, their dimensions are gigantic as compared with the average size of the stars, and as compared in all probability with our own sun. Therefore there ought to be seen in the sun, on that supposition, the same phenomenon as *is* seen in these planetary nebulæ, namely, the photosphere on the far side shining across the gaseous globe. It seems to me that that consideration is fatal to the acceptance of M. Faye's theory as a whole, and that we must have recourse to some other.

Now I have mentioned already Sir William Thomson's meteoric theory, in which is involved the very important consideration of the conversion of work into heat. I do not mean at all, in stating some possible objections to that theory (which he has himself since given up), to go against the supposition that the original source of the sun's heat may have been the conversion of work into heat, but starting with the sun as it was some ages ago, has the subsequent heat been derived from itself, or from the outside? According to the theory of M. Faye, the heat would be derived from the sun itself, which would be spending its heat gradually. So far (giving my own view as to what is probable) it seems to me that the probabilities are in favour of that part of the theory. Well then, if the spots are not due to the dark body of the sun being exposed by something being removed from the outside, be it that the body is dark from a deficiency of heat or from an excess of heat, what may we suppose them to be? In a paper published in the *Philosophical Transactions* by Messrs. De La Rue, Stewart, and Löwy, the authors have advocated the view that the spots are due, not to an uprising from the centre of the sun, but to a down-pour of cooler portions of the matter which has been ejected from the sun. But here I think I cannot go on without going back to some researches in which the spectroscope plays a most important part. It is to Prof. Kirchhoff that we owe the first extensive application of the spectroscope to the study of the sun. He held that since bodies in the state of incandescent gas give out bright lines in their spectrum, according to the extension which he made, independently of Prof. Balfour Stewart, of Prevost's theory of exchanges, these glowing gases ought to absorb light of the same refrangibility coming from a body behind. Now if you had a glowing gas in front of an opaque body glowing at the same temperature, you ought to see neither dark nor bright lines, for the gas would absorb the light of the refrangibilities which itself gives out, and it would not absorb the light of the refrangibilities which it does not give out, so that in the region of the bright lines we should, even if the body behind were away, get the full amount of light due to the temperature, coming from the glowing gas itself; in other regions where there is no such bright light coming from the gas, you get the full amount of light coming from the opaque body behind. But if you suppose this gas in front, glowing though it be, to be at a lower temperature than the opaque body behind, then it would absorb more light of the

kind which it gives out coming from the body behind than it gives out to replace it by virtue of its own emission, and accordingly we should see the place of those bright lines, or what would be bright lines if the gas were there alone, dark on a bright ground. By following out that theory he was enabled to identify a great number of the dark lines in the solar spectrum with the bright lines given out by elements which we know at the surface of the earth, such as iron, magnesium, and so forth. Now this throws a most important light on the constitution of the sun. It indicates that even in the outer, and, comparatively, therefore cooler portion of the sun, there must still be a temperature so enormous as to be above the boiling-point of iron, and above the boiling-point of some of the most refractory metals. And now I will refer to a later application of the spectroscope which was made by a gentleman whom I see before me. First I should say that in the year 1842, in observing a total solar eclipse, a new phenomenon was witnessed, or at least a phenomenon which, if not new, had not previously attracted general attention. The dark body of the moon was seen to be surrounded by rose-coloured prominences having the appearance of mountains. What were these? What could possibly be their nature? We had but a small time to observe them; the greatest duration of a total eclipse of the sun is a little over four minutes, and these eclipses occur only once perhaps in two years or so, and when they occur the totality extends over a strip along the earth's surface of only inconsiderable breadth, with probably a great portion of it falling on the ocean, so that if we were there present in a ship we could hardly make any observations but what could be taken by the naked eye. The study of these prominences and the nature of them must have been therefore a slow matter to get on with, so long as we were limited to the observation of them during the period of a total eclipse. The change of height of those prominences shows that they belong not to the moon, but to the sun. Of course, as the moon moved over the body of the sun they would, if they belonged to the sun, tend to get shorter and shorter as they were covered in, and would reveal themselves gradually in the same way behind the opposite side, which is just what happened. In 1860 special provision was made for the observation of these prominences, and Mr De La Rue undertook to make a series of photographs, which led to some most important results. They showed, among other things, that in some cases the prominences, whatever they were, were not at all attached to the body of the sun, but were suspended as clouds around it. They could not, therefore, be mountains clearly. Mr. Lockyer, for some considerable time prior to 1868, had been devising in his mind a possible mode of rendering those prominences visible, and studying their nature without waiting for or being dependent upon the rare phenomenon of a total eclipse. If the light which those prominences gave out consisted of bright lines, then, by applying a spectroscope of high power to the study of those bodies, we might so far reduce the intensity of the intervening portions of the spectrum where there is the diffused light coming from the immediate neighbourhood of the sun's disk as to render them visible. At last he was rewarded by success, and the announcement of this discovery was made to the Royal Society. Meanwhile M. Janssen had gone out to India to observe a total eclipse, and the special subject which he took up was to observe the spectrum of those prominences, which he did with success. The idea struck him, "Why should not this be done any day?" He tried, and the next day he succeeded. In point of absolute time this was before the observation of Lockyer, although at the time no account of it had reached this country, so that the two observations were perfectly independent of each other. Well, subsequent improvements in the method of observing those prominences have enabled us to see them at will, so that they may be observed from day to day, when we choose, from hour to hour, from minute to minute. The forms of them can be seen, and it is found that they move with astounding velocity. They are projected upwards from the sun with a velocity sometimes of 100 or even of 140 miles per second. Their forms were such as we might naturally attribute to the ejection of gas from the body of the sun. It had been conjectured that they might be of the nature of auroral discharges. Their features however indicate that they are projections of actual matter from the sun, and moreover the nature of their motion indicates the same. This gives us, then, a new idea of the vastness of the changes which are continually going on at the surface of the sun.

Now what is the origin of these changes? It seems to me that the most reasonable idea that we can form respecting them is something derived from what takes place in our own earth, and what we can observe here. Suppose the sun to be shining, we will say on a summer's day. If we look horizontally with a telescope everything is seen to be in a state of tremor; the air is far from homogeneous. What is the reason of that? The greater part of the sun's heat passes through the upper strata of the atmosphere and reaches us, the air being transparent with regard to a large portion of the sun's heat. It warms the surface of the earth. That in turn warms the air in contact with it, and further radiates forth heat of a kind for which the air is opaque. The consequence is that the lower portions of the air in contact with the earth get warmer, and that unequally according to the nature of the ground—more on stones and gravel, for instance, and less on grass and so forth. Being warmer they get lighter, and therefore there is a constant ascent, a constant mixing of the hot and cold portions by currents of convection. As this goes on continually a stratum of air of considerable height becomes warmed in this manner, and sometimes an exchange by convection or something of the nature of convection takes place on a very grand scale. Let us take the case of summer weather. Suppose we have a succession of hot days accompanied by a good deal of evaporation, gradually these several currents of convection cause a warming of a stratum of air below of considerable height, which is also well supplied with moisture from the evaporation. At last, taking the stratum as a whole, the equilibrium becomes unstable, and there is an uprush; hence there is a kind of chimney formed, through which the air flows upwards, and then spreads out laterally overhead. This appears to be what takes place in our summer thunderstorms. The heated and moist air forms for itself a chimney, and in ascending there is a rapid deposition of what was previously vapour of water in the now condensed state of water itself, and a rapid fall of rain occurs after a time. This appears from some cause or other to be the occasion of the development of a great deal of electricity, which is manifested in the form of lightning. While this action goes on you have the in-draught towards what I will call the chimney from all sides; the vapour sooner or later gets condensed, and there is a fall of rain accompanied by lightning. Sometimes there is hail even in summer; for when the air charged with vapour gets to a particular height the vapour becomes condensed and forms rain; but it may be that the stratum of the upper air that is pierced through is below the freezing-point, and, the rain falling through this, it gets frozen. I will just call attention to one fact; according to this view, you see you may have a general current of wind over the country—say, for the sake of illustration, from west to east. Suppose there is a region to the west where an ascending current has been formed; then there is an in-draught from all sides to that place, and when the thunderstorm has not yet come on you are in a comparative calm, because the general direction of the wind being from the west, and the in-draught carrying the air from the east, the two together tend to neutralise one another; or you may have actually a wind blowing towards the region of the thunderstorm. Accordingly we know people often say that thunderclouds move against the wind. I shall have occasion to refer in my next lecture to the development of atmospheric electricity in reference to some speculations in regard to phenomena accompanying changes in the condition of the sun; but at present I merely refer to this process as illustrative of what seems to be the most natural supposition to make regarding the origin of these disturbances which are found to be continually taking place at the surface of the sun. The outer portions of the sun are the source of a gigantic amount of radiation of heat and light which passes out in all directions. By this radiation those outer portions must tend to a certain extent to cool down, and consequently, as the same physical conditions hold good, if the same physical laws hold good, at the surface of the sun that we have on our own earth, you may easily suppose that, having become cooler than they were, the substances become specifically heavier, and accordingly give rise to currents of convection similar to those that we have in our own atmosphere from a similar cause, but operating in one respect in a different way, because in the solar atmosphere there is a cooling from above, but in that of the earth a heating from below. Those minor currents of convection ascending and descending naturally enough give rise to that mottled appearance which is always seen on the sun's surface, because if the interior of the sun be hotter than the portions which have cooled by radiation, then the ascending portions would naturally, being at a higher temperature, be brighter, the descending portions darker, and small

(comparatively speaking) descending currents may very likely, as it appears to me, be the cause of these appearances. Now just as at the surface of the earth these minor currents of convection are continually going on, and mixing up the heated portions below with less heated portions above, till at last a great catastrophe takes place, and we have a thunderstorm or even a cyclone; so the same thing may take place at the surface of the sun, and minor currents of convection may gradually cause a cooling of a greater stratum, and at last the equilibrium becomes unstable, and a great change takes place between the superficial portions and those which lie beneath, and we have the manifestations of faculæ and spots. According to this view the faculæ would consist of the heated portions on a larger scale coming from the interior, and the spots of a subsequent down-rush on a large scale of the portions which had been erupted and had cooled by radiation. Kirchhoff supposed that the spots were due, not to depressions of the sun, but to clouds of comparatively cool gases or vapours rising above the general surface. This was in contradiction to the relative altitudes of the sun-spots and the general surface as made out originally by Wilson, and subsequently confirmed by the observations of others; and moreover there are some other difficulties connected with it. Let us suppose that there is an eruption of hydrogen which has got cool, then if that exists and there is a cold draught at some distance above the sun, we cannot say it would absorb any longer the rays which it is capable of absorbing when glowing, because the correspondence of emission and absorption only necessarily holds good on condition that the substance is at a given temperature. If the temperature changes it is possible, and in many cases we know it is a fact, that the mode of absorption may change with it. We know that the cold hydrogen is transparent; we know, theoretically at any rate, that glowing hydrogen must be opaque with regard to light of the particular refrangibility which it emits; hence a cool mass of gas might cease to be opaque even by virtue of its being cooled. Again, if we had a cloud of, say, vapour of iron, and if this were condensed into actual drops or globules of molten iron in the upper portion of the atmosphere, they would form such a very rare sort of mist as would be something like a very rare haze which barely obscures the sun, and would not give rise to more than a slight general darkening. But if the gases in descending got warmed again, they would then be in a condition to absorb light specifically; but being at a lower temperature than the sun they would not give out nearly so much light as they absorb.

That seems to me to be the most natural explanation of the spots and of the phenomena attending them. I may have something more to say about this on a future occasion; but, as I see the time is going on, it would probably be more agreeable to you that I should postpone anything further I have to say to you upon this subject until my next lecture, in the course of which I hope, as I have said, to point out a speculation as to the connection which exists between sun-spots and certain phenomena which we know exist at the surface of the earth. There are probabilities to my mind in favour of it, but I will, with your permission, defer allusion to it to my next lecture.

(*To be continued.*)

THE HELVETIC SOCIETY OF NATURAL SCIENCES

THIS Society held its sixty-fourth annual session at Aarau on August 8, 9, and 10, under presidency of Prof. Mühlberg, whose opening discourse treated of recent progress in physiology and chemistry. An account of the proceedings (of which we here offer a brief *résumé*) will be found in the *Archives des Sciences*.

In the Section of Physics and Chemistry Prof. Forel read a valuable paper on the periodic variations of glaciers. These periods of advance and retreat are proved to embrace several years (five to twenty and more); they are due in the first instance mainly to variation in velocity of the glacier, and this to small variations in the thickness of the *névé* repeated in the same sense for several years, the consequent variation of velocity becoming much more pronounced as the glacier descends, and the ultimate effect being separated by many years from its original cause. The varying heat of summer appears to be of quite secondary importance. In one of three papers, communicated by M. Raoul Pictet, he described his new method of distillation and rectification of spirits by a rational use of low temperatures. The two processes are performed at once; and with considerable economy a purer product is obtained. Another paper explained the principle of his rapid steamer, now being made, and the working of which will be watched with interest. The third treated of the different qualities of steel as regards magnetisation and permanence of magnetic power. (To this and the preceding, reference has been already made in our columns.)

M. Krippendorff exhibited a model of a balloon, to be propelled in light winds by escape of compressed air at the end of a wooden axis rendered horizontal or inclined according to the direction aimed at (by shifting the suspension of the car). The air would be compressed by four men into a small copper receiver at the other end of the axis; and a second reservoir holding liquid carbonic acid would be at hand in case of need. In a micro-telephone described by Prof. Amsler-Laffon, the flame of a manometric capsule (like those of König) is inserted in a telephone circuit; its conductivity being increased with vapours of potassium. Its change in form and size through vibrations of a thin plate of steel under sound, entails changes in electric resistance, and the telephone is affected accordingly. The apparatus is said to be very sensitive. Some useful hints on representation by projection of longitudinal and transversal vibrations are given in a paper by Dr. R. Weber. MM. Soret and Sarasin indicated a new method of determining the angle of rotation of quartz, and showed, in a curve, how the rotatory power varies with the wave length.

An interesting observation is reported by Prof. Dufour, who finds in deformation of images produced on large surfaces of calm water, a new proof of the roundness of the earth. This may often be witnessed on the Lake of Geneva, *e.g.* the reflected steeple of Montreux, seen from Morges; and in the case of ships some kilometres distant at sea. Prof. Forel, from a study of the recent earthquakes in the Cantons of Vaud and Neuchâtel, finds analogies to the phenomena of a vibrating plate in Chladni's experiments; the intensity and direction of a shock, *e.g.* being very different in places quite near each other. Guided by theoretical considerations, M. Chappuis has measured the liberation of heat through condensation when water is introduced suddenly into an evacuated tube filled with charcoal in temperature equilibrium with the water, and from the data, and the compressibility of water he infers the adherent water to be under a pressure of at least 36 million atmospheres. Among other subjects discussed were the measurement of radiant heat with the differential thermometer (Dufour), the determination of tartar and tartaric acid in wines of commerce (Piccard), and the action of bromine on a mixture of water and sulphide of carbon (Ureck).

In Zoology M. Fatio gave some account of his continued researches on disinfection with sulphurous acid. The vapours act in two ways on all organisms which depend on oxygen for life, viz. asphyxiating them by suppression of that element, and gradually burning them interiorly, the acid being dissolved in their humours or aqueous parts; the doses and times of application are varied accordingly. The more aqueous in substance an animal or plant is, the more quickly is it affected. The dose and time of application, in different receivers, will also be varied according to the temperature affecting diffusion of gas and the hygrometric state of the air and enveloping material. M. Fatio operated successfully on vibrions and bacteria in infusions submitted to an atmosphere mixed with sulphurous acid (the depth of the liquid here determines the time of exposure), and the range of application is evidently wide. M. Vogt gave some interesting facts showing the extensive adaptation of colours in animals of the Saharan Desert to that of the ground. With regard to the exceptional colour of nearly all Coleoptera, viz. black, he considers they find protection in their bad smell, and also their strong resemblance, when contracted and feigning death, to excrement of gazelles, goats, and sheep. The animals brought to the surface by water of Artesian wells in that region M. Vogt finds to be quite without the characters of animals living in caverns and subterranean water; their eyes are well developed, and their colours pronounced. They are indeed proved to live but temporarily underground. With albuminised paper Prof. Forel fixed 40 metres as the limit of penetration of chemical rays into water, but Dr. Aspen has, by a different method, got a photographic effect in the Lake of Zurich as far down as 90 metres. The researches of Dr. Yung on the influence of food on frog development have been formerly noticed; and of the remaining subjects we merely note the sense of colour in Cephalopoda (Keller), a peculiar mode of copulation in dendrocœle marine worms (Lang), and the conditions of production of

rhythmical contractions in the wing membranes of bats (Luchsinger).

In the Botanical Section M. Buser read a paper on Swiss willows, and Prof. Schnetzler gave some observations on the vegetation of *Lathræa squamaria* on tree-roots.

To the Section of Geology M. Jaccard submitted a project of maps of the "erratic phenomenon" in Switzerland, on the plan of those constructed by MM. Falsan and Chantre for the Rhone Valley. Dr. Rothpletz discussed the *rôle* of faults in the geology of the Alps, showing that these are by no means exceptional, and deserve more study than they have hitherto had. Dr. de la Harpe presented a collection of Egyptian nummulites. Dr. Gillieron had a paper on the age of the red schists of the Simmenthal. Prof. Mayer-Ermann furnished proof that the Loire must have flowed into the Parisian Gulf of the North Sea during the whole Eocene period, and that it was only at the end of the Inferior Neogene or Aqnitanian epoch that it made the bend at Orleans and entered the Atlantic. The Pleistocene of Central Europe formed the subject of an instructive paper read by Dr. Rothpletz at the first general meeting.

In Medicine a paper was read by Dr. Bircher on the extension of deaf-mutism in Switzerland, and its relations with goître and cretinism. He finds that these three are merely different manifestations of one and the same principle of degeneracy of race, a principle which, in Switzerland, is endemie in the Triassic, Marine Molassic, and Eocene formations.

THE ARCHÆOLOGICAL CONGRESS AT TIFLIS

THE proceedings of this Congress, recently held at Tiflis, were both interesting and animated. No less than 700 members arrived at Tiflis from varions parts of the Caucasus, and fifty-five from various parts of Russia. The foreign members were few—Prof. Virchow, who took advantage of his stay in the Caucasns to make an excursion to Ossetia, and Messrs. Aeger and Hubsch from Vienna. The Congress was opened by Count Onvaroff in one of the halls of the palace, before an audience of about 800 persons. The President of the Congress, M. Komaroff, pointed out that the Congress had met with much sympathy from all interested in the study of the Caucasus, as well as much help from the teachers of primary and secondary schools, who had sent in many interesting objects for the exhibition. We notice among the objects exhibited a most interesting collection of bronze antiquities from Ossetia, Bosphorian antiquities from a *kourgan* of the province of Kouban, stone implements from Tzalka, Georgian ornaments and stone implements from the provinces of Novgorod and Tver. Ossetia has been known for many years for a great find of interesting bronze implements, of figures of animals, curved hatchets with spirals and zig-zag ornaments and with figures of animals, as well as religious objects belonging to some unknown worship; the collection, which was bought some time ago by M. Chantre, is very complete, and will soon be described by him. The new collections from a *kourgan* at the Sievers Station consist of massive gold, and represent subjects of Greek mythology. On the same day the excellent Caucasian museum which was founded several years ago, but was closed for two years for unknown reasons, was re-opened.

Count Ouvaroff made an interesting communication on the remains of the Stone period which were fonnd near Irkutsk, on the bank of the Angara River, at Talminskoy village. Many human skeletons, with stone and bone implements, and perforated teeth of animals, were found there, together with hatchets of jade (nephrite), which numbered as many as two hundred. This is the first find of jade implements in graves in Russia. This communication gave rise to an interesting discussion, during which M. Moushketoff, the well-known traveller in Turkestan, spoke of the great monolith of nephrite at Samarcand, on the grave of Tamerlane. It has the shape of a parallelopiped, 7·8 feet long, 1·5 foot wide, and 1·2 foot high, and weighs about 1800 pounds, whilst the greatest pieces of nephrite which are fonnd in boulders do not weigh more than 700 or 750 pounds. It is well polished, but is broken through its centre. The rock resembles very much that of Khotan. As to the places where nephrite is found *in situ*, our knowledge is still very limited. Messrs. Shaw and Hermann Schlagintwert have seen nephrite mines in the Kwen-Lun, close by Balaktchi, at a height of 12,000 feet; according to Dr. Stoliczka it appears there as veins in chlorite-slates and quartzites. Two other places where nephrite is found are known north of the Kwen-Lun Mountains, close by the Kilian Pass, at a height of 6070 feet, and near Kamat village on the highway to Khotan, at a height of 5790 feet; a fourth is presumed to be at the sources of the Yourson-tash, or Khotan River. But the nephrite implements which we found in graves were mostly made from boulders of this rock, which are often found in Eastern Siberia on the shores of Lake Baikal, and on the Boutogol Mountain in the Sayan Highlands; however, we do not know that nephrite was found *in situ* in these latitudes. All implements which are in the St. Petersburg museums were made of nephrite from Eastern Siberia, whilst the Kwen-Lun jade is used only in recent Chinese products.

Prof. Samokvasoff made a communication on his finds in the graves on the Caucasus, in the neighbourhood of Pyatigorsk. He excavated about 200 graves belonging to the Stone, Bronze, and Iron periods. In the larger graves he found bronze implements together with stone ones, and as there are in these graves, together with bones of sheep, several split human bones which do not belong to skeletons, he supposes that during the Bronze period the inhabitants of this part of the Caucasus were Anthropophagists. This opinion, however, was not concurred in by the majority of members of the Congress.

The chief work of the Congress was in the branches of History and Antiquities; but we notice also a special sitting for communications in French and German, during which several papers were read connected with the natural sciences. Thus Dr. Obst, Director of the Leipzig Ethnographical Museum, read a paper upon the results of the statistical researches on the colour of hair and eyes in Saxony, and M. Smirnoff gave the results of the same inquiries with the Armenians and Georgians of Transcaucasia, as well as with the Russian population of the provinces of Kouban and Stavropol. Out of 2500 Armenian children there were 63 per cent. of dark, 4 per cent. of fair, and 33 per cent. of mixed (fair hair with dark eyes, or *vice versâ*). Of 1400 Russian children there were only 14·5 per cent. of dark, and it is deserving of notice that M. Smirnoff could not discover any difference between Great Russians and Little Russians, the number of fair children being 33·3 per cent. in the former, and 32·0 in the latter, whilst the mixed make respectively 52·2 and 53·5 per cent. As to Georgians and Imers, the observations are not sufficiently wide, but it may be stated that purely dark children are less numerous (50 to 55 per cent.) than with Armenians.

Prof. Virchow gave a long and interesting lecture on the chief problems of the Ethnology and Archæology of the Caucasus, accompanied with some remarks on the civilisation of its former inhabitants. Speaking on the usually-received opinion that the Caucasus was the highway for populations coming from Asia to Europe, Prof. Virchow expressed some doubts as to the crossing of the Caucasian passes by whole tribes at a time when the communications were so difficult and the ice-covering descended lower than now. It would be most important, therefore, to know if the first inhabitants of the Caucasus came from the north or from the south. Speaking further of the Ossetians, Prof. Virchow was astonished not to find among the adult population a single true fair type, which might seem contradictory of former opinions; only among children did he find fair-haired individuals with rosy Flemish cheeks. On the other hand, some measurements have brought him to the conclusion that the Ossetian skull is short and high, very different from the German type of skulls. Dolichocephalic skulls are very rare, and show that the tribes of the Caucasus have undergone much mixture with other people. As to the antiquities found in Ossetia, Prof. Virchow considers that the civilisation they speak of was far more recent than that discovered by Dr. Schliemann at Troy, as it does not contain stone implements, but has, on the contrary, curved fibulæ which were unknown at Hissarlik. The ornaments of the Ossetian bronze-implements, and especially the figures of stags, horses, and mountain-sheep, seem to show a connection between the former inhabitants of the Caucasus with those of the Altai Mountains.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The following are the courses of instruction in natural science to be given this term at Oxford:—Prof. Odling lectures on the atomic theory, Mr. Watts on organic, Mr. Fisher on inorganic chemistry, and Mr. F. D. Brown on physical

chemistry. The Linacre Professorship of Physiology, vacant by the death of Prof. Rolleston, will not be filled up in time for the new professor to undertake this term's lectures. Mr. Hatchett Jackson will give the professorial lectures, taking the nervous system for his subject; Mr. Thomas will lecture on comparative anatomy; and Mr. Robertson will form a class for practical microscopy. Prof. Pritchard will give a course of lectures on the theory of the transit instrument and on the planetary theory, and will form an elementary class three evenings a week at the University Observatory. Prof. Lawson lectures on vegetable histology at the Botanic Garden; Prof. Maskelyne on crystallography; and Prof. Prestwich on the principles of geology at the Museum. Prof. Westwood gives informal instruction on the Arthropoda every afternoon.

At Christ Church Mr. Vernon Harcourt gives a course of lectures on the metallic elements, and Mr. Barclay Thompson a course on the Lemuridæ and Simiadæ. At Magdalen Mr. Yule continues his demonstrations on the chemistry of the tissues and secretions. At Balliol Mr. Dixon forms a class for the determination of the composition and vapour-density of organic substances. At Exeter Mr. Morgan lectures on histology.

At Trinity College the Millard Scholarship in Natural Science has been awarded to Mr. A. E. Field from the Modern School, Bedford.

SCIENTIFIC SERIALS

The American Journal of Science, September.—Benjamin Peirce.—Emerald green spodumene from Alexander County, North Carolina, by E. S. Dana.—Objects and interpretation of soil analyses, by G. W. Hilgard.—Mineralogical notes, by B. Silliman.—Liquefaction and cold produced by the mutual reaction of solid substances, by E. M. Walton.—Spectrum of arsenic, by O. W. Hartington.

Journal of the Franklin Institute, September.—On the effect of prolonged stress on the strength and elasticity of pine timber, by Prof. Thurston.—Relative economic efficiency of Corliss condensing and non-condensing engines, &c., by Chief-Engineer Isherwood.—Discussions on rails (continued).—Burnishing and ductilising steel, by Mr. Reese.—Industrial education from a business standpoint, by Mr. Clark.

Annalen der Physik und Chemie, No. 9.—On the relation of friction-constants of mercury on temperature, by S. Koek.—On the internal friction of solutions of some chromates, by K. F. Slotte.—Some experiments on heat-conduction, by C. Christiansen.—On the vapour-tensions of liquid-mixtures, by D. Konowalow.—On an electro-dynamic balance, by H. Helmholtz.—On the change of the thermo-electric position of iron and steel by magnetisation, by V. Strouhal and C. Barus.—The cycle obtained through the reaction current of electrolytic transference, and through evaporation and condensation.—On the electro-magnetic rotation of the plane of polarisation of radiant heat in solid and liquid substances, by L. Grunmach.—The height of the earth's atmosphere, by A. Kerber.—On the courses of a free particle on the rotating earth-surface, and their significance for meteorology, by A. Sprung.—On the ether as a means of action at a distance, by G. Helm.—Remark on the paper on a new volumenometer, by A. Paalzow.

Journal de Physique, September.—The principle of conservation of electricity, or second principle of the theory of electric phenomena, by G. Lippmann.—Researches on the refringent power of liquids, by B. C. Damien.—The devioscope, or apparatus showing directly the ratio between the angular velocity of the earth and that of any horizon round the vertical of a place, by G. Sire.—Processes for making figures for demonstrations with the aid of projections, by M. François-Franck.—Notes from the *Journal* of the Russian Physico-chemical Society.

Archives des Sciences physiques et naturelles, August 15.—Comparative study of different qualities of steel as regards magnetisation and permanence of their magnetic power, by M. Pictet.—Some theorems of thermodynamics and their application to the theory of aqueous vapour, by G. Cellerier.—On Comet *b* of 1881, by MM. Thury and Meyer.—On the comet of August, 1881, by M. Meyer.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 7.—On bicarbonate of ammonia, by M. Melsens.—Some experiments on thin liquid films, by M. Plateau.—Effects of lightning on trees placed near a telegraph wire, by M. Montigny.—Analysis of the light of Comet *b*, 1881, by M. Fiévez.—On the theory of binary forms with several series of variables, by M. Le Paige.

Rivista Scientifico-Industriale, August 15.—On the causes of earthquakes, by Dr. Lucchetti.—The Pliocene fossils of Sam-Benedetto del Tronto, by Prof. Spada.

September 1.—Measurement of velocity on railways, by A. Milesi.—Automatic apparatus for coiling metallic wires (with silk or cotton), by G. Serravalle.

Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westfalens, 1880, Second half.—Wandering stones, by H. Reauleaux.—Geognostic results of earth-boring near the infantry barracks in Osnabruck, by W. Treckner.—On the application of the electro-dynamic potential to the determination of ponderomotive and electromotive forces, by R. Clausius.—Description of the spiders hitherto observed at Bonn, by P. Bertkau.

1881.—First half.—The quartzite and slate on the eastern border of the Rhine slate hills and their neighbourhood, by C. Chelius.—On the distribution of animals in the Rhongebirge and the Main valley with reference to Eifel and the Rheinthal, by F. Leydig.—Contributions to the insect-fauna of the coal-formation of Saarbrucken, by F. Goldenberg.

Memorie della Società degli Spettroscopisti Italiani, July.—Protuberances observed at Rome during the first quarter of 1881, by P. Tacchini.—Two solar regions in constant activity during 1880, by the same.—On the distribution of spots, faculæ, and protuberances on the sun's surface during 1880, by the same.—On direct and spectroscopic solar observations made at Rome in the first quarter of 1881, by the same.

SOCIETIES AND ACADEMIES

Vienna

Imperial Academy of Sciences, October 6.—V. Burg in the chair.—The following papers were read:—T. Singer, on secondary degeneration in the spinal marrow of dog.—R. Pribram and Al. Handl, on the specific viscosity of liquids and its relation to the chemical constitution.—James Moser (Cambridge), on the microphonic action of selenium-cells.—V. Dvorak, on some acoustic phenomena of motion, especially on the acoustic radiometer.—Boh. Brauner, contributions to the chemistry of cerium metals.—E. Goldstein, on the band-spectrum of air.—T. Schlesinger, a sealed packet relating to the unity of natural philosophy.

CONTENTS

THURSDAY, OCTOBER 27, 1881

SCIENTIFIC WORTHIES

XVIII.—JAMES CLERK MAXWELL

Born June 13, 1831; *Died November* 5, 1879

WE have already (vol. xxi. pp. 43 and 317) said so much on the life and work of the late Prof. Clerk Maxwell, that in presenting his portrait as one of our Scientific Worthies, little more is necessary than to refer to the leading facts of his life. Born on June 13, 1831, he was the son of John Clerk Maxwell of Middlebie, a scion of a well-known Scottish family, the Clerks of Penicuick. When James was only eight years of age, he lost his mother, after which his father led a retired life, devoting himself to the care of his estates and of his son. The latter was educated in the first instance at the Edinburgh Academy, where in 1845 he gained the Academical Club Medal for Geometry, and the Silver Medal for Mathematics in 1847. A visit to William Nicol at this period was a marked event in his life, leading him, with apparatus of his own construction, to make observations on polarised light. A pair of prisms presented to him by Nicol were treasured by him throughout life, and three weeks before his death they were deposited in one of the show-cases of the Cavendish Laboratory.

After leaving the Academy, Maxwell, to quote the words of Prof. Tait (NATURE, vol. xxi. p. 317), "spent the years 1847-50 at the University of Edinburgh, without keeping the regular course for a degree. He was allowed to work during this period, without assistance or supervision, in the Laboratories of Natural Philosophy and of Chemistry: and he thus experimentally taught himself much which other men have to learn with great difficulty from lectures or books. His reading was very extensive. The records of the University Library show that he carried home for study, during these years, such books as Fourier's *Théorie de la Chaleur*, Monge's *Géométrie Descriptive*, Newton's *Optics*, Willis' *Principles of Mechanism*, Cauchy's *Calcul Différentiel*, Taylor's *Scientific Memoirs*, and others of a very high order. These were *read through*, not merely consulted." In October, 1850, Maxwell went to Cambridge, entering at Peterhouse. Soon after his entry at Peterhouse, however, in December, 1850, he migrated to Trinity, where he found spirits of tastes similar to his own in the matter of physical research; here he soon became a leader among his fellows. In 1854 he came out Second Wrangler, and was bracketed as First Smith's Prizeman. In 1855 Maxwell became a Fellow of Trinity, and in 1856 he obtained the Professorship of Natural Philosophy in Marischal College, Aberdeen. To quote the memoir by Mr. W. Garnett in NATURE, vol. xxi.:—"In 1858 he married Katherine, a daughter of Principal Dewar of Marischal College, thus vacating his fellowship at Trinity. In 1860 he succeeded Prof. Goodeve as Professor of Natural Philosophy and Astronomy in King's College, London, but after the death of his father he retired in 1865 to his estate in Scotland, where he subsequently carried out his father's plans for completing the house and offices at Glenlair. In 1871 he was invited by the Senate of the University of Cambridge to accept the Chair of Experimental Physics which had just been created, and on October 25, 1871, he delivered his inaugural lecture as Professor of Experimental Physics in the University of Cambridge. At first the most important part of his work consisted in arranging the details of the Cavendish Laboratory which the Duke of Devonshire had offered to present to the University, and the building of which was personally superintended by Prof. Maxwell from first to last. The whole of the arrangements which render the Cavendish Laboratory so admirably adapted for Physical investigations, are due to the care and forethought of Prof. Clerk Maxwell. When the building had been completed and formally presented to the University, the Duke of Devonshire further signified his desire to provide it with a complete equipment of apparatus, and all this was procured under the personal supervision of the Professor. In 1872 he was elected Honorary Fellow of Trinity College, Cambridge."

During the winter of 1878-9, Prof. Clerk Maxwell's health began to give way, and with some transient gleams of hope he gradually sank, dying on November 5, 1879. He received many honours during his lifetime; he was a Fellow of the Royal Society, LL.D. of Edinburgh, and D.C.L. of Oxford; Honorary Member of the American Academy of Arts and Sciences, the American Philosophical Society, and the New York Academy of Sciences; Corresponding Member of the Imperial Academy of Sciences, Vienna, and Associate of the Amsterdam Royal Academy of Sciences.

In 1860 the Rumford Medal of the Royal Society was awarded to Prof. Clerk Maxwell "for his Researches on the Composition of Colours, and other Optical papers." In his address on the presentation of the medal, Major-General Sabine alluded to Prof. Maxwell's calculation showing the connection of the "mechanical strains to which elastic solids are subjected under certain conditions with the coloured curves which those solids exhibit in polarised light."

To Clerk Maxwell's private character, to the position he unobtrusively took as a Christian, to his qualities as a poet and humorist, and to the varied work he has accomplished, it is scarcely necessary again to allude here; all these points will be found clearly brought out in the articles by Prof. Tait and Mr. Garnett above referred to. Nor is it necessary to repeat here the list of his principal papers and publications, and the great and important additions which Clerk Maxwell made to the sum of scientific knowledge, or the light he shed on the principles of the departments of science which he specially cultivated. Besides the references already given we would commend the reader who desires to have a fairly complete notion of the value of the work of the remarkable man whose portrait we give to-day, to the articles by Prof. Tait on Clerk Maxwell's "Electricity and Magnetism," vol. vii. p. 478, "Matter and Motion," vol. xvi. p. 119, and the numerous papers by Maxwell himself scattered through the volumes of NATURE.

DR. SIEMENS ON TECHNICAL EDUCATION

FEW can read the address of Dr. C. W. Siemens to the Midland Institute, which appears in another place in our columns (p. 619), without admitting that of

all living men Dr. Siemens has the best right to speak upon the relations between scientific education and the scientific industries. Himself a product of the educational system of Germany, and one of the foremost, if not in his own line the foremost, of scientific men in the industrial world, and in the land of his adoption, he yet uses no unmeaning terms when he tells us that the particular form of technical education afforded by that characteristic institution, the German Polytechnicum, "is certainly inapplicable to the condition of things which we find in this country."

The argument with which Dr. Siemens enforces this view is, so far as we are aware, a novel one from the educational point of view. He assumes frankly and without disguise that in any industry which is, like the railway system of Germany or our own telegraphic system, a Government monopoly, there is essentially a tendency to discourage improvements or any thing savouring of novelty or innovation. He also assumes that the system of "polytechnic" education fosters a like tendency, inasmuch as he thinks that, as administered in Germany, this system turns out students destitute of originality, and dogmatically persuaded that the particular machines or processes they have studied in the Polytechnicum are embodiments of perfection proved and established like propositions in Euclid. From these two premises the inference logically follows that although the Polytechnicum may be all very well for turning out young men fitted for Government appointments in a country where railways, mines, and factories are State establishments, such an institution is inappropriate in a country like England.

There is doubtless much force in this position, though the contrast between industrial conditions in Germany and England is hardly fairly represented by so sweeping a generalisation. If in a land of strong tendencies toward monopolism and conservatism the system of technical education has taken a similar bias, we should be disposed to argue that a complete system of technical education would, in a country where industrial enterprise is freer, tend toward a freer development.

On the other hand, Dr. Siemens sees plainly the inherent badness of the condition of things in England, where technical education has so long been neglected. He condemns *in toto* the old system of binding a lad to an apprenticeship of seven years' drudgery and mechanical routine, causing him, as such a system does, to give up thinking altogether; and is in favour of a much shorter term of pupillage.

Though he is not very explicit on the point, it is not difficult to gather the general drift of Dr. Siemens' views as to what system he would adopt in preference to the method of the German Polytechnic Colleges. Firstly, he would have science-teaching systematically incorporated in the educational curriculum of every school in the manner in which we have for years advocated, and in which Sir John Lubbock and many others have advocated it. He would also have science taught by practical work in chemical, physical, and mechanical laboratories attached to the schools. In the case of the industrial classes he would have mathematics and natural science taught to all lads before the age of fourteen, and would fix that as the *minimum age* at which they should be admissible to work in mines or factories. Were this done, he thinks a three years' apprenticeship would be amply sufficient to learn any mechanical trade; and he would lay upon the employer the responsibility of seeing that during this term the apprentice spent his evenings and his Saturdays in diligent attendance at some technical or technological class where the principles underlying the operations of his business would be taught him.

We cannot too heartily endorse this last suggestion, which is now the more appropriate when not only in the metropolis but in all our chief towns and cities such classes for pure and applied science are being held under the auspices of the Science and Art Department and of the City and Guilds' Institute.

Another point on which Dr. Siemens speaks with weight is the importance of providing an adequate supply of trained teachers. Those who know the history of the attempts to render the teaching of the science classes under the Science and Art Department of greater efficiency, will heartily unite in the satisfaction expressed by Dr. Siemens concerning the reforms now in progress by which the Royal School of Mines and its associated science classes will be reorganised and developed into a Normal School of Science. The neglect and apathy of previous Governments have been indeed deplorable; but it is to be hoped that the greatest of the acknowledged defects of the national system of science-teaching are now in a fair way to be efficiently remedied.

Dr. Siemens points out that while laboratory work in schools is necessary it is comparatively inexpensive, being elementary in character. But for the efficient training not of teachers alone, but of students who have advanced beyond first principles, the delicate and elaborate appliances of exact science are more than ever essential: and for that reason "very complete laboratories are of great importance at the universities and superior colleges, where exact science and independent research take the place of mere tuition of first principles." We trust these words will not be lost in the places where they are most needed. When we look at the large and complete equipment of the mechanical, physical, and chemical laboratories of the colleges and universities to be found in every large town in Germany, France, and Switzerland, and compare them with the utterly shabby and insignificant dens which go by these names in the science colleges of Newcastle, Bristol, and Leeds, we feel that by no means the least important point of Dr. Siemens' discourse is the paragraph we have quoted above.

The concluding remarks, in which Dr. Siemens alluded to the Electrical Exhibition in Paris as pointing the moral of the inevitable changes and improvements which are continually invading every branch of industry cannot fail to impress many whose experience will confirm the truth of the observation. The plain fact remains that in the race of industrial improvements England cannot afford to stand still. And if the Continental nations have in some respects stolen a march upon us in these last years, it is not yet too late to organise and develop a system of technical education of our own adapted to our own special industrial conditions and needs.

RECENT ORNITHOLOGICAL WORKS

The Ornithological Works of Arthur, Ninth Marquis of Tweeddale, F.R.S., etc. Reprinted from the Originals by the desire of his Widow. Edited and Revised by his Nephew, Robert G. Wardlaw Ramsay, F.L.S., etc., Captain 74th Highlanders. Together with a Biographical Sketch of the Author, by William Howard Russell, LL.D. 4to. Pp. i-lxiv., 1-760. (London: For Private Circulation, 1881.)

Ornitologia della Papuasia e delle Molucche, di Tommaso Salvadori. Parte seconda. 1 vol. 4to. 706 pp. (Torino, 1881.)

THE very handsome volume, in which the works of the late Marquis of Tweeddale have now been collected and published, forms a fitting monument of the labours of one of the best ornithologists that this country has ever produced, and its utility to working naturalists cannot be doubted. No one who knew the author of these memoirs will be surprised at the new aspect which is thrown upon his life by the publication of the biographical sketch which Dr. Russell has contributed, though to scientific men Lord Tweeddale was chiefly known as a laborious ornithologist and a thoroughly sound writer and critic; but from the volume now before us we read the highest testimony to his qualities as a soldier, and receive evidence that the same thoroughness which characterised his scientific work was also prominent throughout the whole of his military career. His first ornithological paper appears to have been published in 1844, and was a carefully-written essay; and then for the space of twenty-two years nothing bearing on his favourite study appeared from his pen. His biography, however, shows that during this lapse of time he was completely occupied with his military duties, serving throughout the Sutlej campaign, and later on taking part with the Guards in the memorable Crimean War; and even at this distance of time it is refreshing to read the clear and vigorous criticisms which his keen perception enabled him to make at that period on the conduct of military affairs in the East. Retiring from active service in 1863, he appears to have from that time devoted himself to the pursuit of his favourite science, and until his death, in 1878, he worked with unflagging zeal at the ornithology of the Indian region, amassing one of the grandest collections of birds which has as yet been seen in any country, and forming a library unsurpassed for its completeness in ornithological literature.

To his nephew, Captain Wardlaw Ramsay, who has inherited his scientific tastes, the late Marquis bequeathed his magnificent library and collection, and the pious duty has devolved upon him of editing a complete edition of his uncle's memoirs, at the request of Lady Tweeddale, who has hereby raised a monument to her husband's memory which will keep the latter green in the minds of ornithologists for many a long year to come. Many of Lord Tweeddale's most important observations were contained in letters or short papers to various journals, and there was always a possibility of their being overlooked; but by the publication of the present volume, with its complete indices and cross-references supplied by the editor, there will be no such risk in future. Lord Tweeddale's life forms a pleasing picture of what an English nobleman can do for science, if only his intelligence leads him in that direction; and many who are living can remember with pleasure the days spent beneath the hospitable roof at Chislehurst, where Lord Tweeddale was always glad to welcome any scientific visitors, for his love for scienee took a deeper turn than the mere following of his own particular branch of ornithology, and he always displayed an interest in every branch of intellectual study.

The perusal of his many excellent essays only deepens the regret that was felt by every one at the time of his decease, that a life only in its prime, and capable of doing so much good in every way, should have been prematurely closed.

In our notice of the first volume of Prof. Salvadori's work (NATURE, vol. xxiii. p. 240) we gave some account of the scope and origin of this great undertaking, and of the extensive materials upon which the author had based it. It is with great pleasure that we now chronicle the issue of the second volume, and record the announcement that the third, which will complete the work, is far advanced in preparation.

It will be recollected that the rich collections made by the Italian travellers D'Albertis and Beccari during their several expeditions into New Guinea and the adjoining islands, all of which came under the examination of Prof. Salvadori, were the "moving cause" of the present undertaking. Besides amassing numerous minor novelties and whole series of little-known species, these industrious explorers were the original discoverers of four new birds-of-paradise, several new pigeons and parrots of splendid plumage, and the large and fine bird-of-prey named *Harpyopsis Novæ-Guineæ*. It was an obvious, though by no means light and easy task, to weave together the numerous papers and memoirs in which the different collections had been described into a uniform series, and to supplement it by summarising what was previously known of Papuan ornithology. This is what Prof. Salvadori has undertaken, and the result will be an excellent work upon a subject with which our previous acquaintance was of a very fragmentary description.

In his first volume, published in 1880, Prof. Salvadori treated of the *Accipitres*, *Psittaci*, and *Picariæ* of the Papuan sub-region. In the second volume now before us the numerous army of *Passeres* comes under consideration, and swells its size to 706 pages. The plan of treatment pursued is exactly the same as that which we have described in the case of the first volume. Every species is fully and fairly described, its complete synonymy is given, and a detailed list of the specimens examined from the various localities over which the species is spread is added.

It appears that the Royal Academy of Sciences of Turin, which published Prof. Salvadori's first volume as one of their "Memorie," have unfortunately not found it convenient to adopt the same course as regards the second. The author is therefore compelled to appeal to his brother ornithologists to subscribe for copies of the second and third volumes of his most meritorious work, in which we are sure he will receive every sort of support. Few special works of the present day have been so well planned, or so thoroughly carried into execution.

OUR BOOK SHELF

Deschanel's Natural Philosophy. Edited by Prof. J. D. Everett. Sixth edition. (Blackie and Son, 1882.)

PROF. EVERETT'S admirable adaptation of "Deschanel's Natural Philosophy" is so well known as a text-book, that it needs no commendation from us. We heartily welcome this sixth and greatly improved edition. Amongst the new items we notice that the chapter on thermodynamics has been amplified and re-written; and other parts of the book devoted to heat have also been improved, particularly those relating to the apparent minimum density of water, and to conduction of heat. We notice also a useful note on the mathematical treatment of the periodical variations of underground temperatures. The section dealing with electricity and magnetism has also been greatly improved. The elements of electric testing by Wheatstone's bridge and resistance coils are now included. The modern dynamo-electric machines and such recent inventions as the electric pen and the induction-balance are described. Rowland's experiments on electric convection-currents, and Planté's secondary battery are also mentioned; though it appears to us that by a slight slip of the pen in the paragraph dealing with Planté's researches his "rheostatic machine," which is in reality a compound condenser of mica plates, is described as a species of commutator (like that of Muller) for his secondary batteries. There is another slip in the paragraph on the use of the galvanometer for measuring transient currents, for it is stated that the quantity discharged through the galvanometer is proportional to the swing of the needle, whereas by the well-known balistic formula of Maxwell, it is proportional to the sine of half the angle of the first swing. These are however minor points. In the section on Light and Sound little has been changed; the more recent measurements of the velocity of light, and the phonograph, being the most important additions. It is a pity that in the optical formulæ the editor does not use the same notation as in the accepted Cambridge text-books. The problems, which in former editions were lumped together at the end of the book, are in this new edition placed at the ends of the separate volumes, a change which is a great boon to teachers and students who find it most convenient to buy the separate parts. Why the date of 1882 should be put upon a work which appears in October, 1881, is one of the mysteries of publishing which lies beyond the pale of scientific criticism.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Struggle of Parts in the Organism

As the Duke of Argyll does not appear to have quite understood the meaning which I intended to convey in the paragraph of my review to which he refers, I should like to state that meaning a little more explicitly. This I should have done in the first instance had I not shared the feeling which he expresses, that "a purely scientific journal" is not an appropriate place in which to discuss the relations of science to theology, and I shall now hope to show that in my review I did not transgress the border of any such debatable ground.

My remarks were limited to the "Argument from Design as elaborated by the natural theologians of the past generation," the material of which was furnished by "the endless number and complex variety of those apparently purposive adaptations of structures to functions which are everywhere to be met with in organic nature." By this limitation I intended every one conversant with the writings of these theologians to understand that I alluded only to the Argument from Design as this was expounded by the school of Paley, Bell, and Chalmers, and which amounted to inferring that particular instances of adaptation were so many separate pieces of evidence pointing to as many "operations of special design." This is the form of teleology which I conceive Mr. Darwin's writings to have completely "subverted," for these writings have shown that in natural selection we have a general law whose operation is presumably competent to produce most of the adaptations previously ascribed to special design. This form of teleology is what I called in my review "scientific teleology," and I did so because it embodied what is, in the full sense of the term, a scientific theory; certain definite facts or results were observed, and of these results the immediate cause was inferred. Therefore this endeavour to explain the causation of special mechanisms in organic nature properly admits of being discussed in the pages of a scientific periodical; it is as purely a scientific hypothesis as is that of natural selection.

But the Duke of Argyll clearly attaches to the term "design" a much wider signification than that which I expressly and intentionally assigned to it. For he uses the term in its most unlimited sense, and says: "There are many minds, including some of the most distinguished in science, who not only fail to see any contradiction between evolution and design, but who hold that the doctrine of evolution and the facts on which it is founded have supplied richer illustrations than were ever before accessible of the operation of design in nature," &c., &c.

Here and elsewhere the Duke clearly alludes to the whole question of Theism, or of Mind as the First Cause, and not to the narrower one of this or that particular mechanism in nature as the result of immediate and special design. Now teleology in this larger sense, or the doctrine that behind all the facts open to scientific inquiry (special mechanisms, physical causes, and general laws) there is "Mind and Will" as the ultimate cause of all things—teleology in this sense is a general theory of things which it does not fall within the scope of scientific method to examine. In contradistinction to the cruder teleology of Paley, which, as I have said, may properly be called "scientific," this may be called "metaphysical"—if we use these terms as they are used by Lewes to denote respectively a theory that is verifiable (or the reverse) and a theory that is not. The school of Paley thought that the existence of a designing Mind in nature could be proved by a purely inductive method; Mr. Darwin has since shown that such is not the case; therefore this system of teleology is a scientific system, and, like many other theories of the scientific class, it has had to yield to fuller knowledge. But there remains the metaphysical theory of an ultimate design pervading all nature and blending into one harmonious cosmos what the Duke calls the "combination and co-ordination of physical causes"; and this theory, I quite agree with him, "no possible amount of discovery concerning the physical causes of natural phenomena can affect," either by way of proof or of disproof. But this has nothing to do with the special question between Darwinism and "the argument from design as elaborated by the natural theologians of the past generation"; and therefore I shall not discuss the merits of the theory in these columns.

GEORGE J. ROMANES

"The Micrococcus of Tubercle"

AN article on "Disease Germs," by Dr. W. B. Carpenter, in the current number of the *Nineteenth Century*, contains the following:—"Another line of inquiry which has obviously the most important bearing upon human welfare is the propagability of the micrococcus of tubercle by the milk of cows affected with tuberculosis, a question in regard to which some very striking facts have been brought before the Medical Congress by a promising young pathologist"—naming myself; and I hope that I am sufficiently grateful to a veteran in science for his complimentary if not altogether accurate reference to my work. What I did say at the recent Medical Congress, and at much greater length in a small volume entitled "Bovine Tuberculosis in Man" (London, 1881)—Dr. Carpenter will find it, I think, among his books—was not anything about "the micrococcus of tubercle," but about a variety of somewhat technical morphological details in respect to which certain cases of tuberculosis in man resembled the tuberculosis or "pearl disease" of the bovine species. I did indeed introduce half a page at the end of my essay to show how clear

was the issue between my view of tuberculosis communicated from the cow and the view which Dr. Carpenter has been expounding, and I hope you will have room for the passage:—"The doctrine of a tuberculous virus was stated by Klebs in 1868, and has been advocated by him, as well as by Cohnheim, in recent writings. In its latest form this doctrine asserts the existence of a specific minute organism to whose agency the infection is due. The minute organism is called by Klebs *Monas tuberculosum*. The method of proof which I have followed in this work makes it impossible that the infective agency of a minute organism should in any way come into my view of the communication of bovine tuberculosis to man. I have rested the whole case upon certain minute identities of form and structure in the infected body, due to the mimicry of infection. Among other points there were the leaf-like and cord-like outgrowths of the pleura and peritoneum, these being the early stages of the lentil-like or pearl-like nodules and their connecting threads; the lymphatic glands, with distinct nodular formations in their substance; the lungs, with smooth-walled closed vomicæ or with encapsuled nodules. In the new formations generally there was a particular pattern of microscopic structure, in which giant cells and epithelial-like cells figure largely, and there was a relatively high degree of vascularity. In all these points the disease in man is a mimicry of the parent disease in the bovine animal. That mimicry is not only in single features, but it is of the whole disease. It is possible to conceive of the juices and particles of the primarily diseased body acquiring a kind of spermatic virtue which gave them the power to communicate the specific disease as a whole and in all its several manifestations to another body in which they should happen to lodge. But it is hardly possible to think of a neutral living organism being charged with the power of conveying so complex details of form and structure from one body to another" ("Bovine Tuberculosis in Man," pp. 103, 4).

25, Savile Row, W., October 24 C. Creighton

A Kinematical Theorem

Prof. Minchin's Theorem in Nature (vol. xxiv. p. 557) may be proved easily by considering the motion as due to the rolling of one closed curve on another back into its first position, their lengths being of course commensurable. If you measure y for the rolling curve from the straight line which forms the envelope, and x along that line, then the differential of the area between the envelope and the fixed curve is easily seen to be $ydx + \frac{1}{2}y^2dw$, where dw is the angle turned through by the rolling curve, and is equal to ds multiplied by the sum of the curvatures at the point of contact, which we shall call σ. The summation of the former part is a multiple of the area of the rolling curve, and therefore the same for all lines; that of the latter is half the moment of inertia of matter distributed over its perimeter with density σ, about the line in question. The result is therefore the well-known property of equi-momental ellipses. Similar reasoning, with the use of the property of the centre of inertia of a system, leads to the further result that when the perimeter of the envelope is of constant length, the line touches a circle, and different values of the constant correspond to concentric circles. In the same way by a property of the centre of inertia we may also prove immediately the known theorem that when the area traced out by a point is constant, the point lies on a circle, and different values of the constant correspond to concentric circles; and we may extend it to areas traced on a sphere. Joseph Larmor

54, Antrim Road, Belfast

If Prof. Minchin will refer back to the *Bulletin des Sciences Mathématiques et Astronomiques* for August, 1878, he will, I think, find in a paper by M. Darboux the theorem stated by him under the above title in Nature, vol. xxiv. p. 557.

C. Leudesdorf

Pembroke College, Oxford, October 21

"The Dark Day"

Referring to the account of the phenomenon in New England on September 6 last (Nature, vol. xxiv. p. 540), and in Mr. Harding's letter (p. 557), let me refer your readers to a succinct account of the occurrence on May 19, 1780, which they will find in Webster's Dictionary, "Explanatory and Pronouncing Vocabulary of the Noted Names of Fiction, &c." In *Public Opinion* (June 4, 1881) there is an account of a precisely similar occurrence on the morning of Sunday, November 8, 1819, known, it is remarked, as the "Phenomenon of 1819." The account of this phenomenon is very explicit, and the details furnished correspond so closely with the event of May 19, 1780, that a doubt might be felt whether there had been two such days, or whether there had not been some mistake made in regard to the date given. I wrote to *Public Opinion*, making inquiries (see *Public Opinion* of June 11, 1881, p. 755), but no reply has hitherto appeared to my inquiries. I may observe that the year 1819 would not coincide with any one of the sun-spot cycle of eleven years from 1780 to which the New York *Nation* refers. A. Trevor Crispin

6, Melbury Terrace, Harewood Square, N.W., October 22

OWENS COLLEGE SCIENCE AND LITERATURE FELLOWSHIPS

THE first award of these Fellowships, of the annual value of 100*l.* each, which are intended to encourage original investigation, was made on Friday last by the Council of the Owens College. These are remarkable as being the only fellowships given in any University or College in the United Kingdom solely for the encouragement of research. They are not awarded on the results of examination, but after consideration of documentary or other evidence. Every holder of a fellowship is expected to devote his time to the prosecution of some special study, and before the close of the year to give evidence of progress by the preparation of a thesis, the delivery of a lecture, or the completion of some research. He may also be called upon to render some service to the College either by acting as occasional examiner or by giving instruction by lectures or otherwise to the students.

Of the thirty candidates four gentlemen were elected to Fellowships. Of these one is awarded to Mr. Alfred Sidgwick, B.A. of Lincoln College, Oxford, in the Department of Logic; two were awarded in the Department of Chemistry, namely, one to Dr. Bohnslav Brauner, of the University of Prague, who has already published several papers on original subjects, some from the laboratory of the Owens College; and a second to Mr. Harry Baker, Dalton Chemical Scholar of the College, who has likewise published several papers in the *Journal* of the Chemical Society. These two gentlemen will continue their researches, devoting the whole of their time to original investigation. In the Department of Biology an award has been made to Mr. H. Marshall Ward, B.A., F.L.S., of Christ College, Cambridge, at one time a demonstrator in the Owens College, who has recently distinguished himself as Government cryptogamist in Ceylon, in an investigation of the cause of the coffee disease.

THE AGE OF THE IGNEOUS ROCKS OF ICELAND

DURING a recent visit to the south-west part of Iceland, one or two points connected with the general geological structure of the island came under my observation, which I do not remember to have seen noticed before, and which seem to me to be of sufficient interest to be put on record. It is well known that the rocks of the island are of very different ages, some going back to the Miocene period, while others are quite of yesterday's date. It is also perhaps a general belief that the volcanic forces may have continued to be more or less active from the time that the older Miocene basalts and tuffs were erupted down to our own day. I doubt very much whether there is any evidence to justify this conclusion, and will presently mention some of the facts which lead to a very strong suspicion that a prolonged period of repose supervened after the accumulation of the Miocene rocks, and before the eruption of the later lavas, &c., had begun. The Miocene group consists of a vast

series of basalt-rocks with interbedded layers of palagonitic tuff, &c. These rocks, so far as my observations go, exactly resemble those of the Færöe Islands. The basalt-rocks are chiefly anamesites, but some are true basalts, while others are dolerites. But in the areas traversed by me I saw none so coarse-grained or so highly porphyritic as those which occur so abundantly in Stromöe, Österöe, and other islands of the Færöes. They form lofty plateaux, deeply gashed with gorges, and abruptly truncated, so as to present bold cliffs and precipices to the low grounds at their base, as in the case of the Esja near Reykjavik. Moreover, they appear to be developed chiefly in the maritime districts. Only a glance at these basaltic masses is needed to convince one that they are the mere fragments of what must once have been a most extensive plateau. The Esja, built up chiefly of comparatively horizontal beds of basalt, tuff, &c., rises to a height of nearly 3000 feet above the low tracts at its base. Nor can there be any doubt that these beds formerly stretched far away in all directions, and that they have since been removed by the various agents of denudation from the broad undulating low grounds, over which they may still be traced, sometimes continuously for many miles, at other times in sporadic hills and rising grounds which peer above the surface of the recent lavas by which they are surrounded. In short, the Miocene basalt-rocks of Iceland present precisely the same features as the similar rock-masses of the Færöes. Like the latter they probably formed at one time a wide elevated table-land, which has since been cut down and worn away—the lofty walls of the Esja, &c., serving to give us some idea of the enormous erosion that has taken place. Now all this vast erosion had been effected before any of the later lavas, agglomerates, tuffs, &c., in the south-west part of Iceland were erupted. In the region between Hafnarfjörd and Krísuvík the lavas have poured through old valleys in the Miocene rocks and spread themselves out over the highly denuded surface of the latter in the opener low grounds. In a word, it is evident that in the south-west part of Iceland a long interval separates the accumulation of the Miocene basalt-series from the eruption of the later volcanic rocks, and I incline to think that the same break in the continuity of volcanic action will be found to hold true for the rest of the island. I believe it will be found that there is no more connection between the display of volcanic activity in Miocene times and that of the present day in Iceland, than there appears to have been between the volcanic action which manifested itself in Scotland at such widely separated periods as those of the Lower Old Red Sandstone and the Carboniferous. Had there been more or less continuous volcanic activity in Iceland from Miocene times down to the present, we might well be surprised that the later volcanic masses are not much more considerable than they are. If we think of the time required for the removal by denudation of some 3000 feet of basalt-rocks, &c., over thousands of square miles, we must be prepared to admit that the volcanic forces cannot have been continuously active. Either they have not been so, or the denuding agents have far surpassed them in energy.

There is another point which interested me. I found that the whole of the south-west region had been glaciated before the eruption of the later volcanic series. The Miocene basalts are everywhere ice-worn and abraded; *roches moutonnées* are well-marked, and in many places glacial ruts and striæ are conspicuous. Glacial gravels and coarse boulder-clay are likewise sprinkled over the surface of the low-lying tracts. Between Reykjavik and Hafnarfjörd the glaciation is distinctly from south-east to north-west, and could not have been the result of any mere local glacier. The whole wide tract has been overflowed by a general *mer de glace*. And if this be the case with that part of Iceland which now enjoys the mildest climate, we may be sure that the rest of the island must likewise have been enveloped in ice during the Glacial period. In the south-west region all the traces of glaciation are strictly confined to the Miocene areas. Nothing of the kind is visible upon any of the later volcanic rocks. These last have flowed over a glaciated surface, for the ice-worn Miocene basalts terminate abruptly at the margins of the wide sheets of black scoriaceous lava, as do also the drift-accumulations of glacial gravels and erratics, while now and again ice-worn knolls of basalt-rock may be seen rising up like islands in the midst of the later lava-fields. Everywhere the lavas and their associated agglomerates and tuffs show their original surfaces—the only changes which they have undergone being the result of subaërial weathering. In a word, all the post-Miocene eruptions of the south-west are of later date than the Glacial period. It would be interesting to ascertain whether the same is the case throughout Iceland. As there is every probability that the great break in the continuity of volcanic action, of which I have spoken, is not confined to the south-west, but may hold true of the whole island, it seems not unlikely that the conclusions I have formed as to the post-Glacial age of the later volcanic series of the south-west will also be extended to the same series in other districts. In other words, we may yet be compelled to admit that the oldest eruptions of Hecla and her sisters are not only of vastly more recent age than any of the Miocene basalt-rocks, but belong to one of the latest epochs of which geology takes cognisance. JAMES GEIKIE

THE EVOLUTION OF THE PALÆOZOIC VEGETATION

SOME statements made in Mr. Starkie Gardner's abstract (NATURE, vol. xxiv. p. 558) of the recent work of Saporta and Marion "On the Evolution of the Cryptogams" are so opposed to conclusions at which I have arrived that I can scarcely allow them to pass unchallenged, lest by doing so it may be inferred that I no longer oppose the French school of Carboniferous palæo-botanists on several vital points connected with the interpretation of the Carboniferous flora. But before doing so I may venture to suggest a doubt whether the time has yet arrived for making the attempt to trace the lines of descent of the Palæozoic flora. It is true that much has been done of late years to extend our knowledge of that flora, but perhaps at the same period our knowledge of the extent of our ignorance has, *pari passu*, been equally enlarged. We now possess accurate information respecting the structure of many well-known plants, but we have also obtained glimpses of the existence of many obscure but very important organisms which represent factors that cannot be left out of consideration in dealing with the problem of their evolution. Besides this, opinions of experts are widely divergent on some very important questions of interpretation affecting the relationship of conspicuous plants whose organisation is understood. So long as experienced palæontologists are disagreed on the relations of the Calamites to the Calamodendra, and of the Lepidodendra to the Sigillariæ, a scheme of evolution explaining the development of the Carboniferous flora can scarcely be possible. The French school of botanists still believe that what they call Calamites are Equisetaceous Cryptogams, whilst the Calamodendra are Gymnospermous Phanerogams. In like manner they believe the Lepidodendra to be Cryptogams, and as such to be devoid of all exogenous growths in the exterior of their stems, whilst they regard all the Lepidodendroid stems that possess such growths as Sigillariæ, and relegate them also to the Gymnospermic section of the vegetable kingdom. I am more than ever convinced that these views cannot be sustained, and I think that my memoirs on these subjects, especially Parts IX. and XI.,

contain a sufficiently abundant array of detailed facts to justify the conclusions at which I have arrived.

But even were this not the case, there are other important considerations that cannot be overlooked. As I have already hinted, we have become acquainted with a large number of curious organisms, many of which are unmistakably reproductive, but respecting the botanical affinities of which we are as yet entirely ignorant. New forms present themselves in a more rapid ratio than discoveries are made of the true character of older ones. Yet many of these objects are so remarkable that they must have constituted very important links in the chain of Palæozoic life; and until we learn more about them than we at present know, we cannot possibly assign to them their true place in that chain; whilst their omission must leave serious gaps in the succession.

But our difficulties do not end here. All the objects to which I have just referred have been discovered but recently. Ten years ago we knew nothing of their existence, and new forms are still being added to our cabinets. The old fossiliferous shales and sandstones revealed no traces of them. We only found them when the microscope came to be applied to the calciferous nodules of Oldham and Halifax. Our first supply of special types was derived from the former locality. The examination of the Halifax nodules revealed the existence of several new forms, though obtained from the same geological horizon and from localities but a few miles apart. Arran and Burntisland have, in like manner, contributed types wholly unknown in Yorkshire and Lancashire, and the French localities of Autun and St. Étienne (where also are found Carboniferous plants of which all the structure is preserved) have each their own characteristic forms.[1] We thus learn that so far as these six special localities are concerned, whilst certain common features characterise their floras, each locality has, as in living floras, genera or species peculiar to itself. Now we chiefly know the full extent of the localisation of these six Carboniferous floras from their accidental preservation in calcified or siliceous deposits, and not from the revelations of the ordinary fossiliferous shales and sandstones. But we cannot suppose that the six localities enumerated are the only ones that possessed floras peculiar to themselves. Does not common reasoning justify the suggestion that all Carboniferous plant-bearing localities would exhibit similar features, had their fossils been preserved as they are at Halifax or at St. Étienne? If so, seeing how widely Carboniferous deposits are diffused throughout the world, what myriads of minute, but phytogenetically important forms of plant-life must have existed of which we are absolutely ignorant—an ignorance that can only be diminished by the discovery of other localities as productive as the six that I have enumerated.

But even were we perfectly acquainted with the Carboniferous flora, we should not be much nearer the end. Beyond the fact, established by Dr. Dawson, that in the Devonian age a flora existed almost, if not wholly, as rich as the Carboniferous one, a flora in which Gymnosperms existed with as high an organisation as characterised the similar Carboniferous types, what do we know respecting the minuter forms of this flora, which correspond to those which I have described from the Coal-measures? But can it be doubted that such objects must have existed in abundance? Still less can it be supposed that so rich and highly organised a flora as that of the Devonian age first sprang into existence during that age. That flora must have been preceded by one rich in types of a lower terrestrial vegetation than is represented by the ferns—the Lycopods and the Dadoxylons of the Devonian beds of North America. But what do we know of this earlier flora? Almost nothing. The remains of pre-Devonian plants now known are so obscure that little reliance can be placed upon them. Eophyton is rejected from the vegetable kingdom by Nathorst, and most of the other so-called Fucoids of the Palæozoic strata are of almost equally dubious nature. Where more definite forms of what may probably be Marine Algæ do occur they come too late in time to avail in the construction of the Palæozoic pedigree. Even the Liassic *Chondraites bellensis* of the Lias cannot be depended upon with absolute certainty. It is only when we reach the Tertiary age that we find the Delesseriæ and Halymenites in shapes that leave little room for doubting their true nature. Yet our French friends trust to these dubious objects as being real Fucoids, and as such, the ancestral predecessors of the higher Cryptogams of the Devonian and Carboniferous ages. So long as this ignorance and uncertainty remain, it seems to me that we cannot construct, with any degree of probability, the genealogical tree of Palæozoic plant life.

As to the many detailed conclusions arrived at by MM. Saporta and Marion, I will only refer to two or three statements in addition to the more important ones to which I have already called attention. Thus Mr. Gardner's abstract states that "eight still existing Diatoms have been discovered in British Coal." I thought that I had thoroughly exploded that fallacy in my Memoir, Part X. MM. Saporta and Marion conclude that Asterophyllites was a floating or procumbent plant allied to the Equisetaceæ, thus following M. Renault in separating it from Sphenophyllum, which the authors believe to be a Rhizocarp allied to Salvinia. I see no ground whatever for these conclusions. They further consider that some of his Calamariæ (Equisetaceæ) were heterosporous. They arrive at this conclusion from my discovery that *Calamostachys Binneana*, which I believe to be a fruit of an Asterophyllitean plant, was a heterosporous Strobilus; but I wholly demur to the idea that either the plant or the fruit was Equisetaceous.

For the reasons above given, I doubt whether even my valued friend the Marquess Saporta, highly accomplished as I know him to be, will be able to "make clear the precise lines through which the evolution of the one from the other [*i.e.* the Phanerogams from the Cryptogams] has been accomplished." WM. C. WILLIAMSON

Owens College, Manchester, October 14

[1] I believe that this fact partly explains the unwillingness of the French palæontologists to accept our English views as to the close affinities existing between the Lepidodendra and the Sigillariæ. The peculiar Diploxyloid forms of Lepidodendron, *i.e.* those which possess the outer exogenous zone which the French botanists regard as characteristic of a Sigillarian stem, appear to be absent from the beds of Autun and St. Étienne, as they are rare in Canada and the United States. In Great Britain, on the other hand, they constitute, with several variations of specific details, our prevailing type.

THE INTERNATIONAL EXHIBITION AND CONGRESS OF ELECTRICITY AT PARIS[1]

V.

THE labours of the jury are now finished, and the distribution of medals took place on October 21 at the Conservatoire des Arts et Métiers. It is understood that they have been somewhat liberal in their distribution of honours, and have endeavoured to make things pleasant all round. Indeed the time allotted to them for investigation being postponed for a week at the beginning, and afterwards cut short by a week at the end, was quite insufficient to settle the burning question which is the best of all the electric lights.

The diploma of honour (*diplôme d'honneur*), which is the highest award of all, has been voted to Dr. Werner Siemens, Sir William Thomson, Mr. Edison, M. Gramme, Prof. Graham Bell, Prof. Hughes, Prof. Pacinotti, Prof. Bjerknes, M. Gaston Planté, M. Baudot, and M. Marcel Deprez, the last-named being the inventor of a system of distribution of electricity which has found much favour in Paris. M. Baudot is the inventor of a multiple printing-telegraph. The Exhibition has been announced to close on November 15, but there is some talk of a later date.

[1] Continued from p. 589

During the last week a body calling itself the Réunion International des Électriciens has been holding meetings in a room granted for the purpose in the Exhibition building. It is understood to be mainly composed of persons who felt slighted at not being appointed members of the Congress, and are determined to have a little congress of their own; but their movements have not attracted much public attention.

As this will be our last article, we will endeavour to supplement our previous accounts by some information on what must be regarded as the most important of all the objects in the Exhibition, namely, the machines which generate the electricity. Those which have permanent steel magnets are few in number, and the only large ones are the machines of De Meritens. These usually give alternating currents, but can be made to give direct currents by a change in the connections. The principal type contains five Gramme rings mounted on the same axis, each of them surrounded by eight horseshoe steel magnets with their feet inward. The introduction of the Gramme ring is the chief difference between this machine and the old lighthouse machine of Holmes. The great bulk of the machines in the Exhibition are dynamos, in which the whole current produced passes through the coils of the field magnets, and a large proportion of them are of the Gramme type, generally with one pair of straight massive field magnets arranged in one line above the ring, with a pair of like poles near together close to the ring, and with another similar pair below of opposite polarity to the first pair. The ring thus revolves between two very strong poles outside it, and massive iron pole pieces are usually employed, so shaped as to embrace a considerable arc of the ring. These are the machines for direct current. In the alternating current Grammes, the ring is generally broadened out into a hollow cylinder whose length is as great as its diameter. Sometimes this revolves between four external pole pieces attached to electro-magnets, and sometimes it is fixed, while four broad electro-magnets radiating from the common axis revolve within it. In some examples a separate exciter giving a direct current is mounted on the same stand and on the same axis.

The three firms of Siemens at Berlin, Paris, and London have a very large and diversified collection, partly historical, partly representing the commercial demands of the present day, and partly embodying their latest ideas for future improvement. The prevailing pattern is the well-known Siemens direct acting machine, in which an armature in the form of a cylinder, about three times as long as it is broad, rotates between two sets of pole-pieces, one above and the other below, of opposite polarity produced by the action of four straight flat and massive electro-magnets. The coil of the armature is wound, as nearly as the presence of the axle permits, in planes containing the axis, so that the wires cross one another at all angles at the two ends.

The most remarkable novelty that struck us in going over their collection was a machine in which two armatures consisting of cylindrical iron cores, each inclosed between four longitudinal segments of copper, revolve within two hollow cylinders of iron, which are the poles of a composite magnet, so that each armature is surrounded by a pole of one name, while opposite polarity is induced in the outer part of the iron core. The lines of force thus radiate from the common axis with complete symmetry, and the longitudinal coppers cut these lines at right angles in every position, so that the electromotive force in each copper remains constant as the armature revolves.

A peculiar adaptation of the ordinary Siemens armature has been made by Mr. Edison. The conducting portion of his armature consists of bars and disks. The bars form the outside of the cylinder, and the disks, with mica between for insulation, are built up into two solid masses which form the ends. The intervening portion is occupied by the core, which consists of a thousand or more very thin disks of iron separated by silk paper. The course of the current is nearly the same as in a Siemens armature, being first along a bar, then across a disk, then back along an opposite bar, then across another disk, and so on. The ends of the bars are disposed along two helical curves at the two ends of the cylinder, each helix having two convolutions. The object of having such excessively thin iron plates is to promote rapid demagnetisation and to avoid the formation of induced currents in the iron. This monster machine has only recently arrived, and is not yet ready for action. Its armature (to which, as well as to that of a smaller machine, the above description applies) is about four feet long by two in diameter. It has two straight and very long field magnets, which are actuated by a branch of the main current of the machine.

A very common pattern of machine for alternating currents, which one sees under various names, has a number of flattish cylindrical coils disposed in circular fashion like the holes of a siren, and revolving in siren fashion between pairs of fixed cylindrical field magnets of more massive appearance, the number of pairs of these fixed magnets being equal to the number of revolving armatures.

There are also some direct current machines of this construction. They can be distinguished by having a commutator of many segments on which the brushes rub to collect the currents, while the alternating machines give off their currents from two insulated rings which are not divided in any way.

Last Saturday evening there was a special gala at the Opera House in honour of the Electrical Congress, admission being by presentation ticket. In addition to the ordinary operatic performances there was a somewhat stilted poem in celebration of the achievements of electricity, which was read between two of the pieces by an eminent comedian; and the whole performance wound up with a grand chorus calling on the earth to light itself up. Preparations had been made for illuminating the house by electricity, but they were far from complete, and gas was decidedly in the ascendant. The place where the telephonic transmitters are bestowed was easily recognised, there being a wooden screen about ten inches high and six feet long on each side of the prompter's box.

We stated in a previous letter that a committee of jurors had undertaken some quantitative experiments on the machines and lamps. These are still going on, and will probably be continued till the Exhibition closes.

The chief practical result of the Congress has been the agreement to adopt the British Association system of units, and we understand that Prof. Everett's book, which is the recognised exposition of this system, will be immediately translated into French, German, and Italian.

THE IRON AND STEEL INSTITUTE

ONE of the most interesting features connected with the recent meeting of the Iron and Steel Institute was the fact that the Arsenal authorities abandoned at last the official reserve which has so long been complained of, and descended into the arena of professional discussion by reading papers on the manufacture of ordnance, projectiles, small arms, and gun-carriages, and submitting them to public criticism. We must specially congratulate Col. Maitland, the present distinguished head of Woolwich Arsenal, on having had the courage to take this step. His paper on the Metallurgy and Manufacture of Modern British Ordnance was extremely interesting. Its production also was well timed, coming at a period when the confidence of the public was consider-

ably shaken in the management of the Royal Arsenal, by the bursting of the *Thunderer's* 38-ton gun. Col. Maitland reviews in succession the early history of Steel *versus* Iron, the successive improvements in the manufacture of gunpowder, the processes of the manufacture of the iron and steel, the building up of the gun, and the boring and rifling of the barrel. The paper concludes with a description of some of the special tools and furnaces in use at the Arsenal. As regards the question of powder, it is satisfactory to find from an official utterance that the problem of the proper action of gunpowder is at last thoroughly understood. On this point the author states, "With the large slow-burning powders now used, long heavy shell move quietly off under the impulse of a gradual evolution of gas, the pressure of which continues to increase till the projectile has moved a foot or more; then ensues a contest between the increasing volume of gas, tending to raise the pressure, and the growing space behind the advancing shot tending to relieve it. As artillery science progresses, so does the duration of this contest extend further along the bore of the gun towards the great desideratum, a low maximum pressure long sustained." To this last sentence we call particular attention, for in the attainment of this object by our powder manufacturers lies the whole possible development of the power of artillery. When the author uses the words *low* maximum pressure, we take it that the expression must be understood in a relative sense only, and that the maximum pressure should not be high as compared with the mean; what is in reality the great desideratum is as high a maximum pressure as is consistent with the strength of the gun, sustained throughout the entire length of the bore. How far this object is from being attained at present can be seen at a glance from the shape of even the most modern heavy gun, which is very thick at the breech and dwindles down to almost nothing at the muzzle, showing that the pressures at the breech are still far from being sufficiently sustained. The problem here is more one for powder-makers than artillerists. The latter can but indicate what is wanted. It seems from *à priori* grounds impossible to expect that the solid pebble powder now in use, burning as it does from the surface to the centre, can ever give off the increasing volumes of gas wanted in order to fill up the spaces behind the advancing projectile, and thus maintain the pressure. It is, we believe, no secret that the results attained with our home-made powders are inferior to those furnished by the perforated prismatic powders made in Germany and Russia.

In dealing with pure metallurgical processes Col. Maitland made a great mistake in not making himself acquainted beforehand with the name of the inventor of the process of making steel adopted at the Royal Arsenal. Nearly two pages of the paper are taken up with a description of the process invented by Dr. Siemens, which is described in detail without any reference to that distinguished engineer, so much so that any uninitiated person reading the paper would have inferred that the process was peculiar to the Royal Arsenal. It is true that in the discussion which followed Col. Maitland disclaimed any originality for the Royal Arsenal, but it seems, to say the least of it, curious that he should have occupied so much space in describing a process which was perfectly familiar to everybody in the audience, had he been aware that it was in use in every civilised steel-producing country in the world. Of course this line of conduct compelled Dr. Siemens to speak in his own defence, and the stories which he told of the conduct of the Royal Arsenal authorities towards himself ought to have been sufficient to make Col. Maitland ashamed of some of his own predecessors in office, or of their immediate superiors in Pall Mall. Not only has the Siemens open hearth process of steel manufacture been appropriated without thanks or even acknowledgment, but on a former occasion they endeavoured to imitate his regenerative furnace without his cognisance, by employing a former draughtsman in his office. The furnace failed, having cost the country some thousands of pounds, and then, and not till then, was Dr. Siemens' help called in. It is really time that the Government claim to appropriate all patents without consulting or rewarding the owners should be inquired into, for the present policy cannot even be commended on the score of economy, for in the case in point the blundering of inexperienced engineers cost the country far more than the few hundred pounds of royalty which would have been due to Dr. Siemens.

We are glad to infer from this paper that there is some hope that wrought iron will shortly be entirely superseded by steel in the manufacture of ordnance. Really the caution in this matter which has been hitherto observed at Woolwich exceeds the bounds of prudence and sense. For years past the most eminent metallurgists and users of steel in every branch of manufacture have over and over again declared publicly that steel is in every respect, including ductility and toughness, vastly superior to iron, but we still find at Woolwich Arsenal that wrought iron is used for all the coils of a gun. All that Col. Maitland can bring himself to say on this point is, "but now that the pressures" (of the powder) "are longer sustained, it becomes advantageous to thicken the inner tube of steel, and it will most likely be found beneficial to support it with steel in place of wrought iron." We welcome the conclusion, though we fail altogether to appreciate the soundness of the reasoning which has led up to it; for if it is advantageous now when pressures are weak to use the stronger and tougher material, it must have been doubly so when the internal strains generated by the powder were greater than at present. The remainder of this paper calls for no special comment. It is undoubtedly interesting as an official statement, but in style it seems to us to betray the fact that the author is dealing with information which he has only recently acquired; otherwise how does he betray himself when addressing an audience composed exclusively of technical men into dwelling with minuteness on such trivial details as, for instance, the use of soap and water as a lubricant for cutting tools, in place of oil? Surely he ought to be aware that the same practice obtains in nearly every workshop in the country.

Of the remaining papers read before the Institute, one by the Assistant-Superintendent of the Enfield Small Arms Factory was a mere chronicle of the various details of the manufacture and inspection of Martini-Henry rifles and bayonets. Another, by Mr. Butter of the Royal Arsenal, was a short account of the application of steel and iron to the manufacture of gun carriages and slides. The last paper which we shall notice was by M. Ferdinand Gautier of Paris, on the Application of Solid Steel to the Manufacture of Ordnance and Small Arms. M. Gautier had already communicated two papers to the Institute on the remarkable Steel Castings of the Terre Noire Company. The peculiarity of the castings of this Company is their freedom from blow-holes, which is attributed to the rather considerable percentage of silicate of manganese used in the manufacture.

At Bofors, in Sweden, the same process is used with perfect success in the production of steel barrels for artillery.

The following analysis is given of the material produced:—

Carbon	Silicon	Manganese	Sulphur	Phosph.
0·45 ...	0·351 ...	0·540 ...	Traces ...	0·042
0·40 ...	0·322 ...	0·612 ...	0·02 ...	0·045
0·50 ...	0·183 ...	0·360 ...	0·02 ...	0·040

The tests of this steel, both ordinary tensile tests and in guns, when fired with heavy proof charges, are stated to have given most satisfactory results.

NOTES

On Monday a preliminary meeting was held in the Mansion House in furtherance of the scheme of the International Electrical Exhibition which it is proposed to hold at the Crystal Palace on a very large scale in the winter months. There were present, among others, Mr. William Spottiswoode, Mr. John Holms, M.P. (one of the Lords of the Treasury), Mr. Mungo M'George (Chairman of the Crystal Palace Company), Capt. Douglas Galton, C.B., Dr. Gladstone, F.R.S., Col. Gouraud, Dr. J. Hopkinson, F.R.S., Mr. C. V. Walker, F.R.S., and many more. Mr. Mungo M'George, in moving the appointment of an influential honorary council to advise with the directors of the Crystal Palace in carrying out the proposed exhibition, said that no effort should be wanting on their part to make the scheme a great scientific and commercial success. The honorary council was formed of those present, and, among others, the Lord Mayor Elect, the President of the Institute of Civil Engineers, Dr. C. W. Siemens, Prof. Adams, Sir H. Cole, Prof. Fleeming Jenkin, Mr. W. Crookes, Sir E. J. Reed, M.P., Sir Edward Watkin, M.P., Sir Herbert Sandford, and many more. Major Flood Page, the manager of the Crystal Palace, read a report, which stated that communications have been opened with the leading exhibitors at the Electrical Exhibition in Paris, and with others who have made the development of electricity their special study; and, although but a very short period has elapsed since the first steps were taken, the responses have been such as to render it certain that an effective and varied display will be made at the Crystal Palace. Most of the best-known systems of electric lighting will be represented—among others, the Siemens, Brush, British Electric, Electric Light and Power Generator Company's systems, the Joel, Pilsen, Edison, Swan, Maxim, Weston, Lontin, Rapieff, and Gerard lights; and various new lamps will be exhibited for the first time in public. The storage of electricity will, it is hoped, be illustrated by Faure's and De Meritens' secondary batteries. Telephones, which are not nearly so much used in England as elsewhere, will be strongly represented; and the various applications of electricity as a motive power will be seen in Trouvé's boats and other interesting exhibits. Many eminent scientific men have expressed great interest in the undertaking, and intend to become exhibitors. Colonel Gouraud promised all the help of his fellow-countrymen towards the success of the Exhibition, which, though following that at Paris very sharply, might be more attractive to American exhibitors, for it would be one stage nearer home, and its arrangements would be conducted in a language which the exhibitors could understand. Capt. Galton expressed a hope that military and submarine electricity would be suitably and adequately represented on the occasion. Sir James Anderson also supported the proposal, which was carried unanimously. Major Flood Page then read a *résumé* of the arrangements for the exhibition, which stated that the principal objects to be admitted were comprised in the following:—Apparatus used for the production and transmission of electricity; magnets, natural and artificial; mariners' compasses; applications of electricity—to telegraphy and the transmission of sounds, to the production of heat, to lighting and the production of light, to the service of lighthouses and signals, to apparatus giving warning, to mines, railways, and navigation, to military art, to fine arts, to galvano-plastic, electro-chemistry, and to chemical arts, to the production and transmission of motive power, to mechanical arts and horology, to medicine and surgery, to astronomy, meteorology, geodesy, to agriculture (in its application to industries), to apparatus for registering, to domestic uses, lightning conductors. Major Page earnestly hoped that Mr. Fawcett would allow the Post Office exhibit at Paris to be shown at the Crystal Palace, and that Mr. Childers, as Secretary of State for War, would give aid to experiments in electricity as applied to military purposes.

At the first meeting of this session of the Birmingham Philosophical Society, the Rev. H. W. Crosskey (secretary) read the annual report, in which it was stated that the Council last year reported that Dr. George Gore, F.R.S., had accepted the position offered him, and that the amount of 150*l*. per annum had been allotted to him in order that he might have greater facilities for continuing in Birmingham his original researches. Dr. Gore had forwarded a report stating that since he had been intrusted with grants from the Birmingham Endowment of Research Fund, he had made, partly with the aid of those grants, the following researches in physics and chemistry, which had been communicated to the Royal Society, and published, as follows:—Thermo-electric behaviour of aqueous solutions with platinum electrodes; influence of Voltaic currents on the diffusion of liquids; experiments on electric osmose; phenomena of the capillary electroscope; electric currents caused by liquid diffusion of osmose; influence of Voltaic currents on diffusion of liquids; and phenomena of the capillary electroscope. He hoped before long to submit to the Philosophical Society an original communication. In addition to the before-mentioned researches, and as an entirely separate matter, he had been aiding the cause of original research by preparing for publication a small book on "The Scientific Basis of National Progress," and it was now being printed.

A new zoological station is to be established at Banyuls-sur-Mer, on the Mediterranean, at the end of the natural prolongation of the mole at the beach of Fontaulé. The building will be of considerable size, have several apartments, and be well lighted. It is expected that the laboratory will be ready for work by January. M. Lucaze-Duthiers intends to illuminate the aquarium by electricity. This station will really be an annexe to that at Roscoff, permitting the study of marine zoology to be carried on in winter, when it has often to be suspended on the colder coast of the Atlantic. The municipality of Banyuls, mostly very humble individuals living in an out-of-the-way place, have lent cordial and substantial support to the enterprise.

The arrangements for the festival in honour of the twenty-fifth anniversary of Virchow's appointment as Professor to the University of Berlin—an anniversary which coincides with his sixtieth birthday—are now being made, we learn from the *Lancet*. An influential committee, comprising the names of Prof. Bastian, Director of the Royal Ethnological Museum, Town-Councillor Friedel, Prof. Küster, Dr. Voss, Herr Ritter, &c., have asked permission of the Town Council to grant the use of the large hall in the Rathhaus and to defray the cost of the decorations, as on the occasion of the banquet to Dr. Schliemann. The 19th of November has been fixed for this festival. The most interesting part of the proceedings will be the handing over to Prof. Virchow the title-deeds of a new institution to be devoted to the prosecution of scientific researches especially relating to anthropology, of which he will have the full control. As a politician, an anthropologist, and an antiquarian, no less than as a pathologist, Prof. Virchow has claims not on Germany alone, but on the whole of civilised humanity; and we heartily join in the desire to do him honour.

The first general meeting of the London Sanitary Protection Association was held on Tuesday at the Society of Arts, Adelphi, Prof. Huxley, the president, in the chair. Mr. Holmes, the treasurer, stated that the Association had been in operation only for a few months, and for a certain portion of that time its action had been suspended by legal difficulties. The number of members enrolled up to the 15th of this month was 126, the total contributions, together with a loan of 100*l*.

from Prof. Jenkin, for the purpose of advertising and starting the Association, was 391*l.* 11*s.*, and the total expenditure 346*l.* 3*s.* 6*d.* Prof. Huxley said that, to put it briefly, the Association was a co-operative store for the supply of good advice, and the modest success which had hitherto attended it was very likely due to the antipathy inherent in human nature to the reception of good advice. Their good advice, however, had this peculiarity—that they did not expect anybody to take it unless he liked. His interest in this Association came from the remote connection he once had with medicine and hygiene. Whatever suspicion of knowledge he ever possessed had led him to the conviction, strengthened by every day's experience of life, that when we aggregated close upon 4,000,000 of people on something less than fifty square miles, if we did not take eare we should be desolated, not like old London by the plague or black death, but by those other forms of disease, as fatal in their way, which have the terrible peculiarity of being easily disseminated by the means we took to get rid of them, unless those means were perfect. Disagreeable as the old cesspool system was, it was attended with very little danger compared with that which waited upon the water sewage system if that system was imperfect, for then it was an admirably-contrived arrangement for distributing disease and death in our own houses and in the houses of people who lived adjacent. There were two ways of meeting the danger. One was by the action of Government in some shape or other; but in England no one would tolerate the intrusion of Government officials for the purpose of knocking about and looking into everything, and besides, the expense and difficulty of working such a system would put it out of the range of the practicable. The other way was to meet the danger by means of those who supplied a good report, such as that Association would do. Therefore it was for the public good that the Association should become a great one, and its work be carried out as widely as possible.

The success which has attended Dr. Vines' English edition of Prof. Prantl's "Elementary Text-Book of Botany" has induced the publishers, Messrs. W. Swan, Sonnenschein and Co., to arrange for a companion volume on zoology, viz. an English adaptation of Prof. Claus's "Handbuch der Zoologie," which Mr. Adam Sedgwick of Trinity College, Cambridge, has undertaken to make. Hitherto this work has appeared without illustrations in Germany; but for the present edition between 500 and 600 drawings have been prepared by Prof. Claus himself. The book is announced to appear next spring. We learn also that Dr. Vines has undertaken for the same firm a "School Botany," covering the ground commonly taken up in the school course. The important treatise on the "Theory and Practice of the Microscope," by Professors Naegeli and Schwendener, which Messrs. Sonnenschein and Co. have had in the press for the past three years, has at length reached completion. It is announced for issue next month.

It is stated that the Report of the Commission appointed in 1879 to inquire into the sanitary condition of the cemeteries in and around Paris negatives, generally, the popular belief in the noxious influences of great burial-places. The composition of the air in the cemeteries, according to M. Schutzenberger, is not distinguishable from that of arable lands.

Under the title of "Prehistoric Devon" (the opening address of the seventieth session of the Plymouth Institution), Mr. R. N. Worth, the president, has brought together in an interesting form many valuable data and references on the subject from all quarters.

In No. 9 of the *Chrysanthemum*, the monthly magazine for Japan and the East, to which we have already referred, published in Yokohama (London: Trübner), there is the the first instalment of a useful vocabulary of Aino words by Mr. W. Dening.

Miss E. A. Ormerod, authoress of the "Manual of Injurious Insects," delivered a lecture on Thursday afternoon to the students of the Royal Agricultural College, Cirencester, on the methods of investigating attacks of insects on crops, and the general treatment to be employed. The lecture was profusely illustrated by enlarged diagrams, and was enthusiastically received by the students and their friends. The lecture will be published in full.

The death is announced, at the age of fifty-three years, of Mr. James Craig Niven, curator of the Hull Botanic Gardens.

The Abbé Moigno's journal, *Les Mondes*, has again, we are glad to notice, passed successfully through a crisis. A fresh start has been made, the old title "Cosmos" becomes more prominent, and a bright-coloured cover has been added. Better paper, more illustrations, and re-arrangement of matter will, we trust, procure the journal increased support.

At the meeting of the Institution of Mechanical Engineers at the Memorial Hall, Albert Square, Manchester, to-morrow, the following papers will be read and discussed:—On Bessemer steel plant, with special reference to the Erimns Works, by Mr. C. J. Copeland of Barrow-in-Furness; on compressed air upon tramways, by Mr. W. D. Scott-Moncrieff of London; on meters for registering small flows of water, by Mr. J. J. Tylor of London.

J. B. Lippincott and Co. have in the press "The Honey-Ants of the Garden of the Gods, and the Occident Ants of the American Plains," by the Rev. Henry C. McCook, D.D.

A violent shock of earthquake, lasting three seconds, occurred at Agram at 10 p.m. on the 23rd inst.

La Nature of October 22 has a long article, with microscopical illustrations, on the drinking water of Paris.

Some interesting facts are brought out in a paper by M. C. Nielsen of Christiania on the impression produced upon animals by the resonance of the vibration of telegraph wires. It is found that the black and green woodpeckers, for example, which hunt for insects in the bark and in the heart of decaying trees, often peck inside the circular hole made transversely through telegraph posts, generally near the top. The phenomenon is attributed to the resonance produced in the post by the vibration of the wire, which the bird mistakes as the result of the operations of worms and insects in the interior of the post. Every one knows the fondness of bears for honey. It has been noticed that in mountainous districts they seem to mistake the vibratory sound of the telegraph wires for the grateful humming of bees, and, rushing to the post, look about for the hive. Not finding it on the post, they scatter the stones at its base which help to support it, and, disappointed in their search, give the post a parting pat with their paw, thus showing their determination at least to kill any bees that might be about it. Indisputable traces of bears about prostrate posts and scattered stones prove that this really happens. With regard to wolves, again, M. Nielsen states that when a vote was asked at the time for the first great telegraph lines, a member of the Storthing said that although his district had no direct interest in the line proposed, he would give his vote in its favour, because he knew the lines would drive the wolves from the districts through which they passed. It is well known that to keep off the ravages of hungry wolves in winter the farmers in Norway set up poles connected together by a line or rope, under which the wolves would not dare to pass. "And it is a fact," M. Nielsen states, "that when, twenty or more years ago, telegraph lines were carried over the mountains and along the valleys, the wolves totally disappeared, and a specimen is now a rarity." Whether the two circumstances are causally connected, M. Nielsen does not venture to say.

We are informed that the lists of papers, &c., appended to Mr. C. R. Markham's "Fifty Years' Work of the Geographical Society," referred to in our leading article of last week, were not compiled by Mr. Rye.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀), from India, presented by Mr. G. R. J. Glennie; a Rhesus Monkey (*Macacus erythraeus* ♀) from India, presented by Miss Richardson; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Mr. J. Pope; a Black-faced Kangaroo (*Macropus melanops* ♂) from Australia, presented by Miss Drax; a Black-headed Gull (*Larus ridibundus*), European, presented by Master Rew Lloyd; two Common Kestrels (*Tinnunculus alaudarius*), British, presented by Masters John and Charles Godfrey; a Snow Bunting (*Plectrophanes nivalis*), North European, presented by Mr. H. A. Macpherson; a —— Monkey (*Macacus*, sp. inc. ♂) from Hainan Island, China, deposited; a Sooty Mangabey (*Cercocebus fuliginosus* ♂) from West Africa, an Ariel Toucan (*Ramphastos ariel*) from Brazil, a Naked-footed Owlet (*Athene noctua*), European, an Ornamental Hawk Eagle (*Spizaëtus ornatus*), a Black Tortoise (*Testudo carbonaria*), an Argentine Tortoise (*Testudo argentina*) from South America, two Radiated Tortoises (*Testudo radiata*) from Madagascar, purchased; a Gaimard's Rat Kangaroo (*Hypsiprymnus gaimardi*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

Comet 1881 *f* (Denning).—From the elements of the orbit of this comet it is evident that it was a much more conspicuous object about the time of perihelion passage in the middle of September, than when it was detected by Mr. Denning on the morning of October 4, and its not having been sooner discovered can only be attributed to the general prevalence of clouded skies in September. Mr. Denning writes us that from September 2 to 29 he could not make a single observation before sunrise, owing to cloudy weather, but that on the mornings of September 29 and October 1 he missed the comet "in some unaccountable manner." The comet having escaped in September, the systematic examination of the sky, which is now pursued by him, is thus explained.

It ought now to be possible to decide by calculation from accurate positions, whether the comet be one of short period or not. The resemblance of the orbit to that of the fourth comet of 1819 has been pointed out. That comet was undoubtedly moving in an elliptical orbit of very limited dimensions: a computation founded upon a new reduction of the observations made at the Observatory of Paris, which alone are precise enough for the purpose, has led Mr. Hind to a period of revolution of 5·155 years, which is somewhat longer than that deduced by Encke in 1820 from the same observations as they were published at the time by Bouvard. At the previous aphelion passage in 1817 the comet would pass in close proximity to the planet Jupiter, and considerable perturbations may have then occurred. In the interval between the perihelion passage of the comet of 1819 and that of Mr. Denning's comet there are twelve periods of 5·151 years, and the comet would again be greatly disturbed by Jupiter near aphelion in 1853, so that it is possible to explain to a great extent the differences between the orbits of 1819 and 1881, but that the period of revolution should not have undergone material alteration at the same time, may perhaps be considered as an argument against the identity of the comets. However, as we have intimated, the question should soon be decided by direct calculation. Less than a fortnight's observations have been shown in more cases than one to be sufficient to give pretty close approximations to the periods of comets moving in small ellipses, as in the case of De Vico's comet of 1844, for which from only eight days' observations M. Faye inferred a revolution of 5·15 years, the correct one being 5·46 years, or that of Brorsen's comet at its first appearance in 1846, when from ten days' observations Mr. Hind assigned a revolution of 5·519 years, the true one being 5·569 years.

The following positions of Mr. Denning's comet are from an ephemeris calculated by Dr. Oppenheim for Berlin midnight:—

		R.A. h. m. s.	Decl. ° ′	Log. distance from Sun.	Log. distance from Earth.
October	28	10 10 0	+14 51·6		
	30	10 13 1	14 52·2	0·0685	0·0411
November	1	10 15 52	14 53·1		
	3	10 18 35	14 54·5	0·0888	0·0503
	5	10 21 7	14 56·4		
	7	10 23 30	14 58·7	0·1084	0·0582
	9	10 25 44	15 1·4		

The intensity of light on November 9 is less than half that on the day of discovery.

Herschel's "Garnet Sidus."—This variable star, the μ Cephei of our Catalogues, appears to require more regular observation than, to judge from published statements, it has of late received, and is an object well deserving the attention of some one of our many amateurs. No doubt satisfactory observations are attended with some difficulty from the high colour of the star, but on that account the results of a single observer may perhaps be deemed more reliable. Mr. Webb, in the new edition of his "Celestial Objects for Common Telescopes," assigns it a period of five or six years, which is assuredly a mistake. It has been included amongst the irregular variables, and its period may be usually about 430 days, instead of several years. Argelander, as an approximation to the period, gives 431·8 days, from observations between 1848 and 1863, but there are very material perturbations. He considered that the period of increase of brightness is greater than that of decrease in the proportion of 4 to 3. The position of μ Cephei for 1882 is in R.A. 21h. 39m. 53·7s., Decl. + 58° 14′ 21″.

This star, which was not observed by Flamsteed, is the first of Ptolemy's ἀμόρφωτοι, under the constellation Cepheus, which he places in 13° 40′ of Pisces with 64° north latitude. If we carry back the position of the variable star from the second Radcliffe catalogue to the reputed epoch of Ptolemy's catalogue—the first year of Antoninus, or A.D. 138—we find its longitude to be in 14° 16′ of Pisces, with north latitude 64° 7′, so that, as was first shown by Argelander (*Astron. Nach. Erganzungsheft*), the identity is beyond doubt.

GEOGRAPHICAL NOTES

The St. Petersburg Correspondent of the *Times* writes as follows:—The question of the existence of volcanoes in Central Asia, especially on the Kuldja frontier, has always been a matter of doubt and discussion among geologists and Russian explorers. The Governor of Semiretchinsk, Gen. Kolpakofsky, had already fitted out expeditions to settle the question—once in 1878, and again in 1879; but owing to the difficulties of reaching the mountains, which the Chinese consider impassable, and also to the disorders which were then taking place in Kashgar, both expeditions were unsuccessful. This year General Kolpakofsky again set himself to the task, and now reports that he has at last discovered the perpetual fires in the Thian Shan range of mountains. He telegraphs that the mountain Bai Shan has been found twelve miles north east of the City of Kuldja, in a basin surrounded by the massive Ailak Mountains, and that the fires which have been burning there from time immemorial are not volcanic, but proceed from burning coal. On the sides of the mountain there are caves emitting smoke and sulphurous gas. The *Official Messenger*, referring to this interesting telegram, observes that the question as to the existence of volcanic formations in Central Asia, which has so long agitated the learned world, is now irrevocably decided in the negative, and bears the testimony of many Russian explorers. Mr. Schuyler also, in his "Turkistan," mentions that these perpetual fires in the mountains referred to by Chinese historians were considered by Severtzoff, who explored the region, as being caused by the ignition of the seams of coal or the carburetted hydrogen gas in the seams. The same author further mentions that Capt. Tosnofskey, another Russian explorer, was told of a place in the neighbourhood from which steam constantly rose, and that near this crevice there had existed from ancient times three pits, where persons afflicted with rheumatism or skin diseases were in the habit of bathing.

Mr. Dorward, of the China Inland Mission, has lately made a lengthened journey in the Chinese province of Hunan, of which he has sent home somewhat full particulars. He was absent from Wuchang, opposite Hankow, on the Yang-tsze-kiang, for five and a half months, and visited almost every part of this

province, so notorious for its turbulent braves, whose hostility to foreigners is proverbial. Mr. Dorward has however established the fact that a European, with two native assistants, can now traverse the province in safety. Near the city of Shênchi, some 450 miles from Wuchang, he had an opportunity of observing the processes used for extracting gold-dust from the sand, which consist in roughly sifting and afterwards using quicksilver.

THE Argentine Government has just despatched two officials to survey five thousand square leagues of country in the neighbourhood of the Neuquem, one of the chief tributaries of the Rio Negro. This extensive tract of country is close to the Andes, and is said to be extremely fertile. When the survey is completed the Government will dispose of the land with a view to its early colonisation.

THE Commercial Geographical Society of Bordeaux in its last *Bulletin* publishes a useful topographical note on the itinerary followed by the Upper Niger Surveying Expedition from Kita to Bamaku.

THE Department of the Interior in Canada has issued a new map of Manitoba and the North-West Territories, showing the country surveyed, &c., and in a later edition the line of the Canadian Pacific Railway will be shown.

WE hear that the Dépôt de la Guerre at Paris has just issued the first sheet of Col. Perrier's map of Tunis, drawn from his recent topographical survey of the country, which has been awaited with much impatience by French geographers.

PÈRE DUPARQUET, the well-known missionary traveller, who returned to South-West Africa early in October, has recently commenced the publication, in *Les Missions Catholiques*, of an account of a journey made by him through Ovampo Land as far as the River Cunene. He travelled in company with Mr. Erchison, one of the principal traders of Omaruru, who also had with him a son of the late Mr. C. J. Anderson. Père Duparquet's memoir is illustrated by a sketch-map of the region, on which is shown a singular connection between the River Cunene and Lake Etosha.

IN the new number (Heft 3, Band 4) of the *Deutsche geographische Blätter*, all the existing information on Wrangel Land and Herald Island has been collected, and will be of interest at present in connection with the missing *Jeannette*. Dr. Albrecht Penck of Munich contributes an interesting article on glaciation with special reference to Eschscholtz Bay in Kotzebue Sound on the north-west coast of America; and Herr G. Kreitner gives a detailed account of the Koko Nor and the surrounding region. There are besides a variety of notes on various points of geographical interest.

To the Austrian *Monatsschrift für den Orient* for October, M. Z. Janiczek of Port Saïd contributes a letter containing a good deal of valuable information on the trade of the Red Sea. In a letter from Herr Hansel of Khartoum we find some interesting information from Dr. Emin Bey. Among other things he tells us that there are three lakes to the north of Victoria Nyanza; that Beatrice Gulf certainly does not belong to Albert Nyanza, but to a lake lying from the south; that steamers now go regularly from Dufilé to Mahagi, a station on the west coast of Lake Albert; and that the only radical cure for the Central African slave-trade is the importation of free Chinese colonists. Prof. Blumentritt contributes notes on some important vegetable products and branches of industry in the Philippine Islands.

HEFT 1, for 1880–1, of the *Mittheilungen* of the Hamburg Geographical Society contains a paper of great interest on the distribution and relative value of cowrie shells by Herr John E. Hertz. These shells are used as money mainly in the region between the Niger and the coast of Africa, though they are also in use in other parts of the world. Herr Hertz gives the exchange value of these shells in the various regions where they are used, and traces their history as a trading medium. A kindred paper, of much practical value and considerable interest, is on the barter-trade of Africa, by A. Wörmann. A long paper, with chart, on the paths of barometric minima in Europe and on the North Atlantic, and their influence on wind and weather in North Germany, by Dr. W. Köppen, is of considerable scientific interest. There is also a lecture by Dr. J. Classen on a visit to Olympia.

ACCORDING to the latest census the population of Japan on January 1, 1880, was 35,925,313. Of these 18,210,500 were males, and 17,714,813 females. When the numerous and destructive civil wars of the last twenty years are remembered, this relative proportion of the sexes will appear striking. Writers of the last century held very exaggerated notions of the population of Japanese towns, but the present census shows that some of them may properly rank among the most populous cities in the world. Tokio and its environs has a population of 957,121; Kioto, the old capital, of 822,098; and Osaka, 582,668. The smallest population of any district is that of the Bonin Islands, recently annexed to Japan, which contain only 156 inhabitants, composed of officials and descendants of Kanakas and deserters from English and American whaling vessels.

CAPT. JOHN MACKAY, of the ss. *Southern Cross* (Auckland), sends us, along with a note, an account by himself in the *Queenslander* of his discovery and settlement of the district of Mackay in Queensland. To the now flourishing town of Mackay we referred some time ago in connection with a special number of the *Mackay Standard*. The town bids fair to become one of the most flourishing in Queensland, though its discoverer does not seem to have met with the recognition he deserves.

SOLAR PHYSICS [1]

II.

AT the conclusion of my last lecture I stated my belief that those changes which are continually going on at the surface of the sun had their origin in currents of convection, and I illustrated the processes which are there going on by what we know to be going on on the surface of our own earth. I referred, but only historically, to a theory which was thrown out many years ago as to the origin of solar heat by Sir William Thomson, according to which it depended on the impact of meteoric bodies. I did not suppose at the time that he still retained that theory, regarding it as the most probable; in fact he gave it up many years ago, and I was glad to find, from conversation with him after the lecture, he is quite of the same opinion as I am, that these disturbances—the enormous disturbances which take place at the surface of the sun, have their origin in currents of convection. I stated my belief that the spots were produced by the downward rush of, comparatively speaking, cool portions of gas which had been in the first instance ejected during these eruptions. In speaking to Mr. Lockyer afterwards I found that he had obtained independent evidence from his spectroscopic researches that these spots consisted of down-rushes of gas, and not, as some have supposed, of up-rushes. He may have mentioned it to me before; if so I must apologise for it having passed from my memory. I will not however say anything about the evidence on which he was led to that conclusion, because he is going to lecture himself, and of course he will be the proper person to explain his own discoveries.

Now with regard to these spots I have hitherto said nothing except as to their existence. The German astronomer Schwabe assiduously observed them in the beginning of 1826, and for about a quarter of a century he went on constantly observing them and making careful drawings of them. As the result of this long-continued and careful work, he was led to the conclusion that these spots as to their frequency and magnitude appear to be subject to a periodical inequality. The period appeared to be about ten years, during which, supposing you start with the maximum of spots, they dwindle away to the minimum, then after some years again rise afresh, and by the end of ten years or thereabouts you get to the maximum. M. Wolf of Berne has discussed the subject, and referred back to older observations, and was led to the conclusion that the period was longer than ten years. He makes it eleven years, or perhaps more exactly nine periods per century.

I will now come to some phenomena observed on the earth with which the solar spots would, at first sight, appear to have no possible connection. You are all, of course, familiar with the magnetism of the earth, by the aid of which our ships are navigated through the ocean. Now it has been long known that the magnetic needle is subject to disturbance; by the magnetic needle I mean the magnet suspended so as to turn freely round a vertical axis. For a long time after the discovery of magnetism that was the only kind of instrument used for the observation, and it had been observed that these disturbances were of two kinds. There was a regular diurnal movement of the needle to the west, and then to the east, of its mean position,

[1] Lecture by Prof. Stokes, Sec.R.S., in the South Kensington Museum Theatre, continued from p. 598.

and besides that there were from time to time irregular, or apparently irregular, disturbances following no observable law. It was known, too, that the diurnal fluctuation, which has now been known considerably more than a century, was greater in summer than in winter. It had been also observed that these apparently irregular fluctuations in the direction of the magnet are observed when an aurora is seen.[1] If there is an aurora there are sure to be these fluctuations, and if there are these fluctuations the probability is, if other circumstances permit, that we shall see an aurora. The connection between the two was made out about the year 1750, so that it is by no means new. Of course we cannot expect that for every magnetic disturbance we shall have a visible aurora; for in the first place the disturbance may take place in the day-time, in which case of course no aurora can be seen; then, supposing even it takes place at night, it may be that at the time the whole sky is covered with clouds, which prevent the aurora being seen; or again, it may be a bright night with the moon shining brightly, not far from the full, and then a faint aurora would not attract much notice. In fact I have often felt in doubt when I saw a luminous streak in the sky on a moonlight night whether it was an auroral streamer or merely a mare's-tail cloud illuminated by the light of the moon. After watching some time one can generally determine which it is, because if it be an auroral streamer it is pretty sure to be unsteady; but if the observer happens to have a small spectroscope in his pocket, or even a prism and a slit, the distinction can be made out at once, on account of the peculiar spectrum of auroral light.

There appears then to be evidently some intimate connection between magnetic disturbance and the aurora. The recent progress of telegraphy has caused us to be familiarly acquainted with another electrical phenomenon, or, if you like, magneto-electrical. (I assume here that the aurora is an electrical phenomenon—that, in fact, has long been admitted—for considerably more than a century.) I allude to the earth-currents. In telegraphy we have occasion to use insulated wire, the ends of which are placed, or may be placed, in connection with the earth. Now when that is done it frequently happens that, without sending any current from the battery at all through the line, there is a more or less powerful current transmitted along the wire, which is made evident by the deflection of the galvanometer. In fact, in certain cases these currents are so strong that they interfere with the working of the lines.

At the failure of the first Atlantic cable in 1865, Sir William Thomson (whom I am happy to see before me) made some experiments with these earth-currents, as they are called, transmitted through this cable. The failure was of such a nature as would have been caused if there was a breakage in the cable something like 300 miles off, and currents were transmitted through the cable, indicating an electromotive force, as it is called, amounting to one or two Daniell's cells; on one occasion to five or six; and currents more powerful even than those are observed from time to time.

Now it is well known that at times of magnetic disturbance we have these earth-currents powerful; and as I mentioned that magnetic disturbances and auroræ come together, we have here a third phenomenon, that of earth-currents, which accompanies the two former. These three are evidently intimately connected with one another, whatever be the cause of that connection. But at present I have said nothing whatever of the relation of these three phenomena to the sun. Of course any one would say there is the remote relation, that it is to the radiation from the sun that all the great changes that take place on the surface of the earth are due: the evaporation of moisture, the heating of the air, and consequent production of winds; and in a remote sense, therefore, there would in all probability be a relation between these three phenomena and the sun. But that relation is very far from remote. I forgot at the proper time to mention one circumstance connected with these earth-currents before I came to the sun, which, if you will allow me, I will do now. I mean the magnetic disturbances.

One of the first fruits of the establishment of regular magnetic observatories was the remarkable discovery that these magnetic storms occurred simultaneously over large tracts of the earth's surface; so that even the sudden and apparently capricious variations of say the direction of the declination needle would be observed simultaneously at the same moment of absolute, not local time, at places far separated from one another, such as London and Paris, and London and Lisbon even.[1] The cause of this magnetic disturbance, whatever it may be, must be one very widely spread. In discussing the results which have been obtained at the colonial magnetic observatories, Sir Edward Sabine made a remarkable discovery, namely, that whether you take the range of ordinary diurnal fluctuations of the magnet, or whether you take the frequency and magnitude of these magnetic disturbances that I spoke of, in both cases there appeared to be a decennial period, or a period nearly decennial, and that corresponded to the period of solar spots, corresponded not merely as to the duration of the period, but also as to the time of the maximum; so that in those years when the sun showed an unusual number of spots of unusual magnitude, both the regular diurnal variation of the magnet was greater than the average, and there were more numerous and more violent magnetic storms; on the other hand, when the sun was comparatively free from spots, the magnetic elements were, comparatively speaking, in a tranquil state. In the older observations the declination was the only one of the magnetic elements which had been observed, but all three components of the magnetic force were observed in these observatories, and accordingly the phenomena could be more searchingly investigated. Further research has fully confirmed this connection, so that there can be no doubt now that there is some intimate connection, whatever be its nature, between solar spots and magnetic disturbances.

I will mention one circumstance which is a remarkable corroboration of this observation. The late Mr. Carrington for many years was engaged in a series of most careful and elaborate measurements of the positions and magnitudes of the solar spots. The way he worked was by throwing a large image of the sun by means of an equatorially-mounted telescope, with its eyepiece suitably focussed, on to a fixed screen. One day he was engaged at this work when he saw two bright spots on the screen. His first impression was that the screen which was used to shut off the light of the sun, which otherwise would have passed down outside of the object-glass of the telescope, outside the tube, had got disarranged somehow or other, and that it was merely the sun shining through the holes, and coming on the screen. He moved the telescope a little, and these spots moved with the image of the sun, proving that it was not merely the sun shining through holes in the shading screen, but that they really belonged to the sun. They remained visible some minutes, during which they moved over a very sensible portion of the sun's disk at such a rate that the actual lineal motion of them must have been—I forget the figures, but I think it was something like 100 or 150 miles per second. Moreover one of them passed over a dark spot, which is confirmatory of the old observation of Wilson, that the spots are at a lower level than the general surface of the sun.

Now it so happened that on examining the records of the magnetic needle, which were kept automatically by a photographic process at Kew, just at the moment when these spots were seen, there was an unusually great magnetic disturbance. Well then, what can be the connection between these apparently so dissimilar, apparently so disconnected phenomena, and what is the cause in the first instance of the three terrestrial phenomena I first mentioned—magnetic disturbances, auroræ, and earth currents?

Different theories have been started as to this connection. Some have supposed that the disturbance of the magnetic needle was an electro-magnetic effect due to the earth currents; others have supposed, on the contrary, that the earth currents were due to the electro-magnetic induction produced by a change in the magnetism of the earth. But what of the auroræ? It has long been recognised that the aurora is an electrical phenomenon. It has been supposed to be imitated—and there can be no reasonable doubt that the supposition is a correct one—by sending an ordinary electric discharge through a highly-exhausted tube. But whence comes the electromotive force requisite to effect that discharge? My colleagues are not in any way responsible for what I am going to advance. I am going to suggest a cause for this phenomenon which, so far as I know, has not hitherto been broached,[2] and of course you must take it for what it is worth. It has not seen the light, and therefore has not had the opportunity of being subjected to the criticism of men of science. If laboratory experiments are to be any guide to us it requires no

[1] At least of the dancing kind; faint, steady auroræ do not seem to be accompanied by sensible magnetic disturbances.

[1] A number of photographic records from various magnetic observatories have recently been compared and discussed by Prof. W. G. Adams.

[2] This refers to the theory as a whole; the individual parts of it had mostly formed limbs, so to speak, of one or other of a set of theories which, taken in their entirety, must be regarded as quite different.

inconsiderable electromotive force to send an electric discharge through even a moderate length of rarefied air, though it passes far more freely through rarefied air than through air at the ordinary pressure. I will endeavour to show you that experimentally.

[An experiment was here exhibited in which the coatings of a Leyden jar were connected with the terminals of a Holtz machine, and also, by two branches, with each other, each branch involving an interruption by air. One branch led through a universal discharger, the brass knobs of which were separated half or three-quarters of an inch, the other through a long tube filled with rarefied air—a so-called aurora tube. The second branch being at first broken, the knobs were adjusted to a distance not too great to allow the spark of the jar to pass without fail. The connection with the terminals of the aurora tube being now restored, the discharge, which was at liberty to pass by either branch, chose the aurora tube.]

It appears then that the resistance to the passage of the electric discharge is greater across about three-quarters of an inch of air at ordinary pressure than across the whole length of the tube, which I suppose is somewhere about five feet, so that, although there is considerable resistance to the passage of the electric discharge through rarefied air, it is very much less than through air at ordinary pressure; but although it is very much less, it is very far indeed from being inconsiderable. Mr. De La Rue has a splendid battery of about 11,000 cells of chloride of silver. It required about 2000 of these to send electric discharges through tubes perhaps two or three-quarters the length of that, but not quite so broad, exhausted to such a degree as to oppose least resistance to the passage. We see then that, if one may judge by laboratory experiments, it requires a very considerable electromotive force to send an electric discharge through even a moderate distance in rarefied air.

Now attempts have been made to measure the height of the aurora, and very large figures have been brought out. It is said to be fifty or sixty, or even eighty miles high; I think some have made it even higher than that. It is a difficult matter of course to measure with much certainty, because you want a base to measure from; and the two stations must be distant a few miles, in order that you may get a sufficient angle of parallax. Then with observers situated a few miles apart it is a difficult matter, with such a variable and indefinite phenomenon as aurora, to fix on what they should observe. Possibly in the future, when such observers may be put in connection by telephone and be able to speak to one another and tell each other what sort of aurora they see, and settle by conversation at that distance what particular part they shall observe, we shall get more certain results. However, there can be no doubt that, although there may be some uncertainty as to the precise height of the aurora, it is very high indeed.

Now, even in spite of this great height, the auroral streamer subtends a very considerable angle at the eye of the observer. If this be a discharge, the length of that discharge must be very considerable, probably many miles. Where shall we get the electromotive force sufficient to send a discharge through so great an interval of air, rarefied though it be, and that not too highly? I say "and that not too highly," because experiments with exhausted tubes have shown that the resistance to the passage of the spark through the tube goes on diminishing as you make the exhaustion higher, until you reach a certain point, after which it goes on increasing again, and this exhaustion, at which the resistance to the passage of the discharge is least, is by no means very considerable as exhaustions go nowadays. Tubes have been so exhausted that rather than strike across a millimeter within the tube from terminal to terminal, the discharge would pass some inches outside in air. Well, then, it would appear from that that we do not gain so very much as regards facility of passage for an electric discharge by going up to a tremendous height in the air. Where then can we get an electromotive force sufficient to send an electric discharge through such a length? Sir William Thomson, in the case of the Atlantic cable which failed the first time, as I said before, obtained earth-currents indicating an electromotive force of a few Daniell cells; but a few Daniell cells, or a few scores of Daniell cells, or a few hundreds of Daniell cells would be quite insufficient for sending a discharge through such a space as I have spoken of, if laboratory experiments are to be any guide. There is, however, one instance of electric phenomena where we have tremendous tensions to deal with—I mean atmospheric electricity. In the case of the atmosphere we may have the electric spark striking all the distance from a cloud to the earth, perhaps half a mile or a mile. I need not say that the electric spark I refer to is a flash of lightning. Here I found some difficulty in getting the discharge to strike through air of ordinary density, across more than about three-quarters of an inch, but in lightning it strikes all that distance that I mentioned. Atmospheric electricity of tension sufficient to strike across a mile of air at ordinary density (or at least slightly reduced as you go up) might have an opportunity of striking across many miles of rarefied air, and if the experiment which I have showed you just now with that tube is to be any guide, then it would be competent to do so. In atmospheric electricity it is conceivable that we may have a sufficient tension to cause the electric discharge to strike across that great distance which the length of an auroral streamer must be.

It has long since been remarked that displays of auroræ seem in some way or other in high latitudes to take the place of thunderstorms in low latitudes. Well, then, I will endeavour to explain what I imagine takes place. I do not enter into any speculation as to the cause of atmospheric electricity. We know as a fact from its manifestation that it exists, and that is sufficient for my purpose. Suppose now that the air, especially the higher portions of the air, over a large tract of country, say to the north of us, were more or less highly electrified—positively or negatively, as the case may be—we will suppose positively—if the electric tension were sufficient, although, considering that the air is a non-conductor, we might not have a flash of lightning, which gathers into itself in one moment the electricity from an entire cloud and sends it down into the earth (we might not have tension enough to produce such a discharge, the resistance to the passage of electricity from one portion of the air to another, which at any rate would be comparatively dry compared with what we have in warm latitudes, would prevent it by itself alone), we might nevertheless have a discharge taking place in the higher regions of the atmosphere where the air is rarefied, and accordingly opposes less resistance to the discharge.

Now let me refer to this figure. This great circle, P E *p e*, I suppose to represent a section of the earth by a plane passing

through its centre. This blue [faint in cut] outside represents the atmosphere, the height, of course, being enormously exaggerated, in order to make it visible at a distance. Suppose in some way or other a portion of this upper atmosphere, as C, got considerably charged positively or negatively, say positively; it would act by induction on the earth below. The opposite electricity, negative in that case, would be accumulated underneath, as at c, and this portion of the earth would form, as it were, a portion of a Leyden jar, the lower atmosphere being the di-electric or glass of the jar, the upper atmosphere being partly, you may say, the di-electric and partly also the charged coating. It would be represented more precisely by an imaginary coating outside composed not of tinfoil, but of some badly-conducting substance. The positive electricity about C would be bound

down in part by the negative electricity about C, which it induces. In another portion of the atmosphere—it may be at some considerable distance from the former—you may have the atmosphere charged in an opposite way, and of course if this were negative there would be induced positive electricity below, or it might be that the whole of the atmosphere from C to D is charged positively, but at D the negative charge is much feebler than at C. The end-result would be the same; but for facility of explanation I will suppose that the upper portion in one place is actually charged with electricity of the opposite kind to what the charge is at the other. If the tension were sufficient, then there might be a striking across of the electricity of this name in the atmosphere from C to D, and in the earth in the reverse direction. Compared with the atmosphere, the earth would be an exceedingly good conductor, so that the electromotive force concerned in sending the currents from one part of the earth to the other would be, comparatively speaking, trifling, and therefore the electromotive force represented perhaps by a few scores of the elements of a Daniell's battery. Well, then, in atmospheric electricity we appear to have the tension requisite to send the discharge through a considerable space of rarefied air. Now if a discharge took place, and if it were night, and the sky were clear, it would, at least where sufficiently concentrated, be visible to us just in the same way as the discharge passing through the exhausted tube is visible, by the light it produces. It would produce in fact an aurora. The air is not a comparatively good conductor, like a thunder-cloud, from which a great quantity of electricity strikes in one moment, but is, after all, a bad conductor, so that the electricity can only pass in a spitting sort of way. We may conceive here that we have a sort of double current, yet not forming a complete circuit; nevertheless a discharge would go on nearly of the same nature as if the circuit were complete, and the effect of such a discharge on the magnetic needle would be nearly the same as that of a circuit which was complete. I will endeavour to produce a discharge in a circuit which is doubly incomplete.

[An experiment was here shown in which two Leyden jars were charged, one positively, and the other negatively, and were laid on the same wire resting on a table. On connecting the knobs the jars were discharged, and that almost completely, as it happened that the charges were almost exactly equal. The inner coatings here represent two portions of the upper atmosphere, the outer coatings the opposed portions of the earth's surface, and the glass of the two jars the intervening portions of the lower atmosphere.]

As I said, although the circuit is not complete, the electro-magnetic effect of the whole system would be nearly the same as if you had a complete circuit with an electric current passing through it. Now if there be only a sufficient quantity of electricity, we have here the elements necessary for producing a disturbance of the magnetic needle. Moreover, those disturbances, as the instruments show, are of a most fitful and apparently capricious character. They resemble in that the fitful character of electric discharges through air. I need hardly say that according to this theory the earth-current consists in the return currents produced by the statically-induced change on the surface of the earth, induced by the charged atmosphere above. When there is a neutralisation of the electricity from one part to another of the atmosphere above, the induced electricity in the earth is set free, and we have earth-currents to bring about the redistribution of the electricity on the surface of the earth.

It seems to me that this theory not only accounts for the connection between the phenomena, which could be otherwise accounted for, but enables us to conceive how it is that electricity strikes across such enormous distances in the upper regions of the air, and I think, further, it will account for some interesting features of the electric discharge which constitutes no doubt itself the aurora. I have here a sheet of blotting- or filtering-paper, and I will suppose this to represent an electrified tract of air lying over, it may be, an extensive tract of country, say somewhere to the north of us. Suppose that this air is charged positively, it will induce negative electricity on the earth below. This metallic coating on this sheet of glass [over which the blotting-paper was held horizontally at a little distance] may be supposed to represent the surface of the earth on which this negative electricity is induced. The two may be quietly in equilibrium. Suppose, however, that from some cause or other the tension becomes sufficient to enable the electricity from some point in this stratum of air to strike across higher up—because the streamers are found to be parallel to the direction of the dipping-needle, and parallel accordingly to the lines of magnetic force—higher up in the first instance; from thence I do not know where the discharges go, but I should suppose that in our country they generally go somewhere to the south of us. Now if a tract of air were pretty uniformly electrified, it would induce electricity of the opposite name underneath it, pretty uniformly distributed except about the edges, where the electrified air which was the inducing body would tend rather to overlap the electrified portion of the earth below, and where accordingly, if the charge were the same throughout, there would be the greatest tendency for the electricity to strike off and pass into the upper regions of the atmosphere, and thence probably to the south. Well, suppose now that a discharge begins anywhere, say somewhere along this edge of the paper, which I will suppose to be the northern edge. The paper which I hold in my hand is really touch-paper (such as boys use for amusement), and I will light the edge of it. Now this smouldering away of the touch-paper I conceive to represent the mode in which the rarefied air becomes successively discharged. Suppose that a discharge takes place somewhere about the edge of this sheet of electrified air covering a large tract of country, then if once a hole (so to speak) were formed, the tendency would be for the discharge to continue along that edge, because, as I said, as soon as the electricity at the edge was discharged the electricity of opposite name which had been induced on the surface of the earth below would be set free, the earth-current would be set up; and then again, what now is the edge of the electrified tract of air would be left exposed, no longer protected in the same manner as before by the induction of the electricity of opposite name beneath; the electricity would fly off from it in turn, and so on, so that there would be formed a sort of curtain composed of auroral rays, and gradually advancing, in our country usually in the direction from north to south; because we live in a sort of neutral region not too far south to see the aurora from time to time, and not far enough north to be exempt from thunderstorms. This auroral discharge, which takes the place of thunder-storms in lower latitudes in some way or other, usually occurs to the north of us, and accordingly the aurora is called the Northern Lights; but when there is a fine display it sometimes reaches down to us and goes south of us. So I say the discharge would usually begin from a place north of us, and would creep along the edge of the electrified stratum of air, forming a sort of luminous curtain, and passing from north to south, just as the smouldering edge of the touch-paper passes along the paper gradually. When we are just under the edge at which the discharge takes place we have, as I conceive, an auroral arch passing, it may be, through the zenith, generally stretching also east and west, and generally moving with a slow motion from north to south.

Now supposing that that is the explanation of the three phenomena—magnetic disturbances, earth-currents, and auroræ—can we in any way connect their occurrence with changes going on at the surface of the sun? I think we can. We know that a tube containing rarefied air, supposing the density of the air in it is given, opposes less resistance to the electric discharge through it when it is warm than when it is cold. The conducting power of a wire for electricity decreases if you heat the wire, but it is the reverse with air. The passage of electricity through rarefied gases is very different in its nature from the passage of electricity through a wire or through an electrolyte. Mr. De La Rue has shown in the course the researches made by means of his splendid battery, that in these highly exhausted tubes the electric discharge, be it ever so steady to all appearance, obeys laws connecting it rather with a series of disruptive discharges, with a rapid succession of sparks, than with a discharge passing through a wire. Now connected with that difference, or at least accompanying it, there is that opposite action of heat which, as I say, in the case of gases renders the passage of electricity more easy instead of less easy. We may imagine that if from any cause the sun gives out greater radiation than usual, the upper regions of the atmosphere may thereby become heated to a certain extent[1] and oppose less resistance to the passage of the electric

[1] The rays of the visible spectrum, and even the invisible rays for some considerable distance beyond the extreme violet, pass freely through clear air, which could not therefore be sensibly heated by them. But there is reason to think that the atmosphere, or some of its constituents, are more or less opaque to rays of very high refrangibility; and it is just for copious emission of these that sources of radiation of an excessively high temperature are so remarkable. The substance, which is opaque to the rays of excessive refrangibility, and consequently enables them to heat upper regions of the atmosphere, is probably not nitrogen or oxygen, but some gas or gases present in very small quantity.

discharge through it than they did before. In this way we may conceive that in a great outbreak like that observed by Mr. Carrington, where the hot interior of the sun is turned up, as it were, and radiates towards the earth, the facility for the passage of the electric discharge is increased, and it may be very rapidly increased. So that according to this theory the foundation of these three phenomena lies in atmospheric electricity, which forms as it were the magazine, and the solar radiation, as it were, supplies the match, and allows it to be discharged. Of course, over and above that, when solar radiation is active, all the phenomena which depend on solar radiation may be expected to be active too; and therefore beyond its influence in firing the match, to speak metaphorically, this solar radiation, when more active than usual, and lasting, will also produce a more rapid development of all those processes at the surface of the earth which depend on solar radiation, among others, no doubt, the generation of atmospheric electricity, although we are not at present able to explain with certainty the manner in which it is produced. So that in two ways, by applying the match to the train already laid and by gradually manufacturing the powder, the increased solar radiation may cause an increase in those electric discharges and earth-currents as the result of the redistribution of the induced electricity at the surface of the earth, and thereby a disturbance of the magnetic elements. I do not know of any other theory than that of atmospheric electricity which furnishes anything like sufficient electromotive force to account for these auroral discharges, if they are really electric discharges analogous to those which take place in exhausted tubes. As I said, it has been supposed by some that the magnetic disturbances are due to earth-currents; according to the theory which I have advanced, they are due rather to a vast assemblage of currents, partly atmospheric and partly terrestrial. An objection has sometimes been taken to the supposition that the magnetic disturbances are due to earth-currents arising from the consideration of the electro-magnetic effect which the earth-current actually observed would have upon the needle. But this, I think, is obviated when you remember that an earth-current actually observed is merely what results from the examination of a very small portion of this vast electric system, stretching it may be over hundreds of miles of country. At Greenwich, for instance, there are now wires by which earth-currents are regularly observed. The coincidence between photographic traces left by the earth-currents and those left by the magnetic storms is most remarkable. Every peak of the one, you may say, answers to a peak of the other. It has been noticed, however, that there appears to be a slight difference in the time of the occurrence. It would appear as if the disturbances preceded the earth-currents. Well, that may very well be, because, according to the theory which I have advanced, the effect on the magnet is the resultant effect of a vast series of currents, partly terrestrial and partly atmospheric, stretching over a very large region of country, whereas the earth currents observed are merely obtained by tapping the earth at a couple of places at no great distance, so that the two do not by any means necessarily correspond exactly.

I forgot to mention at the proper time a diagram which Capt. Abney has kindly prepared for me. This is a copy of the diagram made by Mr. Ellis of the Royal Observatory, giving the result of his discussion of the Greenwich observations on two out of the three magnetic elements, namely, the declination and horizontal force, as compared with sun-spot frequency. [Mr. Ellis's diagram in Part ii. of the *Philosophical Transactions* for 1880 was then referred to.]

You see that an examination of the phenomena going on at the solar surface itself leads us to the conclusion that there are vast currents up and down, by means of which the comparatively speaking cool upper portions are continually replaced by hotter matter from beneath. Mr. Lockyer, in the lectures he is about to give, I have no doubt will have a great deal of very interesting evidence derived from spectroscopic study of the phenomena to lay before you, bearing out that same conclusion. We have seen that the supposition that there is extra radiation when the interior portions of the sun are ejected and come to the surface, falls in very well with the known relationship between the occurrence of sun-spots and the three terrestrial phenomena I have mentioned—magnetic disturbances, auroræ, and earth-currents. I say between the sun-spots, although it is not, strictly speaking, the sun-spots themselves, but the tremendous disturbances which are their precursors, and of which they form the most easily observed manifestation.

Now if there is reason to believe that, when the sun is in a state of activity in this manner, there is increased radiation from it, it may well be that the meteorology of the earth is affected by the changes which take place at the surface of the sun; but the meteorology of the earth forms an exceedingly complicated problem. We have, so to speak, to deal here with a very complicated integral of a differential equation. I am speaking somewhat metaphorically, but my words will be understood by the mathematicians who happen to be present. We cannot very directly connect that integral with the disturbing forces. One thing, however, we may say: supposing that there is a system of any kind subject to periodic disturbing forces—and we have seen reason to believe that these great eruptions which take place on the surface of the sun are, perhaps somewhat roughly, periodic—if, I say, we have a periodic system of disturbing forces, then the system which is acted upon by these forces will show a periodic disturbance which may be more or less concealed by apparently capricious disturbances, but which yet may be expected to come out in the long run; and it has been supposed by those who have studied meteorological phenomena that there are indications of a decennial, or nearly decennial period in some of the meteorological elements, for instance, the mean temperature of the air and the fall of rain. Again, in some observatories thermometers have been sunk to a considerable depth in the earth, and observations of such thermometers were carried out for a great number of years by the Astronomer Royal of Scotland, Prof. Piazzi Smyth, and they are regularly carried out now at Greenwich. Connected with the annual variation of temperature between summer and winter there are, so to speak, waves of heat and cold slowly propagated down from the surface of the earth to the interior, rapidly decreasing in amplitude as they descend, and by going a suitably moderate depth you get these fluctuations, indicating the annual fluctuation of the atmospheric temperature, and free in a great measure from the fluctuations which take place at much shorter periods. When you go a little way down the results given by these thermometers seem to indicate something of a decennial or nearly decennial period. Conflicting statements, however, have been made by different observers as to the time of maximum of the meteorological elements which were supposed to have such a period, and some have argued from the results that when the sun was in a highly spotted condition we had a higher temperature than usual, and some the reverse. Now this is an important matter to attend to. Suppose we had such a system acted on by periodic disturbing forces; it will show at least in the mean a corresponding periodic fluctuation, corresponding however only as regards the length of the period. The epoch of maximum of the element observed, whatever it may be, has no necessary relation to the epoch of maximum of the disturbing forces, excepting that they are separated by a constant interval, and the epoch of maximum of the element observed may be different at one locality from what it is at another. So that it is only the period and not the epoch of maximum which you can expect to arrive at possibly by an observation of such elements as I have spoken of. It is very difficult indeed to say, even if a ten yearly period be observed, what ought to be the year of greatest solar radiation if we have given the observed results.

Is there any way in which we may hope to attack that problem? I think there is. It is by no means hopeless to attempt to measure by a direct process the solar radiation. Instruments have been devised for the purpose, called actinometers. One was devised by the late Sir John Herschel, and goes by his name, and it is a very beautiful instrument; but unfortunately it is excessively fragile, and if the instrument has got rough travel or rough work at all to go through, it is pretty sure to be broken. Other instruments have been devised for the purpose, and among them I may mention one by Prof. Balfour Stewart. He has lately devised a new actinometer, and one of his construction has recently been sent out to India, and is at present under trial. In these cases heat is observed by a thermometer. Another has been devised by Prof. Roscoe, depending on the chemical action of radiation. I think none of these have yet had a thoroughly complete trial, because in such a climate as ours a fair trial can hardly be made, since there are so many disturbing elements in the lower atmosphere. An exceedingly slight cirrus-haze makes an enormous difference in the amount of heat radiated from the sun as received by us without being deflected from its course. If there be the slightest haze a good deal of the heat rays are deflected from their course, perhaps not much deflected, so that if we take in the direction of the sun itself and the neighbouring

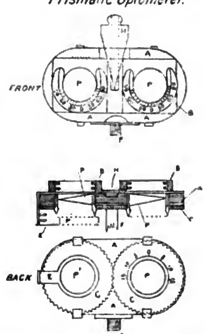
directions from the sun for some considerable distance all round it, the totality of radiation from that portion of the heavens may not be so much inferior to what it is when there is none of that slight cirrus haze there. But still we have haze enough in the lower regions, and besides that we have water in an invisible state of vapour, and Dr. Tyndall has shown that that absorbs with great avidity a portion of the heat rays. Those, however, are mainly rays of very low refrangibility. Still the absorption of these may very sensibly affect the totality of radiation received from the sun. How then shall we, if possible, get rid of these sources of disturbance? The best plan seems to be to take observations at a considerable altitude, where if possible you may get many thousand feet above the level of the sea and get rid of the lower, dustier, hazier portion of the atmosphere, and get rid also of by far the greater portion of the aqueous vapour, which by itself would absorb a portion of the heat. That is what the Committee on Solar Physics have attempted to do. We contemplate having actinometric observations made in the north of India. Of course, if observations are to be continued, it is not sufficient to go to some high mountain. You must go to some habitable place where the observer can live and be in some sort of comfort. Now in the Himalayas you may get up to many thousand feet and yet be still within reach of human habitations; or what is better still, if you cross the range and go over into Thibet, you have there a high table land many thousand feet above the level of the sea, with a sky usually cloudless, and where observations of this kind may, it is hoped, be made with success for a considerable period together, and the result may, we hope, in time throw light on the question whether or no there is in reality a change in the amount of radiation received from the sun, and whether the amount of that change is sufficient to make any material difference in the meteorological conditions of our globe.

I have spoken of meteorological elements in which various observers suspected that they saw some indications at least of a decennial, or nearly decennial, period. Speculations have been made as to whether there is not a decennial period, or something of the kind, traceable even in the occurrence of Indian famines. If so, there may be some very close relationship between the solar spots and these famines. At first sight one would be disposed to say, "What possible connection can sun-spots and famines have with one another? You might as well speak of the connection between comets and wars!" But when we go deeper below the surface, and study carefully the phenomena presented to our view, we see that a possible connection between such apparently remote things as sun-spots and famines may not be chimerical; and there is no saying what practical application may in the end result from a study of solar phenomena undertaken in the first instance for a purely scientific object.

A PRISMATIC OPTOMETER[1]

IT is well known that in the normal eye, with its accommodation relaxed, parallel rays of light, that is, those from distant objects, are brought to a focus on the retina. Rays from near objects are divergent, and if they enter such an eye they are not brought to a focus on the retina, but would be at some point behind it. In order that they may be so brought to a focus and form a distinct image on the retina, an effort of accommodation is necessary. This is performed by a small muscle called the ciliary muscle, inside the eyeball, the ultimate effect of whose contraction is an alteration in the shape and perhaps the condition of the lens, which causes the rays to be more strongly refracted, and brings them to a focus on the retina. The effect is in fact the same as if a convex lens were added to the optical system of the eye. As age advances, the muscle and lens become stiffer, and work with difficulty. They are relieved of part of their work by putting a convex glass in front of the eye. Hypermetropia is a condition in which the axis of the eyeball is too short, compared with the refracting power of the lens. In it an effort of accommodation is necessary to see even distant objects clearly, and a still stronger effort to see near objects. A person suffering from it requires convex glasses. When both eyes are used together, the optic axes of both are directed to the object, so that in looking at a distant object they are directed parallel, and in looking at a near one they converge. These movements are effected by the external muscles of the eyeball, which are supplied by branches of the same nerve as the ciliary muscle. As a fact these movements of the ciliary muscle and of the external muscles of the eyeball are associated, or habitually performed in conjunction; that is, the brain has become accustomed to send an impulse to the one set of muscles proportionate to that sent by the other. Any disturbance of this association can only be accomplished by a distinct effort which, if severe or long continued, is apt to be painful. Suppose a man has become presbyopic, *i.e.* his accommodation has gradually become stiff, and its range reduced. In order to accommodate for rays from an object at the ordinary reading distance of ten or twelve inches, he has now to exert an effort equal perhaps to what he would have employed when young on one four inches off, but the change has been gradual, and the convergence of the eyes for twelve inches has become associated with this amount of effort. If he now use convex glasses of suitable power, the want of refracting power is supplied, the effort of accommodation is reduced to its natural amount, but the amount of convergence which has become associated with this small effort is now insufficient, and the eyes, instead of converging to twelve inches, converge on a point several feet distant, so that double vision would be produced, unless by a distinct effort the eyes were converged more, and

Eye End of Dr. T. Anderson's Prismatic Optometer.

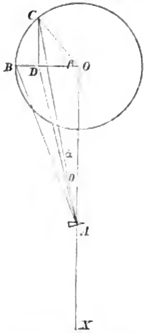


A, main frame carried by F, graduated rod; C, rotating frame carrying P P, prisms; E, frame carrying P′, third prism; H, wedge to separate B B frames for lenses.

this effort is often painful, and is expressed by the term that the spectacles "draw" the eyes. After a time new associations are formed, and the spectacles can be used comfortably; but this does not happen in all cases, and for these it is necessary to grind the lenses on glasses of prismatic section. The action of the prism is so to bend the pencils of rays coming to the eyes that they appear to diverge from a point corresponding to the new focal distance of the eyes provided with the spectacles. Sometimes the amount of prismatic effect required is calculated, but the calculation, being based on general considerations, does not always suit individual persons; at other times prismatic glasses from a trial case, are combined with the calculated spherical, or spherical and cylindrical glasses, until one is found with which vision is comfortable. In many cases it is not necessary to use glasses specially ground on prisms, but sufficient to move the centre of the glasses nearer together. The glass being thicker in the centre, looking through the part near the edge produces an amount of prismatic effect which is often sufficient. If concave glasses are used, as in cases of short sight, then they must be further apart than the distance of the eyes, in in order to produce this effect. The object of the instrument exhibited is to find experimentally the amount of prismatic power, and the distance of the centre of the lenses which is

[1] "On a Prismatic Optometer," by Tempest Anderson, M.D., B.Sc., read at the York meeting of the British Association.

required in any individual case. Two circular frames each $2\frac{1}{2}$ inches in diameter, and with teeth cut in their edges, are mounted, so that the teeth gear into each other, and they can rotate freely, but in opposite directions. In the centre of each frame is mounted a prism of 18°; one of the frames is graduated, and when the graduation is at 0° the axes of the prisms are parallel, so that parallel pencils of rays falling on both are deviated both in the same direction, and still parallel. Thus when the pair of prisms are arranged horizontally in front of a pair of eyes, an object looked at appears displaced up or down, but there is no lateral deviation on either. If the frames be rotated 90° in one direction, the prisms both have their bases inwards, or, if in the other direction, both outwards, so that two pencils of rays are deviated to the full power of the prisms. In the intermediate positions part of the prismatic effect is resolved in a direction at right angles to the line joining the centres of the frames, and can be neglected as only producing parallel displacement of the image, and part is resolved in the direction of this line so as to produce apparent separation or approximation of the images. This amount is read off from the graduation, which is constructed on the following principle:—Suppose a ray of light X A O perpendicular to the plane of the paper meets the paper at O. Suppose a prism be introduced at A



having an angle of deflection θ, the ray of light now falls on the paper at B. If the prism be rotated through angle β, the ray now falls on the paper at C. Join O B, O C, and resolve O C into vertical and horizontal co-ordinates C D, O D. C D being neglected as described, we wish to find O D the horizontal component of the deflection.

$$\text{Since } OB = OC$$
$$\therefore \frac{OD}{OB} = \frac{OD}{OC}$$
$$\therefore \frac{\frac{OD}{OA}}{\frac{OB}{OA}} = \frac{OD}{OC}$$
$$\frac{\tan a}{\tan \theta} = \cos \beta$$
$$\text{Log} \cos \beta = \text{Log} \tan a - \log \tan \theta.$$

Two other frames are placed in front of the prisms. They contain grooves to hold lenses or combinations of lenses, and are graduated so that cylindrical lenses can be set at any desired angle. The frames can be separated or brought nearer with greater accuracy by a wedge, and the distance of the centres of the glasses is marked on the bearing. The whole is carried at the end of a graduated bar which carries a sliding support for an object. This bar is graduated in inches for use in calculation and also in focal lengths of a set of dioptric lenses. A third prism is attached, so that it can be placed between one of the frames and the object. When it is in position, the rays going through it to the eye appear to come from an object higher than when it is absent. Double vision is produced, and the eyes left free to find their most comfortable position free from any effort to make the two images coalesce. To use the instrument, the spherical and cylindrical elements of the spectacle required are first found either by some of the ordinary methods or by the ophthalmometer described in the Annual Volume for 1880, and the required lenses from the trial case put in the appropriate frames. The third prism is interposed, and an object, such as a vertical line, looked at at reading distance. If the images seen by the two eyes are exactly one above the other, the prismatic adjustment is presumably correct, the third prism is removed, and trial is made whether reading can be carried on for some time without fatigue. If the images are slightly displaced externally, trial is made whether shifting the centres of the lenses nearer or further off suffices to bring them into position. If so, the distance is noted and sent as a direction to the optician. If the displacement be more than can be corrected by this means, the prisms are rotated till the desired effect is produced, and the amount of prismatic deviation to be given to the proposed spectacles read off. The third prism is removed, and reading practised as above.

SCIENCE AND INDUSTRY[1]

CONSIDERING the high position in literature and science of my predecessors in this chair, I feel that I have been bold indeed in accepting the distinguished office of President of the Midland Institute during the current year. I shall not attempt to rival my predecessors in those literary or philosophic flights which befitted their powers, but shall confine myself to certain suggestive remarks flowing from personal experience of men and matter, which may prove of some interest to an audience consisting in the main of persons who, like myself, are intent upon combining science with practical aims, but who, unlike myself, have the best part of their career still before them.

In venturing to express my views regarding the great question of the day, that of Technical Education, I shall run considerable risk of disappointing some of its most ardent advocates, who may have looked upon me, a foreigner by birth, as a staunch supporter, if not as the living embodiment, of that particular form of education that the Polytechnicum of Germany and other Continental countries imparts to the aspiring engineer and manufacturer, but which, in my opinion, leaves much to be desired, and is certainly inapplicable to the condition of things which we find in this country.

The subject of education, and of science education in particular, is one the practical and national importance of which it would be difficult to over-estimate. It is well known that the Continental nations have in some respects stolen a march upon us in providing for the education of the young engineer, the architect, the manufacturer, and the craftsman. Colleges of high and low degree abound where both science and practical processes are taught, whereas the teaching of the latter has been looked on hitherto amongst us as professional or trade knowledge to be acquired during lengthy periods of pupillage or apprenticeship.

The more ardent advocates of the Continental method of technical education go so far as to think that the irksome system of apprenticeship should give way entirely to technical teaching within the college walls, whereby it is assumed much time could be saved and a better knowledge be imparted to the aspiring engineer or manufacturer. Having had some experience of young men brought up at these technical schools, I am bound to say that I have not been favourably impressed with the results produced by that system. The practical knowledge acquired at those establishments is wanting in what may be called the commercial element, that is of due regard to cost of production, of which the teacher himself must be comparatively ignorant, as otherwise we should find him employed at the factory or engineering office, instead of in the schoolroom.

The young polytechnic student is apt to look on the machine or process which he has studied, not as one of many solutions of a practical problem influenced by ever-varying external circumstances, but as something representing an absolute condition of things almost as completely proved and established as a first

[1] Abstract of Address at the opening of the Birmingham Midland Institute, by Dr. C. W. Siemens, F.R.S., president of the Institute.

principle in nature, or a proposition of Euclid; he is very proud of this positive knowledge, and impatient of any suggestion aiming at the accomplishment of the same object by means not sanctioned by his authoritative text-book. He is apt to be a dogmatist, a splendid man for coming out first-class in a competitive examination, and likely enough to make a good official in a Government administration, but most unlikely to venture of himself on such new embodiments of first principles of nature as are essential to the accomplishment of improved results, and as have animated our Watts, our Cromptons, our Corts, and our Bessemers in enriching the world with new processes.

On the Continent, where the Governments themselves are largely engaged in trade and enterprise, where railways, mines, and factories are State establishments, it was necessary to create a large staff of men educated to the point of being able to assume at once a position of some authority in the ranks of rigid organisation, and such men are provided by the polytechnic schools. Our Indian Government being similarly situated, had to resort to similar means, and to establish Cooper's Hill Engineering College.

In this country, where happily the great commercial interests, with one exception, are still in private hands, educational establishments on the Continental model would be, I consider, inappropriate. The object a young man has in view is not the attainment of a snug position in a Government establishment, but to be fitted by his education for the great battle of life, in which he will be judged, not by the answers he can give to certain set questions in his competitive examination, but rather by the faculty he may have acquired of realising useful results under even adverse circumstances and conditions.

The time was, not long ago, when the opinion prevailed in this country that useful knowledge could only be attained in the workshop; that a lad, after having mastered the three R's at a primary school, had to be bound to a manufacturer or craftsman for a period of seven years, where his time was occupied in routine work or in mechanical repetitions of one and the same operation, causing him to give up thinking altogether, and to become what was dignified by the appellation of practical man—a man of notions, with a supreme contempt of theory or science. The reign of this practical man *par excellence* is happily drawing to a close; for those who wish to treasure up his memory, I would recommend a lucid description of him by my friend Sir Frederick Bramwell in his presidential address to the Mechanical Section of the British Association in 1872 (which may be found in the *Transactions* of that year). Since then Sir Frederick Bramwell has done much to hasten the burial of the character he describes, in making himself the principal promoter of that splendid endowment, the London City Guilds Institute, which, under wise direction, cannot fail to exercise a very important influence on the educational development of the country.

Having now spoken, somewhat disparagingly, I fear, of both the old English system and of the more recent Continental system of technical education, I shall be asked, no doubt, what in my opinion should be the plan adopted in preparing the mechanical engineer, the manufacturer, and the artisan of the future for their respective careers. The answer to such a question is one involved in much difficulty, scarcely admitting of universal solution. There are, however, certain principles of general application which, I submit, should never be lost sight of. Moral education being provided for, the main object in teaching the young should be to strengthen the power of memory, and after that the reasoning faculty. The first is most appropriately accomplished by the conventional three R's, and by the teaching of geography, history, and languages, both ancient and modern; and the second by mathematics, logic, and the natural sciences. Sir John Lubbock, in addressing you some years ago from this chair, forcibly called attention to the necessity of combining both literary and scientific education in our grammar schools, suggesting that at least ten hours a week should be given up to the teaching of science.

Such a system of education has since been established at Eton, where (as reported in NATURE, vol. xxiv. p. 287) all pupils attend science classes, and are said to be very fond of what they are pleased to call the "stinks" (in allusion to the chemical laboratory); whereas at other grammar schools a "modern department" has been added to the establishment, where science is taught to those only who elect not to go in for a classical career, whilst the classical scholars remain untaught in science as before. I am of opinion that the Eton system is the better of the two, for I cannot regard an education to be complete that does not combine literary with scientific training; the one gives the polish and the other the fibre and practical direction to the understanding. A Birmingham manufacturer by no means despises polish to make his goods tempting in the market, but he would hardly like to offer them composed entirely of lacquer and polish without that solid fibre in the interior that is necessary to fit them for practical usage; such internal fibre may in our case be likened to the knowledge of useful information such as modern languages and natural science, without which the classical polish must be devoid of the power to produce results, which after all is the standard to be aimed at.

The man of classics, the Bishop, the Legislator, and the Judge of the future, educated at Eton, will be none the worse for standing upon an educational foundation comprising "stinks" in its composition, whereas the man of practical pursuits will be all the better for his early literary culture.

But it may be urged that the time available for study is too short to admit of both, and that one or other must therefore be chosen. I should venture to doubt the sufficiency of this objection, being of opinion that the study of the one kind of knowledge qualifies the mind the better for the other, in the same way as in after life recreative exercise of mind and body is resorted to in order to relieve the drudgery of daily duty.

The usefulness of science teaching depends of course to a great extent upon the teacher, and upon the system adopted. Science taught as it were by rote is of comparatively little value in after life; to be beneficial it should be practical, impressing the mind vividly with the simplicity and the beauty of the laws of nature, and for this purpose each statement of a law should be followed up by ocular demonstration, nay by active co-operation on the part of the student in the experiment. For this purpose no school ought to be without its chemical, its physical, and its mechanical laboratories, where students could test for themselves chemical reactions, verify physical laws, and ascertain the mechanical properties of materials used in construction. Nor do these laboratories necessarily involve a large expenditure for apparatus, the most instructive apparatus being that which is built up in the simplest possible manner by means of pulleys, cords, wires, and glass tubes, and, if possible, by calling into requisition the constructive ingenuity of the student himself.

Only after the student has attained a thorough knowledge of first principles will it be desirable to introduce him to elaborate instruments such as telescopes, polariscopes, electrometers, and delicate weighing-machines wherewith to attain numerical results and to commence original research. For this reason very complete laboratories are of great importance at the universities and superior colleges, where exact science and independent research take the place of mere tuition of first principles.

After first principles have been taught at school, the university on the one hand, and the workshop, aided by study on the other hand, are requisite to impart that special knowledge necessary for the profession or business to be followed in after-life. In this respect the German University—that glorious institution for the development of independent thought—offers advantages much more commendable for imitation than the technical school, and it is a significant fact that while the thirty universities of Germany continue to increase both as regards number of students and high state of efficiency, the purely technical colleges, almost without exception, have during the last ten years been steadily receding; whereas the provincial "Gewerbe Schule" has, under the progressive Minister, Von Falk, been modified so as to approximate its curriculum to that of the "Gymnasium" or grammar-school.

In some technical schools mechanical workshops are provided, in which students may work at the lathe, the vice, and the planing-machine, and where they are allowed to construct small steam-engines or other pieces of machinery. I doubt very much whether these toy steam-engines are such as would satisfy a mechanical engineer in real practice, and think that both the money of the institution and the time of the student could be much better employed if, instead of imitating practical engineering, he were made to experiment with testing-machines in order to obtain a thorough insight into the mechanical nature of materials, their absolute strength, their elastic limits, and the effects produced upon them by the processes of annealing, tempering, and welding. University College, London, has taken a lead in this respect under the able direction of Prof. Kennedy, and its example will, I hope, be followed by other colleges.

As regards middle class education, it must be borne in mind

that, at the age of sixteen, the lad is expected to enter upon practical life, and it has been held that under these circumstances at any rate it is best to confine the teaching to as many subjects only as can be followed up to a point of efficiency and have reference to future application. It is thus that the distinction between the German Gymnasium or Grammar School and the Real Schule or Technical School has arisen, a distinction which, though sanctioned to some extent in this country also by the institution of the "modern side," I should much like to see abolished.

But I shall be told that it is impossible to teach everything properly within the time, and shall be reminded of the proverb that says, "A little knowledge is a dangerous thing." I, for one, do not believe in this proverb, which I consider erroneous, and mischievous in its application. Referring to myself as an example, I am sorry to state that I had not the advantage of being taught Greek at school beyond the mere letters of the alphabet—my early education having indeed been irregular and cut short much too soon—which surely is the minimum of knowledge that could possibly be possessed of that language. Yet even this amount of knowledge of Greek has stood me in good stead, because it has enabled me at any rate to use those letters in mathematical formulæ, and on a push to puzzle out some of those Greek names which are given to scientific instruments. In this case, at least, exceedingly little knowledge has proved no danger, but a considerable advantage to me, and it would not be difficult to multiply examples to the same effect. A little knowledge of a modern language will be best appreciated by an English person who, speaking no language but his own, has occasion to go abroad. Arriving at his destination he finds that he is unable to make the railway porter understand what conveyance he intends to take, and where he intends to go; his perplexity will be still greater when, on entering a restaurant, say at Paris, he is presented with a bill of fare extending over several pages, from which to select his dinner. In despair he points at random to some of the enumeration of dishes, and finds to his discomfiture that the one is presented to him in the form of a *pâté* of snails, another as a preparation of legs of frogs, and the third as water ice with which to appease an appetite quite equal to roast beef, potatoes, and cheese.

In physical science a little knowledge may be a matter of the greatest importance to an artisan when he is called upon to set a machine to work, and is stopped by some such accidental cause as the accumulation of air below a valve, or unequal expansion due to a local source of heat. The knowledge of a few fundamental laws of physical science will at once enable him to divine the cause of difficulty, which has only to be recognised in order to be removed. I should therefore be disposed to reverse the proverb, and to say that "a little knowledge is an excellent thing," only it must be understood that this little is fundamental knowledge; that it is not the knowledge of the conceited pretender who has committed to memory a few scraps of information of a particular subject; who quotes a Greek author without having learned as much of the language as I have; who speaks of planetary perturbations without having a knowledge of the fundamental law of gravitation; or who pretends to know all about steam-engines without having the least knowledge of the laws of heat, of elasticity, or of dynamics involved in their action.

On the whole I am inclined to agree with Lord Brougham, who, himself a great lawyer and a lover of science, gave origin to the pithy expression, "Try to know something about everything, and everything about something." It would be hard, indeed, to realise the latter portion of his saying, but it would be difficult to know even a good deal about something without knowing at least something about a great many other things.

The question of education becomes even more difficult when we approach the condition of the artisan who needs to send his boy into the mine or factory at the tender age of twelve years. I am of opinion that fourteen years should be the minimum age at which lads should be admitted into works, in order that they may have had not less than four years of judicious training at elementary or Board schools, where in addition to the purely elementary subjects, at least so much of general history, easy mathematics, and natural science should be inculcated as to implant, if possible, the desire to acquire more of those subjects in after life. School education, whether followed up to one point or another, can after all do no more than lay a foundation and implant, if possible, a desire in the mind of the student to follow up the subjects taught in maturer years with the experience of life present to give a practical direction to his studies.

In order to aid him in these endeavours, such bodies as the Midland Institute must prove to be of great service, with its science classes and lectures open to all who thirst after knowledge and who want to understand more particularly the scientific principles involved in their occupations. Technical education such as this is indeed indispensable if this country is to maintain the supremacy won for it by men of exceptional genius, enterprise, and perseverance, but which without it can hardly be expected to withstand in the long run the competition of foreign nations, with cheaper labour and a higher standard of general education in their favour. The English system of technical education has this advantage over the system established elsewhere, that it is not governmental but essentially spontaneous and self-supporting, and will therefore shape itself into the mould best suited to the free and vigorous development of trade itself.

The system of pupillage or apprenticeship will still be necessary, but instead of involving the sacrifice of seven of the most important years of a young man's life, half that time, or say three years, will be found amply sufficient to give to the lad imbued with first principles the practical knowledge necessary for his trade. The employer would be amply compensated for the shorter time of gratuitous service by a corresponding improvement in its quality. He should be expected to see to it that during the term of his authority the pupil attended Saturday and evening classes, where, in addition to general subjects, the principles underlying the operations of his business of spinning, dyeing, paper-making, or metal-working are taught by competent persons.

It is important that the teacher himself should not be a mere specialist, but a man capable of generalising and of calling to his aid other branches of science and general knowledge, that he should be, in short, a well-educated person. It is difficult, I believe, as yet to find a sufficient number of teachers equal to such a standard, and in order to supply this deficiency normal schools will have to be established upon a much larger scale than has hitherto been the case. It is satisfactory to learn that South Kensington is coming to the rescue in converting its science teaching into a normal school for the education of science teachers; only it is to be hoped that literary subjects will be added to their curriculum.

The importance of a higher education of the working classes will be appreciated by all who have watched the rapid strides with which one branch of industry after another undergoes fundamental change, by which the mere craft-skill acquired yesterday becomes obsolete to-day, when a new process, involving entirely new modes of operation, takes the place of a previous one. Nor is there any promise of stability in the process of to-day, which may be again superseded to-morrow by something more nearly approaching ultimate perfection.

To those who still have some confidence in the stability of things as they exist in arts and manufactures, I would strongly recommend a trip to Paris, where they will still be in time to visit the International Exhibition of Electricity. That form of energy known as the electric current was nothing more than the philosopher's delight forty years ago. Its first practical application may be traced to this good town of Birmingham, where Mr. George Elkington, utilising the discoveries of Davy, Faraday, and Jacobi, had established a practical process of electroplating in 1842.

It affords me great satisfaction to be able to state that I had something to do with that first practical application of electricity; for in March of the following year, 1843, I presented myself before Mr. Elkington with an improvement on his processes, which he adopted, and in so doing gave me my first start in practical life. Considering the moral lesson involved, it may interest you, perhaps, if I divert for a few minutes from my subject in order to relate a personal incident connected with this my first appearance amongst you.

When the electrotype process first became known, it excited a very general interest, and although I was only a young student of Göttingen under twenty years of age, who had just entered upon his practical career with a mechanical engineer, I joined my brother Werner Siemens, then a young lieutenant of artillery in the Prussian service, in his endeavours to accomplish electro-gilding, the first impulse in this direction having been given by Prof. C. Himly, then of Göttingen. After attaining some promising results, a spirit of enterprise came over me so strong that I tore myself away from the narrow circumstances surrounding me, and landed at the East End of London with only

a few pounds in my pocket and without friends, but with an ardent confidence of ultimate success within my breast.

I expected to find some office in which inventions were examined into, and rewarded if found meritorious, but no one could direct me to such a place. In walking along Finsbury Pavement I saw written up in large letters "So and so" (I forget the name), "Undertaker," and the thought struck me that this must be the place I was in quest of; at any rate, I thought that a person advertising himself as an "undertaker" would not refuse to look into my invention with a view of obtaining for me the sought-for recognition or reward. On entering the place I soon convinced myself, however, that I came decidedly too soon for the kind of enterprise here contemplated, and finding myself confronted with the proprietor of the establishment, I covered my retreat by what he must have thought a very lame excuse. By dint of perseverance I found my way to the patent office of Messrs. Poole and Carpmael, who received me kindly and provided me with a letter of introduction to Mr. Elkington. Armed with this letter, I proceeded to Birmingham to plead my cause before your townsman.

In thinking back to that time, I wonder at the patience with which Mr. Elkington listened to what I had to say, being very young, and scarcely able to find English words to convey my meaning. After showing me what he was doing already in the way of electro-plating, Mr. Elkington sent me back to London in order to read some patents of his own, asking me to return if, after perusal, I still thought I could teach him anything. To my great disappointment I found that the chemical solutions I had been using were actually mentioned in one of his patents, although in a manner that would hardly have sufficed to enable a third person to obtain practical results.

On my return to Birmingham I frankly stated what I had found, and with this frankness I evidently gained the favour of another townsman of yours, Mr. Josiah Mason, who had just joined Mr. Elkington in business, and whose name as Sir Josiah Mason will ever be remembered for his munificent endowment of education. It was agreed that I should not be judged by the novelty of my invention, but by the results which I promised, namely, of being able to deposit with a smooth surface 3 dwt. of silver upon a dish-cover, the crystalline structure of the deposit having theretofore been a source of difficulty. In this I succeeded, and I was able to return to my native country and my mechanical engineering a comparative Crœsus.

But I was not to remain there, for in the following year I again landed in the Thames with another invention, worked out also with my brother, the Chronometric Governor, which, though less successful, commercially speaking, than the first, obtained for me the advantage of bringing me into contact with the engineering world, and of fixing me permanently in this country. This invention was in course of time applied by Sir George Airy, the then Astronomer-Royal, for regulating the motion of his great transit and touch recording instrument at the Royal Observatory, where it still continues to be employed.

Another early subject of mine, the anastatic printing process, found favour with Faraday, "the great and the good," who made it the subject of a Friday evening lecture at the Royal Institution. These two circumstances combined obtained for me an entry into scientific circles, and helped to sustain me in difficulty until, by dint of a certain determination to win, I was able to advance step by step up to this place of honour situated within a gunshot of the scene of my earliest success in life, but separated from it by the time of a generation. But notwithstanding the lapse of time, my heart still beats quick each time I come back to the scene of this, the determining incident of my life.

At the time I am speaking of, the electric telegraph was occupying the minds of the philosophers of different countries, but it was not until the year 1846 that the first practical line of telegraph was established between Paddington and Slough, where it soon gained notoriety in preventing the escape from justice of a great criminal. It is unnecessary for me to insist upon the enormous results that have been achieved by this great modern innovation, which goes even beyond the poetic vision of Shakespeare himself, who in the extravagance of his "Midsummer Night's Dream" makes Puck "encircle the earth in forty minutes," a rate of communication which would nowadays hardly satisfy the City merchants, who expect Calcutta and New York to respond to their calls much more promptly than that.

The telegraph has found its simplest but most remarkable development in the telephone, which, although shadowed forth by Ries in 1862, was only reduced to anything like a practical shape by Graham Bell in 1876, and subsequently extended by Edison, Hughes, and others.

This latter invention appeared at first particularly unpromising of practical results. The currents set up through the vibrations of a metallic diaphragm facing the poles of a small magnet are so feeble, and the rate of succession of currents necessary to produce sound (represented by 440 vibrations per second to produce the note fundamental *la*) was so very much beyond anything met with in telegraphy, that it was difficult to conceive how such a succession of distinct currents with the infinite variety of strength and quality necessary to reproduce speech could be transmitted through a line wire many miles in length, and could reproduce mechanically the same sounds at the receiving end. Yet the telephone has become a practical reality, and its ultimate powers are illustrated in a very remarkable manner at the Paris Exhibition.

There, in a certain room, you may listen of an evening one minute to the performance going on at the Great Opera House, the next minute to an air sung at the Opéra Comique, and again the next minute to the well-known voices of the principal actors of the Théâtre Français. The novelty of this particular arrangement consists in having each receiving telephone connected separately to a transmitting telephone, fixed in front of the footlights towards the two sides of the stage, whereby an acoustic effect is produced that may almost be called stereoscopic; you actually hear when the actor turns his or her head from one side to the other, and are able to separate most distinctly the several voices, as well as the orchestral instruments when concerted music is being produced. Nor are the sounds in any way distorted or disagreeable, or too low to be enjoyable, but loud and full, producing an agreeable impression even on the musical ear. The person with his ears to the two receiving telephones imagines himself in a mysterious dreamland of sound, but remove the instruments only half an inch from the ear, and all has departed; no sweet sounds of music are heard, but in their stead the speaking voice of the person anxious to take your place at the auditory. I leave to your imagination to picture the innumerable applications which this new power of man in directing the forces of nature may ultimately lead to.

The most striking feature upon entering the Paris Exhibition in the evening is the blaze of electric light that makes the interior of that large building even brighter than by daylight; nor is the effect of this illumination marred by the flickering, fizzing, and colour changing of the earlier attempts in this direction. The character of the lights comprises a range from the central arc of 10,000 candle-power, to the incandescent lamp of only fifteen candles, equalling the light only of an ordinary gas-burner, and the grouping and shading of some of these lights are such as to produce effects extremely agreeable to the eye. Who would venture to say, after this display, and after the practical applications that have been made of the electric light in the City of London, at several of our docks and harbours, at works, halls, and theatres, that it is not a practical illuminant destined to work as great a change as gas-lighting did before it, thirty years ago, when it was inaugurated at the Soho Works not many miles away from this hall?

But although I predict a great future for electric light as being the most brilliant, the cheapest, and the least objectionable from a sanitary point of view of all illuminants, I do not agree with those who consider that the days of gas must therefore be at an end.

In addressing the British Association of Gas Managers in this town a few months ago, I called attention to certain means by which gas of much higher illuminating power might be obtained from the ordinary retorts, if only, at the same time, the gas companies or corporations could be induced to supply at a reduced rate heating gas, of which we so much stand in need; and how, by certain improvements in the burners themselves, the illuminating power of a given quantity of gas might be still further augmented. Gas companies have for many years enjoyed the sweets of their monopoly position, which position is generally speaking not productive of desire for change. The electric light has furnished for them the incentive to advance, and the effect of that incentive has told already, I am glad to observe, in a very striking manner upon the street illumination of this immediate neighbourhood.

The time is not far distant, I believe, when gaseous fuel will almost entirely take the place of solid fuel for heating, for obtaining motive power, and for the domestic grate; and if gas

companies and corporations rightly understand their mission, they will take timely steps to supply, separately, heating gas at a greatly reduced cost, the demand for which would soon be tenfold the gas consumption of the present day. The economy and the comfort which would accrue to the inhabitants of large towns by such a change would be great indeed, and it would, amongst other things, effect a radical cure of that great bugbear of our winter existence, a smoky atmosphere.

The third great practical illustration furnished by the Paris Exhibition has reference to the transmission of power from one place to another by means of the electric conductor. When, only five years ago, in addressing the Iron and Steel Institute, I ventured upon the assertion that the time was not distant when the great natural sources of power, such as waterfalls, would be transferred to considerable distances by means of stout electric conductors, to be there utilised for providing towns with light and motive power, I elicited an incredulous smile even from some of those most conversant with the laws of electricity. Electricity had been looked upon by them as a swift agent to flash our thoughts from country to country, but the means of producing that form of energy by the expenditure of power on the dynamo-electric machine, although known, was not yet properly appreciated. Such can hardly now be considered the case. I could point to at least three instances in this country where power is practically transmitted to a distance by means of electricity, to be utilised for pumping water, for lighting, and for working machinery, and the Paris Exhibition furnishes additional illustration of the facility with which that transmission may be effected.

The electric railway leading from the Place de la Concorde into the Exhibition, and only half a kilometre in length, does its work regularly and well, running a trip every five minutes, and conveying generally as many passengers as can be packed both inside and outside of a tram-car of ordinary dimensions. This system of propulsion will soon be in operation on a new line of railway six miles long, with which I am connected, in the north of Ireland, to be extended, if successful, to a further equal distance. This will give us twelve miles of electric railway worked without expenditure of fuel, for the motive power will be obtained from a neighbouring waterfall, which at present runs to waste. Mr. W. A. Traill, the Resident Engineer of the line, has already commenced operations, and I hope that by next spring, visitors to the sister island may reach one of its most interesting sights, the Giant's Causeway, propelled by invisible but yet potential agency.

The experience gained by my brother in the working of the first electric railway, two miles in length, established by him at Lichtenfelde, near Berlin, leaves no reasonable doubt regarding the economy and certainty of this mode of propulsion, although it is not anticipated that it will supersede locomotive power upon our main trunk railways. It will have plenty of scope in relieving the toiling horses on our tramways, in use on elevated railways in populous districts, and in such cases as the Metropolitan Railway, where the emission of the products of combustion causes not only the propulsion but the suffocation of passengers.

Another application of electricity, also at any rate indicated at the Paris Exhibition, is that to agriculture and horticulture, upon which I have been practically engaged during the last two winters on my farm near Tunbridge Wells. This is neither the time nor place for me to enlarge upon this application, which should be mentioned, however, because I believe that it will ultimately exercise a considerable influence upon an important interest, besides providing a means of adding to the pleasures of country pursuits. Electroculture by itself would be expensive, but not so if combined, as it is at Sherwood, with the utilisation of electric energy for accomplishing other objects—such as chaff- and root-cutting at one place, wood-cutting at another, and pumping of water at a third, while the waste heat of the steam at the generating station is utilised to heat the water circulating through the greenhouses, &c. In this way labour and expense are saved in many ways, and the men employed on the farm find no difficulty in working the electrical horses, no longer experimentally, but as a regularly established thing.

A somewhat special application of electricity, also shown at the Paris Exhibition, is its employment as a heating agent. For temperatures not exceeding that of a welding furnace, solid or gaseous fuel produces the desired effect at a cheaper rate than it is likely to be accomplished by electricity. When electricity is used, heat energy has in the first place to be transferred from the burning fuel to the boiler of the steam-engine. The mechanical energy of the engine works the dynamo-electric machine, whence electric energy is transmitted through the conductor to the point where it is to be utilised as heat. At each intermediate stage a loss will have to be incurred, and it is therefore absolutely certain that the amount of heat finally produced in the electric arc must fall very much short of that generated by the fuel under the boiler. But the electric arc has this advantage over other sources of heat, that no waste heat need pass away from it in the shape of heated products of combustion. This loss of heat in the furnace by combustion increases with the temperature at which the work has to be accomplished, and reaches its maximum in a furnace for melting steel or platinum. Beyond this the point is soon reached where combustion ceases entirely, where, to use the scientific phrase, the point of dissociation of carbonic acid is reached; and it is for purposes where such degrees of heat are required that the electric arc can be advantageously employed, and will enable us to accomplish chemical effects which have hitherto been beyond the reach of science.

My chief object in dwelling, perhaps unduly, upon these practical questions is to present to your minds in a concrete form the hopelessness of looking upon any of the practical processes of the present day as permanent, to be acquired in youth and to be the staple occupation of a lifetime.

The respectable millwright of former years had already to enlarge his scope of knowledge and become a steam-engine builder; having made himself master of the construction of simple forms of high-pressure engines, he has had to go to school again, to study the laws of condensation and of the expansive action of steam, in order to produce an engine using only a fractional amount of the fuel which his customers were willing to expend in former years for a given effect; he now has to study the laws of electricity and understand the construction of dynamo-electric machines, in order to be able to transmit and distribute his steam power more readily than could be accomplished by means of wheels and belts. But even his condensing steam-engine with variable expansion, of which he is so justly proud to-day, will no longer be acceptable to his client to-morrow, when it will be made clear to him, by the light of thermo-dynamics, that even the best of steam-engines utilises barely a seventh part of the heat-energy residing in fuel, and that the attainment of perhaps three-fourths of that ultimate limit will be required of him.

Analogous changes threaten to invade almost every existing branch of industry, and it is necessary for every one of you to be prepared for such changes.

The practical man of former days will have to yield his place to the unbiased worker who with open mind is prepared for every forward step as it arises. For this purpose it is necessary that he should possess, beyond the mere practical knowledge of his trade, a clear appreciation of the principles of action underlying each operation, and such general acquaintance with the laws of chemistry and physical science as will make it easy for him to adapt himself to the new order of things.

In order to be so prepared, it is by no means necessary that you should have had the advantage of an elaborate school education. No man or woman should consider him or herself out of school until approaching the final reckoning, and it is through advantages such as are offered by the Midland Institute, that the means are afforded you of continuing the educational process near your homes, and without much expense or difficulty of any kind.

Let no one of you suppose that his early training or natural ability is unequal to the task of making a career in life. Goethe, that man of wonderful insight into the working of the human mind, says:—

> "Was man sich in der Jugend wünscht,
> Hat man im Alter in Fülle."

Or, translated,

> "What you desire in youth,
> Mature age will give you in abundance."

At first sight this expression seems to involve almost an absurdity, and it is necessary to interpret the "desire" of youth to mean not simply a vague sentiment or wish to be looked up to in after life, or to drive about in easy carriages, but a determination to leave no stone unturned, and let no opportunity go past that may advance you towards the well-defined object of your ambition. With a firm resolution almost every difficulty in your way will recede before you; disappointments you will

have, and they are most desirable, because they are the real teachers in practical life, only you must not allow yourself to be discouraged, but rather to be strengthened by them, in your determination to succeed.

A fond mother has sometimes come to me with a doleful story that her son, "an excellent young man," had tried several things in life and had always failed, through some untoward circumstance, but that she felt sure he would succeed if I would only give him a trial in my own particular pursuits. On some occasions I have perhaps yielded to such representations, but found that the "excellent young man," though commencing with a certain vigour, soon tired of the new occupation when he approached its difficulties. He could not realise the fact that the secret of success lies not in the avoidance of, but in the victory over difficulties, that each disappointment teaches an important lesson, and that by taking these lessons to heart without swerving from his purpose he would soon find himself possessed of a power exceeding his most sanguine expectations.

Success in life depends in fact much more upon diligence and steadiness of purpose than upon the more brilliant qualities possessed by an individual; but in order to give force and direction to the sterling qualities within him, it is most important that means should be brought within his reach of enriching his stock of useful information. The Birmingham and Midland Institute, counting its 2688 students of various degrees and of both sexes, has accomplished this important object in a manner never before dreamt of; but not content with this splendid result, the Council has made provision for a further extension of its beneficial action through the erection of this magnificent lecture hall, which it is my proud privilege to inaugurate this evening, for the use of our members.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—One interesting outcome of recent changes is the promulgation by the Governing Body of Caius College of the following scheme, to take the place of the regulations providing for the annual delivery of the Thruston speech on the progress of medicine from the time of Dr. Caius, by a medical graduate, who received the sum of 18*l.*:—The money—about 54*l.*—shall be given triennially to that member of the College who has published in the course of the preceding three years the best original investigation in physiology (including physiological chemistry), pathology, or practical medicine; the person to whom the prize is awarded being required to give an account of his investigation in the form of a lecture in the College. If within the specified period no investigation of sufficient merit shall have been made, the money shall be carried forward to augment future prizes; the first prize will be awarded in 1884.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, October 5.—H. T. Stainton, F.R.S., president, in the chair.—Exhibitions: Mr. R. McLachlan, a specimen of *Gastrophysa raphani*, Fabr., bred from a parthenogenetic ovum.—Mr. T. Wood, an abnormal specimen of *Notiophilus biguttatus*, Fabr.—Mr. R. Meldola, on behalf of Mr. W. J. Argent, some interesting varieties of British *Lepidoptera*.—Mr. H. B. Pim, a specimen of *Harpalus discoideus*, Fabr.—Mr. E. A. Fitch, *Lasius mixtus*, Nyl., an ant new to Britain.—Mr. A. S. Olliff, a specimen of *Papilio Americus*, Koll., with abnormal neuration.—Communications: the Secretary read a letter respecting the ravages of *Lopaphus cocophages*, Newp., destructive to cocoa-nut trees in Fiji; and some further communications from the Colonial Office relative to locusts in Cyprus, &c.—Papers read: Mr. D. Sharp, Descriptions of some new *Coleoptera* from the Hawaiian Islands.—Mr. C. O. Waterhouse, on some new South American *Coleoptera* of the family *Rutelidæ*.—Prof. Westwood, description of the immature state of a Cevlonese insect apparently belonging to an undescribed genus.—Mr. P. Cameron, notes on *Hymenoptera*, with descriptions of new species.

PARIS

Academy of Sciences, October 10.—M. Wurtz in the chair.—The following papers were read:—On the first volume of the "Nouvelles Annales de l'Observatoire de Bruxelles," by M. Faye. It contains a new uranometry, and a repertory of constants of astronomy. M. Houzeau has represented the Milky Way on a large scale by means of curves of equal luminous intensity. He distinguishes thirty-three luminous masses, carefully determining their position. Our solar world is situated almost exactly in the plane of the great celestial circle these nearly form, and is probably near its centre. The "Catalogue des Constantes" comprises seventy six determinations of the solar parallax, extending over twenty-one centuries. The increasing precision of astronomical measurements is well brought out.—M. Daubrée presented a large specimen of a holosideric meteorite from Cohahuila, Mexico. It contains chrome-iron, a mineral not before met with in a metallic meteorite. Prof. Laurence Smith also found in it another chromiferous mineral, *Daubrélite*.—On the employment of tar as a preservative against phylloxera, by M. Avignon. A mixture is made of tar and fine sand, and triturated to render it homogeneous. Wood-ash is added; the mixture is put in a hole round the stem in spring and covered with earth. It effectually repels the insect.—A letter of M. Govi relating to a *brochure* by Prince Boncompagni on the unpublished will of Nicolò Tartaglia, noted the fact that the true surname of this celebrated mathematician of Brescia was Fontana. He was called Tartaglia (which means a *stammerer*, and which appears as his name, even in the will) because of difficult articulation arising from a bad wound in his jaw and palate received when he was a boy, during the sack of Brescia in 1512.—Comet discovered by Mr. Denning on October 4, 1881; observation at Marseilles Observatory, by M. Coggia.—On the part of M. Arnaud, a sample of a new alkaloid from quinquina (of Santander, Columbia) was presented; M. Arnaud calls it *cinchonamine*. It differs from cinchonine by an excess of two atoms of hydrogen, and presents the composition of hydrocinchonine, with which it is probably isomeric.—On the sounds produced in a telephonic circuit during thunderstorms, by M. de Lalagade. He recalls effects similar to those got by M. Thury, which he described in 1878. To amplify the sounds he afterwards added two small microphones to the plate of the receiving telephone; the least sounds can thus be heard 1 m. or more from the second telephone in a quiet room.—Galvanometer with angular deflections proportional to the intensities, by M. Gaiffe. The multiplier frame in the instrument presented (a horizontal galvanometer) was of elliptic form. The deflections are regular under two angles of about 35°, representing 35 milliwebers, on either side of zero, and then diminish slowly, allowing of division of the scale by units to the fiftieth milliweber. With a different curve of the multiplier frame the deflections may be rendered proportional up to about the seventy-fifth degree.—On the innervation of the heart and the action of poisons in lamellibranchiate mollusca, by M. Yung. *Inter alia*, the heart is chiefly innervated by fibres from the posterior or the branchial ganglions, which fibres have an accelerative *rôle*. Rise of temperature accelerates the heart's movements up to 40° C. *Curare*, in strong dose, makes the animal's movements very slow. *Strychnine*, whatever the dose, only causes temporary convulsions, never tetanus; in direct contact with the heart it lessens the number of beats, and causes stoppage in fifteen to thirty minutes. *Nicotine* accelerates the heart-beats, enlarges the heart, and in strong dose causes death. *Veratrine* acts similarly, &c.

CONTENTS